Final Authors Comments

"Technical Note: On the use of the mushy-layer Rayleigh number for the interpretation of sea-ice-core data"

by M. Vancoppenolle et al.

We warmly thank the reviewers and the editor. We highly value the care and time they have taken to read and comment on the manuscript.

Let us first attempt to summarize the reviewers' general impressions. Reviewer 1 thinks the paper is worth publication as a technical note and suggests a few minor modifications. Reviewer 2 is generally positive but has more remarks, which are all easily addressable. Reviewer 3 believes the theoretical background is (too?) light, but thinks the contribution is valuable nonetheless, and has a number of suggestions as to how to improve the presentation of the paper.

Based on these assessments, we feel that this unpretentious technical note could be useful to the community and is worth revising.

Hence, in the text below, we reply to the reviewers' comments and describe how we will address those in a revised manuscript.

M. Vancoppenolle, D. Notz, F. Vivier, J. Tison, B. Delille, G. Carnat, J. Zhou, F. Jardon, P. Griewank, A. Lourenço and T. Haskell

Reviewer 1

General comment. *This paper contains some intrinsic matters although it is a rather simple investigation, and I think it is worth publishing as a technical note.*

Remark 1. *It seems to be intriguing that Rayleigh number shows diurnal changes after early spring (T>-5C), which means brine convection may have such variation as well.*
Answer: This is actually quite easily explicable. The amplitude of the temperature diurnal cycle in this type of snow-free ice reaches ~5°C near the surface in mid-november, driving Π and Δρ fluctuations. Ra follows temperature since the effect of permeability dominates the value of Ra for temperatures between -5 and -2°C (as discussed in Jardon et al, 2013). We will explain this in more details in the revised version.

Remark 2. *For publication the authors should discuss more quantitatively how much the activity of brine convection is affected by the possible error caused by physical parameters and how it affects the profiles of salinity, brine volume fraction, and so on. It is important because at the early stage it may determine the subsequent evolution of salinity profile within sea ice and in spring it may determine the timing of major desalination. I recommend the authors to examine this quantitatively, if possible.*
Answer: We will provide more discussion as to the practical implications of our findings for the resulting salinity profiles.

Remark 3 *... an underestimation of the bulk salinity ranging up to 20 g/kg compared to non-destructive measurements" I am wondering if 20 g/kg might be too much because in most cases bulk salinity of sea ice is less than 12 g/kg...*
Answer: We will clarify that with this statement we only refer to salinity near the ice—ocean interface. There, real bulk salinity is close to that of sea water (i.e., more than 30 g/kg), but is often measured to be less than 10 g/kg because of brine loss during sampling. We will be more cautious regarding the error magnitude, which needs more data to be precisely evaluated.

Remark 4. *(Fig.4) The line of NW-dS is hard to see.*
Answer: Thanks for noticing, we will adjust this in a revised version.

Reviewer 2

General comment. *...The methodology appears technically sound on the whole, but with a few methodological clarifications required as detailed below. The manuscript is attractively written, with a rather modest level of novelty. Some suggestions for increasing the impact of the study follow below, by more widely exploring the potential sources of variability in the Rayleigh number based on the current data sets...*

Specific comment 1. *... discuss the vertical extent of convection that is expected in relation to particular Ra(z) profiles ...*
Answer: We admit that the vertical shape of the profiles is not discussed in sufficient detail. We will address this criticism.

... the maximum value of Rayleigh number is more relevant to the occurrence of convection, rather than the mean value used in figure 3...
Answer: We understand that the maximum seems a more logical choice. We tested both mean and maximum before writing the manuscript. Based on this, we found that the mean value was a better choice. When the maximum is used, the contrast between winter and spring is not as clear as when the mean is used. Besides, whereas the actual numbers are different, the shape of the time series is similar. We will clarify this point in the revised version of the manuscript.

Specific comment 2. *...It is not clear exactly how the time series of Rayleigh number were calculated in figure 4(c)...*
Answer: We indeed did not explain that salinity has been linearly interpolated in time. We will detail this in the figure caption.

Specific comment 3. *Do the vertical profiles of T(z,t), S(z,t), e(z,t) and Ra(z,t) show anything interesting during high frequency variations of T? Are there any Rayleigh number maxima located below the top layer?*
Answer: There is a net tendency of the ice to desalinate, especially near the surface, which might be associated with convection events. We cannot really draw any firm conclusion on this point. Brine volume and permeability also have oscillations near the surface. We will clarify these items based on our computations.

Specific comment 4. *It would be interesting to check the sensitivity of computed Rayleigh numbers to the choice of the choice of the permeability value (harmonic mean or vertical mean), and it seems you have sufficient data to explore this.*
Answer: This is indeed feasible and we will add this to the paper. We quickly did the computations. It makes some difference when vertical permeability variations are large.

Specific comment 5. *A discussion of the impact of spatial variability on Rayleigh number would seem a natural addition to improve the scope of the present study of uncertainties in Rayleigh number.*
Answer: This is actually done in the paper (Page 3220, first paragraph).

Technical correction 6. *Discussion of equation 1. Emphasise that this particular definition of Rayleigh number is for convection that exchanges fluid directly with the ocean.*
Answer: We will add this to the respective sentence.

Technical correction 7. *p3213, l 20. It may help to elaborate on the physical rationale for assuming phase equilbrium.*
Answer: Phase equilibrium simply means that brine salinity is determined by local temperature through the liquidus relationship. This is necessarily the case directly at the brine-ice interface, since there two different phases are directly in contact with each other and phase equilibrium must be maintained. Any temperature change will hence lead to an immediate adjustment of salinity directly at the interfaces, which sets up a salinity gradient within the brine. This gradient is rather large because of the small length scales involved. This gradient will therefore quickly cause the salinity also in the interior of the brine inclusions to adjust to the new temperature. The time scale for the adjustment is given by L^2/D, which for a salt diffusivity of $D \approx 10^{-9}$ m^2/s and a spacing of brine pocket inclusions in sea ice of usually less than 1 mm gives an adjustment time scale of a few minutes until phase equilibrium is again reached after a temperature change. This time scale is usually much faster than the time scale across which temperature changes occur in the interior of sea ice, which is why the assumption of phase equilibrium is usually justified.

Technical correction 8. *p3214, l 18. Emphasize that this comparison is for an assumed $S = 5$ g/kg.*
Answer : Truly missing, we will fix that.

Technical correction 9. *p3215, lines 5-7. Stated "marked permeability increase near e = 5%". I don't see evidence for a marked increase at this particular value in figure 1(c) - the Freitag permeability shows a continuously varying permeability with log Π increasing most rapidly from e = 0%, whilst the Eicken et al. permeability shows that Π changes exponentially throughout the range $0 < e < 0.096$. Can you reword appropriately?*
Answer: You are right, there is no true, specific threshold. Permeability rapidly increases over the 0-10% brine volume fraction range. At 5% of brine volume fraction, permeability reaches 10-12 m2. For this value, the Darcy velocity reaches 1m / day and the brine flow through the ice matrix will certainly start to have a significant impact on the salinity evolution.

p3216, line 27. You mention that the brine diffusivity and LIM model diffusivity span the entire range of possible choices, but both have a diffusivity smaller than the pure-ice diffusivity over the plotted range in figure 1(d). This sentence needs rewording - you might note that using the pure-ice diffusivity instead of the brine diffusivity simply reduces all estimated values of Rayleigh number by a uniform factor of around 10.
Answer: Thanks for this remark. First, the pure ice diffusivity is reached in LIM below -15°C which is why it is not seen on the plot. Second, the brine diffusivity is about 6 times smaller than the pure ice diffusivity. We will clarify both points in the final text.

Technical correction 10. *p3217, line 8. Typo - should be $\nu = \mu / \rho$.*

Answer: Thanks for noticing, this correction will be made.

Technical correction 11. *p3217, line 13, p3221 line 2, and abstract line 11. Recommendation of using Notz and Worster parameterisation. There are some differences in the shape of the profiles between the different formulations (e.g. the LIM version tends to predict larger Ra near the ice base than the NW08 definition, even though the two are similar higher up in the ice), and so it is plausible that one might want to justify a different specific choice in the future. It would be good to emphasise that the present choice is primarily for practical reasons based on the current state of knowledge, in the absence of a clear physical/theoretical rationale for choosing one particular formulation over another.*
Answer: We will follow this very helpful recommendation.

Technical correction 12. *p3218, line 8, "essentially driven by relatively high temperatures". It would be useful to clarify the logical link here - do you mean high temperatures causing increased porosity and increased permeability?*
Answer: We will explain - based on Jardon et al (2013) - that the effect of temperature on permeability dominates the variations of the Rayleigh number.

Technical correction 13. *p 3219, lines 12-14. YROSIAE data. If no reference is available, can you provide some contextual information on the ice growth conditions, sampling strategy etc so that the reader has a feeling for what these data represent?*
Answer: We will explain in a short paragraph the sampling strategy and so on. We agree that there is a need to describe this in the paper.

Technical correction 14. *Section 5. Whilst the message is implicitly there, you might add an explicit statement emphasizing that one cannot rule out the occurrence of transient convective events in the time periods between cores, based only on instantaneous estimates of Ra at the time of coring - further contextual information would be required.*
Answer: We will add the required sentence, indeed necessary. Contextual information is now available in a submitted paper by Carnat et al.

Technical correction 15. *Figure 1(a) - the caption and vertical-axis label appear inconsistent.*
Answer: True, thanks for noticing.

Technical correction 16. *Figure 1(d). It would be more intuitive to plot the pure ice and brine diffusivities for T=0°C at the point with T=0°C rather than at T=-8°C.*
Answer: True, we will do the required modification.

Reviewer 3

General comment. *The missing theoretical basis leads to an ad hoc trial-and-error approach in this work devoid of theoretical insight. Also, only case examples are calculated and it is not stated whether there are any circumstances under which conclusion may be different. While the lack of deeper insight is a shortcoming for publication in a*

scientific journal I think this work makes a valuable contribution nonetheless. However, the presentation appears to be a bit too sloppy in parts where some handwavy comments or half-truths should be backed up with references and qualified, respectively.
Answer: As correctly interpreted by the reviewer, this contribution was mostly born out of our experience that different formulations of the Rayleigh number cause different results regarding a possible onset of convection in sea ice. Based on this experience we felt it useful to summarize this finding and to recommend some best practices for future studies using the Rayleigh number, and in particular to provide some guidance on the interpretation of the Rayleigh number. We tried to emphasize the lack of truly deep, new insights by characterizing this paper as a "technical note". However, we agree with the reviewer that even then, a somewhat more robust interpretation of the case studies we provide, and possibly some more general analysis as to their limitations, would be very helpful. We will therefore aim at providing more such insight in the revised version of this manuscript. We will also put our findings into broader context to published literature that provides related insights.

Comment 1. *I think theoretical clarification should be presented on whether the thermal conductivity should be that of the fluid or that of the solid or a mixture.*
Answer: At this stage, theory does not give a definite answer. The theoretical conundrum is discussed in Griewank and Notz, 2013 (JGR), § 30. We however agree that a heuristic choice of this important parameter should be theoretically justified and will therefore address this specific point in more detail in a revised manuscript.

Comment 2. *Also, the meaning of a Rayleigh number "at" a particular depth defies my imagination and should be elaborated on.*
Answer: We define the Rayleigh number at a specific depth to refer to the ratio of the diffusive and advective timescale of brine travelling between that specific depth and the underlying ocean. We will more strongly clarify this nomenclature in the revised version of this manuscript.

Comment 3. *Tests were performed based on salinity datasets that are either not published or still in press. This may have impeded the peer review process.*
Answer: The salinity data set will be further described in the text to allow the reader to better assess their reliability. Contextual information is also now available in a submitted paper by Carnat et al.

Comment 4. *(Abstract) Where in the manuscript is the uncertainty due to brine loss quantified? Where is brine loss quantified?*
Answer: The uncertainty due to brine loss during ice coring is quantified using the artificial correction on the salinity profile. This point will be clarified in the abstract.

Comment 5. *Where does Untersteiner (1968) say that brine convection (in the sense used in this publication) desalinates sea ice? Isn't he commenting on gravity drainage of levitated sea ice?*
Answer: You are right, thanks, we will correct this point in a revised manuscript.

Comment 6. *Under what circumstances can convection be described by a Rayleigh number of the given form? Reference?*
Answer: We will add an additional paragraph in the introduction focussing on what is the Rayleigh number, how it is derived, when it can be used in a specific form, and why it is subject to interpretation. We will in particular cite more related work. For instance, Wells et al (2010, 2011) and Rees Jones et al (2013a, 2013b). Those will be added in a revised manuscript.

Comment 7. *Interpretation of the Rayleigh number: there should be references that can be added here, in particular since the development of this ratio is a bit more sophisticated than the interpretations suggest. It is not mentioned why this "interpretation" paragraph is useful. The authors return to this briefly in the context of thermal diffusivity where it turns out that the former interpretation seems to imply that the diffusivity is that of sea ice while the latter seems to imply that it is that of brine. Shouldn't there be a right and wrong answer here? This little pearl of science should be elaborated on.*
Answer: See answer to comment 6.

Comment 8. *«If the diffusive time scale is shorter than the advective time scale»sounds like the critical Rayleigh number is 1.*
Answer: Indeed, in an perfect setup the critical Rayleigh number should be around 1. For example, in a recent related model experiment Griewank and Notz (2013) found that their model best agreed with experiment for a critical Rayleigh number of 1.01. However, as for any dimensionless number, the threshold value is usually only given as an order of magnitude, like for the transition to turbulent flow for high Reynolds number. This is related to the fact that these dimensionless numbers only describe idealized systems, and Ra does, for example, not know about the exact flow paths in both brine channels and mush.

Comment 9. *What is the reference for the in-situ measurements? Are there many studies out there to back up this claim?*
Answer: Since Notz and Worster (2008) is the only study that describes an experiment in natural sea ice, we will broaden this statement to allow us to also refer to some of Wettlaufer's papers and to Notz and Worster (2009).

Comment 10. *Full-depth desalination due to Rayleigh number: how does flushing fit in here as de- scribed by Untersteiner (1968)?*
Answer: Flushing is unrelated to the value of the Rayleigh number, since it is driven by the hydraulic head of surface meltwater. We will briefly clarify this point in the revised version.

Comment 11. *Permeability Pi depends on T and S indirectly at best. It depends on the structure of the pore network. T and S specify average porosity. What about air?*
Answer: We will specify in more detail that when air content is neglected (which is fair enough for first-year ice) permeability has been found to be primarily a function of porosity (e.g., Freitag 1999), which, as you describe, is determined by T and S.

Comment 12. *Ice core derived Ra has been provided" apparently only by studies the current group of authors has co-authored. Or are there independent groups that could be mentioned?*
Answer: The sea ice world is small, but «Gough et al» were not part of the authors of the present study. There are Canadian studies that are in preparation (we have seen posters at various conferences) where Ra is calculated from ice cores. We'll check if this work has been published yet.

Comment 13. *Apart from errors in S and T: errors in Pi due to unknowns in the pore network, maybe? Are we talking about FYI only? Arctic and/or Antarctic? Granular ice?*
Answer: We will mention «uncertainties in pore network structure» and specifically address the uncertainty of relating porosity to permeability for different ice types.

Comment 14. *These days sea ice salinity is the result of a conductivity measurement rather than a mass measurement. I believe nobody has measured mass fractions to determine salinity for decades. I would like to refer to the pertinent UNESCO documents and strongly advise that gkg-1 not be used as a sea ice salinity unit in The Cryosphere. According to the UNESCO documents salinity is unitless.*
Answer: We use the new TEOS (2010) convention throughout, according to which salinity should be given as absolute salinity with units g/kg (see www.teos-10.org). TEOS is the new standard for the equation of state of seawater, which is or will be used everywhere, by field scientist and modellers and is highly cited. Our choice is to be consistent with TEOS.

Comment 15. *"often a simpler": references?*
Answer: Malmgren 1927; Schwerdtfeger, 1962; Bitz and Lipscomb 1999; ... could be cited.

Comment 16. *It is implicitly assumed here that sea ice permeability is only a function of porosity. It should be acknowledged that it is not and why. What kind of ice do the parameterizations relate to: newly formed ice? Ice late in the growth season? How does this matter for the pore structure?*
Answer: We will add a sentence to explain the role of pore structure on permeability. We will also explain more how both parameterizations were derived (see also comment 13).

Comment 17. *"a marked permeability increase exists": what does "marked" mean? A break in slope on a log-log plot??? (trick question).*
Answer: Another reviewer made a similar comment (Reviewer 2). The sentence is indeed imprecise. Permeability rapidly increases over the 0-10% brine volume fraction range. They key point is that at 5% of brine volume fraction, permeability reaches 10^-12 m2. For this value, the Darcy velocity reaches 1m / day and the brine flow through the ice matrix may start to have a significant impact. We will clarify this point in the revised manuscript.

Comment 18. *What is the "Rayleigh number for a certain level"? The Rayleigh number as I know is describes an entire system with a specific domain configuration and well-defined boundary conditions. Talking about a Rayleigh number at certain level simply does not seem to make sense. This should be clarified in the introduction when the Rayleigh number is thoroughly introduced.*
Answer: See comment 2, we will clarify this use of terms.

Comment 19. *Based on what theoretical basis should the harmonic mean be the relevant quantity? How should anisotropy of permeability be dealt with?*
Answer: The harmonic mean follows directly from applying Darcy's law individually to each layer in 1 dimension. Using the minimal permeability is a simplification of this harmonic mean. We will add a proper explanation and will try to find a reference (probably textbook). The anisotropy of the permeability cannot really be dealt with in a 1D framework.

Comment 20. *"usually": references?*
Answer: Notz & Worster, 2008; Vancoppenolle et al 2010; Gough et al., 2010; Griewank and Notz 2013 could be added.

Comment 21. *"z": early formulations of the Rayleigh number... I think insights should be added here (or in the introduction) on the nature of the Rayleigh number, see comment above. Wettlaufer may have had a very good reason to do as he did.*
Answer: As stated earlier, we will add a paragraph in the introduction on the Rayleigh-number itself and on its depth dependence.

Comment 22. *The fundamentally different interpretations of kappa merit, imho, a thorough introduction to the concept of Rayleigh number in the introduction. Are all approaches presented of equal scientific value?*
Answer: We will try to clarify the different interpretations of kappa and more thoroughly justify our particular choice.

Comment 23. *Is "heat diffusivity" the same as "thermal diffusivity"?*
Answer: Thanks for noticing, we will use thermal diffusivity throughout.

Comment 24. *Unfortunately, there is absolutely no scientific insight in the presented recommendation. Could this be changed if only to acknowledge that this is a scientific journal? Also, it would be much appreciated if a theoretical basis would be provided explaining whether thermal diffusivity should be that of ice, sea ice, or brine.*
Answer: We will aim to more thoroughly justify the choice of this particular recommendation, in particular regarding the choice of thermal diffusivity. Given the current uncertainties in representing some of the other parameters that go into Ra, we will clarify in as how much our choice of a particular recommendation simply reflects our shortcoming in robust experimental and theoretical insights, but that for practical purposes a standard choice is still helpful.

Comment 25. *"close to the freezing point" of what?*

Answer: We will change the text into «seawater freezing point».

Comment 26. *what is a "0.5 underestimation"?*
Answer: We will change this to "underestimates Ra by 0.5"

Comment 27. *"This type of error": what type of error?*
Answer: We will be more precise and change into: «underestimating salinity near the ice base by a few g/kg».

Comment 28. *Temperature varies strongly at sub-daily time scales? Close to the ice-ocean interface???*
Answer: No, we meant to refer to sea ice near the surface, thanks. We will correct the sentence.

Comment 29. *"are also important" for what?*
Answer: We will change this to "Large spatial variations exist…"

Comment 30. *«weak evidence of the basal winter permeable layer». Petrich, Karlsson, Eicken (2013) have recently discussed porosity of the winter permeable layer. Is that related to the matter discussed here?*
Answer: The basal permeable layer in winter sea ice is known from very early times (see for instance the introduction of the paper you mention). It has adopted several different names that encompass similar things. We will be more precise in the revised paper.

Comment 31. *After reading this manuscript I get the impression that the nature of the Rayleigh number is a bigger unknown than brine properties at low temperatures. As an aside, could the authors recommend a reasonable salinity sampling interval at the ice–water interface in the light of brine loss at different times of the year?*
Answer: We agree that the interpretation of the Rayleigh number should be backed up by more experimental work to decrease uncertainties. There will be more material to describe what the Ra number is. We do not fully understand the last comment: brine loss at the ice-water interface is rather independent of a particular time of the year, since temperature there is always comparably warm and brine loss will always occur. This cannot be overcome by changing the sampling interval.