-----TO THE EDITOR-----

We would like to thank the anonymous reviewer for their work to improve our manuscript. Please find our responses immediately following each comment in blue text.

-----START REVIEW-----

The Cryosphere Discuss., 7, C2628–C2632, 2013 www.the-cryosphere-discuss.net/7/C2628/2013/

Interactive comment on "Morphology and distribution of liquid inclusions in young sea ice as imaged by magnetic resonance" by R. J. Galley et al.

Anonymous Referee #1

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——————– General comment ——————–

This paper documents an experiment where an ice core stored at -20C was imaged using magnetic resonance. The merit of the paper is to show that MR gives nice and precise images of the brine microstructure. This is an interesting result, but regarding writing and analysis, the paper is a bit light. For making the paper suitable for publication, a few items should be addressed.

1-The writing of the paper could be more concise and precise. The objective of the study could be clearer and should be narrower. The introduction goes in many directions. Instead it should be more focused towards the goal of the paper. Besides, it is not clear what the paper brings compared to previous studies (e.g. Perovich et al., 1996; Eicken et al. 2000).

Agreed. We will present a revised manuscript written as succinctly and precisely as possible. In light of the comments of both anonymous reviewers, we must more clearly define the goal of this work so the reader has the appropriate context for our presented results. That is, (1) to present a three-dimensional image of a brine drainage channel feature in young sea ice, (2) give the physical context for its formation by presenting the physical conditions of the atmosphere and water/sea ice volume (the sea ice volumes' life history) from prior to sea ice growth through the date of the sample discussed, (3) give observation of it's physical properties/characteristics and (4) the amount of time in which it formed. Finally, we (5) present a vertical brine volume distribution profile of our sea ice sample using magnetic resonance image data to extend the (non-imaging) nuclear magnetic resonance work of Callaghan et al. (1999) and Hunter et al. (2009) who first showed that their technique well-replicated calculated brine volume fractions in sections of Antarctic sea ice up to 220cm thick and 180cm thick respectively, but were unable to determine pore sizes, shapes or distributions within the sea ice volume. Mercier et al. (2005) show isotropy and anisotropy in sea ice diffusion coefficients using the NMR method, highlighting the need for a greater library of physical observations of liquid inclusions in sea ice under a variety of sea ice types/environments. We will include this information and citations in the revised manuscript.

The works mentioned above by the anonymous referee also indicate a dearth of knowledge on the structure of sea ice and its liquid inclusions. For example, Callaghan et al. (1999) cites only Niedrauer and Martin (1979) to illuminate the geometry of brine inclusions in congelation sea ice (which was grown in a laboratory), while Hunter et al. (2009) notes that the growth history of the ice prior to their measurements is likely important to anomalies in their brine volume data.

Additional citations we will include in the revised manuscript with pertinent discussion:
Callaghan, P. T., R. Dykstra, C. D. Eccles, T. G. Haskell and J. D. Seymour, 1999. A nuclear magnetic resonance study of Antarctic sea ice brine diffusivity, Cold Regions Science and Technology, 29: 153 – 171.

Hunter, M. W., R. Dykstra, M. H. Lim, T. G. Haskell, and P. T. Callaghan, 2009. Using Earth's field NMR to study brine content in Antarctic sea ice: Comparison with salinity and temperature estimates. Applied Magnetic Resonance, 36:1 – 8, doi: 10.1007/s00723-009-0003-9

Mercier, O. R. M W. Hunter, and P. T. Callaghan, 2005. Brine diffusion in first-year sea ice measured by Earth's field PGSE-NMR. Cold Regions Science and Technology, 42:96 – 105, doi: 10.1016/j.coldregions.2004.12.004.

The introduction is organized chronologically with attention paid to previous publications that have quantified liquid inclusions in different sea ice volumes, including Perovich and Gow, (1996) (we presume this is the article to which the reviewer refers above) and Eicken et al. (2000). We see our work as a contribution to the aforementioned and relatively sparse library of brine inclusion geometry in young sea ice, using MR imaging as a method.

Perovich and Gow (1996) present horizontal thin sections sea ice and cumulative brine pocket distributions using optical photography of 20-cm thick young sea ice at 12 and 19-cm depths (among other ice types). Their work should elucidate how difficult it would be to reconstruct the size and shape and distribution of brine inclusions using this method over an entire young sea ice (<20cm) core, never mind for thicker sea ice stages of development. Eicken et al. (2000) demonstrated close correspondence of MRI data and thin-section analyses and showed the thermal evolution of brine inclusions in sea ice, imaging them as they changed as their temperature was brought up from their initial storage temperature (-25C). We view our work as complementary to that of Eicken et al. (2000) in that we were able to present a three-dimensional reconstruction of the morphology and vertical and horizontal distribution of liquid inclusions in sea ice using a comparatively fast but still high resolution MR imaging parameters suggested by the authors to image the entire thickness of a sea ice volume at once.

2-At first order, brine volume fraction depends on 1/T. Hence, cooling the whole core at -20C will not "freeze" the microstructure, but will largely modify it. Therefore any conclusion on the sea ice desalination based on such an experimental protocol is biased. This should be made clear and discussed.

We agree that the storage of sea ice at temperatures other than those of in situ conditions could theoretically modify physical and chemical parameters. However, in the absence of any current method that preserves the natural temperature gradient within the sea ice volume immediately and without change upon the samples' extraction from the larger sea ice volume, ex situ analysis of sea ice samples after storage at low temperatures is an established protocol. We have chosen to remain consistent with other published studies of this nature. For example, Eicken et al. (2000) and Bock and Eicken (2005) based their conclusions regarding sea ice microstructural evolution with temperature on the premise that core samples stored at low (<-20C) temperatures then subsequently warmed as they were imaged behave similarly to sea ice in the natural system. Analysis of sea ice samples after them having been stored at low temperature is long-standing, standard protocol in the absence of method that preserves the natural temperature gradients within the sea ice volume immediately and without change upon the samples' extraction from the larger sea ice volume. We acknowledge that our initial manuscript did not precisely present and discuss the current shortcomings of core sample storage and will address this issue in the revised manuscript. We note that we have avoided making conclusions about sea ice desalination and rather have discussed the potential for brine motion in the sea ice volume grown here.

Furthermore, the temperature of the ice core during measurement is unknown, which renders the comparison with Cox & Weeks formula difficult. You should inform that the comparison is made just to check that your measured brine volume fractions make sense.

The temperature of the ice core as stored was -20C and we assume that it warmed from that point during the four and half minute scan. We could perform a simple test with a stored sample by taking it out of storage just as the imaged sample and then measuring the resultant temperature change. However, just as in the field, the outside of the core will not be the same temperature as the inside of the core where the temperature is measured, so we are not convinced that the additional data would add anything to the current manuscript. We expect that the core will not have changed appreciably from the storage temperature of 20C.

In this context, the only conclusion I retain as valid is that imaging sea ice microstructure with MR is possible; but other conclusions (on sea ice desalination and permeability) are hard unless the experimental control of temperature is better handled.

We will change the manuscript to better highlight the fact that we were much less concerned with developing magnetic resonance imaging as a method and much more concerned with making observations of and quantifying the liquid inclusions in a young sea ice sample that we know the entire 'growth history' of. Important conclusions of our manuscript which we argue should be retained are that a first-generation brine drainage channel formed in young sea ice near-simultaneously as it grew downwards; the brine drainage channel imaged and presented here formed in as little as six days as the sea ice grew thicker. This first generation brine drainage feature penetrated 43% of the vertical thickness of the young sea ice sample we obtained (80mm of 185mm total thickness). Further we refer the reviewer to paragraph 2 of section 5 on page 4994 for further observational characterization of the imaged first-generation brine drainage channel.

To pick up from our response to the previous comment, we have made no conclusions on sea ice microstructure or desalination. We have purposefully left these points in the discussion section of the manuscript because they are in some respects conjecture based on the current literature on those subjects in the context of our results.

We appreciate that the temperature control situation was not perfect, but have outlined our shortcomings with regard to that situation within the manuscript. If it is deemed necessary we could remove the section of the manuscript detailing the vertical profile of liquid in the sea core imaged using magnetic resonance due to this lack of control, but we argue that it is (at worst) a qualitative affirmation of the NMR method used by others (Callaghan et al. (1999) and Hunter et al. (2009), for example) to relate the vertical distribution of brine in sea ice and should be left in the manuscript.

3-The contents are quite light. More quantitative analyses of the produced images would be worthwile. The uncertainties on the method, especially the alleviated linearity of the relationship between brine volume fraction and the DN should be discussed.

We are not sure what the anonymous referee means by alleviated linearity, but we defer to the work of Callaghan et al. (1999) who made a regression analysis of NMR-derived brine volume (V_{NMR}) and brine volume calculated using the equations of Cox and Weeks (1983) (V_{calc}) yielding an equation of best fit of $V_{NMR} = -0.0035 + 1.12V_{calc}$ with a correlation coefficient of 0.952. We can add this information to the manuscript.

In conclusion, I recommend to give a precise scope to the paper, which I think is feasible, and rewrite each of the sections accordingly.

———————— More specific comments ————————

*** Abstract

-Should explain that this is a study of feasibility, but that the experimental control of temperature was weak.

Our manuscript deals primarily with illustrating the formation time and structure of a brine drainage channel in young sea ice; this is not a study of feasibility, though we do show that imaging a brine drainage channel in three-dimensions with high resolution with a very short 4.5-min scanning time is possible. We will change the title of the paper to "Observations of the morphology and distribution of liquid inclusions in young sea ice as imaged by magnetic resonance" to clarify that this work is meant as an observation of the physical characteristics of a first-generation brine drainage feature, the time it took to form while the ice grew, and the physical characteristics of the atmospheric and water/sea ice environment during which the sea ice and the feature formed.

-Contains a few redundancies (e.g. sentence line 20-31)
-line 33, use brine inclusions instead of permeability (which cannot be counted and located)
-line 34-37, this sentence could be more compact

Acknowledged. We can remove redundancies and state these sentences more concisely.
We will make sure to use "brine inclusions" instead of "permeability" (on line 10 in our copy of the discussion paper named *tc-2013-140-discussions-typeset_manuscript-version2.pdf*).

*** Introduction
The introduction clouds the issue.
-There are too much details on some aspects (importance of brine microstructure, location of the various experiments of previous authors). There is an almost chronological list of previous work. I would have expected a more synthetic presentation of the state-of-the-art for the different techniques to visualize sea ice microstructure. I am not sure that the sequential (historical) approach to describe previous work is ideal. I would divide previous work into 1) the techniques used to map sea ice microstructure; 2) the morphology of brine channels. Then a last paragraph describing how the technique used by the authors compares to this previous work.
-Second and third paragraphs are built from very old references, while more recent work contributed a lot.

Again, it was not our intention to write a paper on the method of magnetic resonance imaging, but rather to present a quantification of the size, shape and location of brine channel inclusions in young sea ice along with the atmospheric and water/sea ice volume physical forcing and characteristics that created them. It was our goal to show that are relatively few observational studies that quantify the morphology and distribution of liquid inclusions in sea ice. The 'library' of brine inclusion geometry is sparser than one would initially expect given how old some of the work is on the subject. We think that the chronological approach we have taken is appropriate to highlight the previous observational work on the subject.

e.g., Line 76-78. This statement is not in line with recent research summarized by Notz and Worster (JGR 2009). Ice forms at the salinity of the seawater. Brine drains simultaneously. It is likely that this old result -that the apparent S is a function of growth rate -is because brine convection removes salt more efficiently when the growth rate is slow.

We think that the reviewer is referring to page 4980, lines 25 and 26. The line numbers to which the reviewer refers in this review are seemingly gleaned from an earlier draft of the paper not published in The Cryosphere Discussions. We have made an effort to match the line numbers identified in these comments, with those of the manuscript published in The Cryosphere Discussion. The conclusions of Notz and Worster (2009) indicate that the high bulk salinity formed at high growth rates arise from limited convection (especially in thin ice). So, we propose to change the sentence noted by the referee to indicate these ideas.

Notz, D. and M. G. Worster, 2009. Desalination processes of sea ice revisited, Journal of Geophysical Research, 114, C05006, doi: 10.1029/2008JC004885.

-Line 81-82 -It would be nice to give more information on how Cox and Weeks 83 measured brine volume fraction to construct their relationship

We think the anonymous referee is referring to page 4980 line 29 and page 4981 lines 1 and 2. The Cox and Weeks (1983) equations for brine volume in sea ice samples have been the enduring standard for many years. We are not convinced it benefits the reader to include more detail from their study.

-Line 110: "fracture of air and brine pockets" is not clear

Page 4981 Lines 27 and 28. We could re-write the sentence as follows: "Eide and Martin (1975) speculated that these feeder channels appear to form suddenly as a result of the sudden fracture of air and brine pockets connecting them into a channel shape".

-Lines 148-156: This is the core of your paper, the rest should be based on it. However, I would think that addressing all these objectives is not feasible in the context of your experiment. Testing the law of fives with a core stored at -20 is not possible, unless the core is progressively warmed, which was not done. Similarly, investigating brine channel formation is hard, as the storage must have largely modified the microstructure of the ice.

Page 4984 lines 6 – 13. We acknowledge the potential effects of core storage on brine drainage features (see also our response to point (2) from this reviewer). To understand the dynamics of brine channel features it is crucial that the physical history of the ice core, as well as that of the atmospheric and water/sea ice volume in which the liquid inclusion formed, and its formation time, are coupled with the discussion of its morphology. This requirement exists regardless of whether operating under natural or experimental conditions. We have adhered to this rationale by presenting a detailed description of the conditions under which the brine inclusion both initially formed, and under which it was imaged. We acknowledge that the experimental conditions are not entirely comparable to those under which the law of fives pertains, but we would be remiss not to include a comparison with the most established and currently used method of brine volume calculation.

*** Section 2
Second paragraph. The error on temperature is likely higher than what you state, just because your core might get colder or warmer before you measure temperature. I think the paper by Zhou et al. in JGR recently showed that the error locally can be up to +/-1-2C. Similarly, for salinity, the error due to brine drainage during coring can be high when brine volume is high. There is work by Eicken et al. 1991 illustrating this. Because of these errors, using CW formula underestimates brine volume.

The accuracy stated on page 4984 line 8 was that of the temperature probe we used was gleaned from documentation on the instrument provided by the manufacturer and was not intended to be a statement of the experimental accuracy of the method of drilling holes in sea ice core samples and measuring their 'internal' temperature. We agree that the manuscript could benefit from discussion regarding the pros and cons of the widely used method for measuring sea ice core temperatures and will include that information along with relevant citations in a revised methods section. The fact that the equations of Cox and Weeks (1983) underestimate sea ice brine volumes is actually beneficial to our discussion, as it means that the theoretical brine volumes of our sample at the storage temperature of -20C are probably closer to the MRI-derived brine volumes we calculated and presented in Figure 6.

The chapter entitled *Thermal, Electrical and Hydraulic Properties of Sea ice* by D. Pringle and M. Ingham in the book Field Techniques for Sea Ice Research edited by H. Eicken, R. Gradinger, M. Salganek, K. Shirasawa, D. Perovich and M. Leppäranta (2009) reveals that a common rule of thumb of making temperature measurements using extracted cores should be done within five minutes. We made our temperature measurements nearly immediately. Pringle and Ingham (2009) also note that the accuracy of these measurements can easily be less than 0.5C and the study of Eicken et al. (2004) achieved an accuracy of 0.2K. We will add this information in the methods section of a revised manuscript.

Citations to be included:
Pringle, D. and M. Ingham, 2009. Thermal, electrical and hydraulic properties of sea ice, In: Field techniques for sea ice research, H. Eicken, R. Gradinger, M. Salganek, K. Shirasawa, D. Perovich and M. Leppäranta (eds.), University of Alaska Press, Fairbanks, AK.

Eicken, H., T. C. Grenfell, D. K. Perovich, J. A. Richter-Menge, and K. Frey, 2004. Hydraulic controls of summer Arctic pack ice albedo, Journal of Geophysical Research, 109, C08007, doi: 10.1029/2003JC001989.

You took three cores. The presentation would be clearer if you labelled them. You should have re-measured S on the stored core since storage changes salinity.

Agreed. The cores will be separately labelled.

Line 225 typo "to quantify"

Page 4985 line 23. We will add the word "to" to the sentence to correct for the typographical error in the manuscript. We thank the reviewer for catching this error.

Line 228-232. Show the histogram of DN (if meaningful) to illustrate this.

Page 4985 Line 25. We are not convinced the image histogram of DN requires presentation in this instance. Here, the statement of the maximum value should suffice.

Line 232. Is there any reason to believe that the relation between Lf and DN could be non-linear. This should be part of the discussion.

Page 4986 Line 2 (Equation 1). Based on the work of Callaghan et al. (1999) using NMR and our work presented here, we assume that the relationship between the liquid fraction and DN is linear. Discussion of any potential non-linearity in this relationship would be purely speculative and contrary to the results we present.

Line 234 typo: remove "were"

Page 4986 line 4. This typographical error is present in an earlier draft of the manuscript, but is not present in the discussion paper published in The Cryosphere Discussions, so its removal is not necessary.

Line 265 "caused the sea ice volume to cool" -> "cooled the ice" (there are many instances where the wording could be more concise).

Page 4987 line 8. Agreed. We will change the sentence as the reviewer has suggested and edit the revised manuscript thoroughly for brevity.

*** Section 3 There could have been more analyses of the images.

Though we appreciate the sentiment, we don't find this comment instructive.

*** Section 4 and 5. Errors and scope of the results are under-discussed.

Lines 422-427: the sentence questioning the sackhole technique is not clear

Page 4992 Line 12-18. Our image results indicate that the sackhole technique for brine collection, where blind core holes are drilled to specific depths in sea ice cover then covered to collect brine which may percolate into the bottom of the hole should be called into question because although the data gleaned is typically presented as having come from the depth to which the hole was drilled and this is not likely an accurate representation of the brine origin. The three-dimensional images of liquid brine inclusions indicate that the brine within the lower 80mm of a 185mm thick sea ice volume may be connected horizontally to varying degrees. So, we will make every attempt to clarify the sentence in this light.

Line 427: just explain how rather than saying that they "fall in line with"

Page 4992 Line 19 and 20. In the lines following (page 4992 lines 21-25), we expand what we meant by "fall in line with…" the literature we cite in line 20. That is, that our observations of the sea ice growth in our work seem to indicate that initially brine is remains in the interstices of ice crystals as the form before being trapped by freezing as the ice front moved downward into the water column, and that we provide observation of the sea ice reaching some critical thickness when brine is able to drain to the seawater below via gravity or convection.

Line 435 "pemeability of the sea ice volume" is ambiguous Line 441 "contained less bulk salinity" -> "contained less salt"

Page 4992 Line 26. We are not convinced that this sentence is ambiguous.
Page 4993 Line 3. We will change the wording here from "contained less bulk salinity" to "was less saline".

Line 443-445: it is also possible that some salt was expelled during the storage of the core
Page 4993 Lines 5 and 6. We will state the possibility that the core sampling method causes underestimation of sea ice bulk salinity due to brine loss during sampling or potentially in even storage.

Line 459-477: why discussing this here ?

Page 4993 Line 20 to page 4994 Line 13. The discussion section seems like an appropriate opportunity to broaden the scope of the results presented. The effect of ikaite precipitation on under-sea ice pCO_2 depends on whether these crystals are retained in the ice matrix or exported with brine drainage. Our imaging results indicate that the attending channels to the main brine drainage channel range from 0.8 to 1.5 mm in diameter, which is greater than the ikaite crystal lengths of <0.1 mm to 1 mm reported by Rysgaard et al. (2012) Arctic sea ice. Our imaging results are thus consistent with recent investigations of ikaite dynamics that suggest ikaite removal from sea ice through brine drainage is a process affecting the alkalinity of under-sea ice seawater and consequent effects on air-atmosphere CO2 exchange. We did not explicitly make this connection in our original manuscript but will now do so.

Line 491-494: the brine channel could have formed before if it moves downwards within the ice as ice grows. 10 days is not a speed, it is a time.

Page 4994 Lines 20-22. We agree that days are a unit of time, not speed, and will change the wording on line 20 to reflect that. We do not think it is appropriate to conclude that the brine channel feature imaged have formed earlier and moved down within the ice as it grew, because though we have ice thickness measurements throughout the experiment to indicate sea ice growth over time, our images are of the entire sea ice volume as it existed at the end of the experiment. Any thoughts on how the brine channel feature formed and moved within the volume in the conclusion section would be speculative.

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Figure 3 -how the brine volume fraction was computed ?

The brine volume was calculated using the equations of Cox and Weeks (1983). This will be added to the figure caption.

Figure 5 -The field that is depicted with the gray scale should be described. Captions for 5a and 5b should be different

Agreed. We will change the figure caption to include that the grey scale depicts MR signal intensity. We will change the figure caption for Figure 5b to more accurately describe the figure.

Interactive comment on The Cryosphere Discuss., 7, 4977, 2013.
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