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Interactive comment on "The multiphase physics of sea ice: a review" *by* E. C. Hunke et al.

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Response to Reviewer Ackley's comments

S. Ackley: General Comments:
Since this Review paper is not the usual presentation of scientific results, it cannot be subjected to the usual criteria for a Journal article, e.g. original work, new results, scientific accuracy, etc. I therefore offer some personal criteria to judge the quality of this review such as: Is the work reviewed a relatively complete summary of the subject area? Does the review provide a source of relevant material that can be used as background for other papers on the specific details of the subject, such as field work or modeling studies? The context of this article is the authors' statement that the review resulted from a Workshop on The Multi-Phase Physics of Sea Ice: Growth, Desalina-

tion and Transport Processes held in 2010. As a result, the authors have sacrificed the breadth of sea ice physics, experimental and field data, for instead looking at Mushy Layer Physics and how it explains basically steady state growth of sea ice in a highly controlled laboratory situation, which was an emphasis at that workshop. It may be useful for understanding the physics of the medium. However, in the 2nd half of the paper the authors review thermohaline descriptions of the sea ice component of global climate models. Since these rely on older physics, and make simplifications for computational efficiency, the relevance of Mushy Layer Physics to at least current numerical modeling is questionable. A better connection between the first half and second half of the paper is therefore sorely missed. I personally therefore did not find this review particularly useful as it is lacking in both completeness and as a source. There are only two figures in the paper (four, if you count the three parts of Figure 2 individually). Comparatively, the chapter on Sea ice Salinity in the book by W.F. Weeks (On Sea Ice, U of Alaska Press 2010) has 43 figures. That chapter is of similar length to this paper and also includes a succinct description of Mushy Layer Physics, including about 5 figures, none of which are included in this paper, despite the emphasis here on that topic. I think a Review titled the Multiphase Physics of Sea ice should include a figure of the Phase Diagram, and a discussion on the calculation (and dependence) of Brine Volume and Brine Salinity on Temperature. As well, Figure 1, after Malmgren 1927, implies that the evolution of the salinity profile is pretty standard. However, while Eicken 1992 is in the Reference List, his classification of the salinity profiles observed in Antarctic sea is not referred to. These indicate that of the order of only one third of the measured profiles in Antarctic sea ice are similar to those shown in Figure 1 (The "C" profile). Three other profile models are found due to the variations of driving forces, including surface flooding and high ocean heat flux , that are found to a different degree in the Antarctic. One premise of the Review is that the evolution of ice salinity makes a large difference in the way that sea ice is modeled, and drives many of the results. However, if the salinity profiles over the majority of the Antarctic ice pack do not conform to the assumed "C" profile, how can modeling results give an accurate representation? Maybe they do, but by not showing that they do, the reader is left with the impression that the 1927 results from Malmgren are the observed state of the art which the modelers are working toward replicating. In the abstract, the focus of the review is identified as mushy-layer theory (since it “describes general multiphase materials" and on numerical approaches now being explored to model the multiphase evolution of sea ice. The implication is that these numerical approaches are using mushy layer theory but, with the absence of any model derived figures (figs 1 is Malmgren's observed profiles and Fig 2 is pictures of brine pockets, and brine tubes observed in the laboratory and field), at least no comparisons are available. The entire review is a narrative that could be described as a continuous annotated bibliography. No information can be extracted from the review itself without using the original references. The first half of the paper, on mushy layer theory is actually a review of a previous paper , Notz and Worster 2009, so the first half is a review of that review? If all you're going to talk about is a single paper, why go through it again in abbreviated form? Why not just say: “the main processes of salt removal from sea ice are initial rejection at the growing interface and gravity drainage from sea ice, followed by flushing from meltwater in the summer. Except for Flushing, mushy layer theory (Notz and Worster 2009) best explains the underlying physics of these processes. See that paper for details." This article therefore represents the end product of the workshop and reflects the participation there, rather than the more comprehensive view taken in other reviews. Since these other reviews are recently available in book form, (Dieckmann and Thomas, 2nd Edition Sea Ice Physics Chemistry and Biology, and W.F. Weeks, On Sea Ice), I think an attempt to expand this review to be as useful is perhaps a duplicative effort, so recommend that the paper remain as a TC Discussion paper rather than trying to correct it into an article in The Cryosphere.

Response: We are very grateful to Steve Ackley for his very thorough reading of the paper and his thoughtful commenting on the paper's shortcoming in its present form. We believe that much of these comments are caused by the fact that we did not make sufficiently clear what the intention of this paper (and the workshop on which it is based)

is: to provide model developers with a one-stop reference which will allow them to develop the next-generation of sea-ice models that, because of the increasing focus on biogeochemistry, also must include a description of the ice's multi-phase physics. Hence, this review was never intended to replace or supersede the relevant chapters in Willy Weeks' book.

As such, we find a certain disconnectedness between the first and the second half of the paper easily explicable by the fact that the first half provides a theoretical foundation on which future sea-ice models can be built, while the second half focusses in part on past models to allow for an appreciation of what is needed to improve these models in the light of our modern, improved understanding of sea-ice microphysics.

First, we propose to clarify the intent of the paper by changing the title to: “The multiphase physics of sea ice: a review for model developers". We will also emphasize the intent further in the introduction and elsewhere in the paper.

How to parameterize mushy-layer physics in sea ice models is an open question. The thermodynamics model of MU71 is a particular case of mushy-layer physics (Feltham 2006), and recent one-dimensional approaches are somewhat more complete (Notz AG06, Vancoppenolle et al 2010, Jeffery et al 2011). The best approach is not yet identified, but in the paper we will better explain how sea ice models represent mushy-layer theory.

All authors of this paper are currently involved in developing such improved sea-ice models, mostly based on mushy-layer theory, and these developments are promising enough that we would like to encourage and help others to work in the same direction. This was the fundamental reason for holding a related workshop and for writing this review.

Despite the existence of related chapters in recent text books, the specific focus of this review makes it unique and, in our opinion, useful for developers of advanced sea-ice models. For example, a similarly succint description of mushy-layer theory is, as far as we know, currently lacking from the literature. The description of mushy-layer theory by Weeks is general and reviews the approach proposed by Worster and Wettlaufer (1997). Here, we instead propose a one-dimensional version of the mushy-layer equations, applied to sea ice. In particular, the splitting of the different terms of the equations is specific to sea ice: the formulation of the solute equation we propose is written in terms of bulk salinity and includes an effective diffusivity, for instance. This is more advanced than the equation found in Weeks' book. We also propose an equation for ice enthalpy instead of a temperature equation, which is more suitable for modeling. Moreover, we provide an overview of how multi-phase physics is represented in models currently and how this affects the sea ice mass balance. This has not been done elsewhere.

Despite our general disagreement with the reviewer's comment that this review is, in his opinion, unnecessary, we do agree with many of the more detailed comments by the reviewer and will improve the paper along the lines of his comments.

S. Ackley: Specific Comments:
p. 1950 l.7 The statement that brine can drain from the ice, taking other constituents with it, depends on the ice porosity is a bit inaccurate. The permeability is the correct term, and it depends on structure as well as porosity. The two terms are used interchangeably throughout the paper but they should be distinguished.

Response: We will be more precise in our use of these terms.

S. Ackley: p.1951 There is a very qualitative description of the variation of brine, salts and ice as a function of temperature with no significant information passed on. This would be a good spot for the Phase Diagram. (e.g. It is stated that "solid salts start to precipitate at -2.2C" yet no identification of this as Calcium Carbonate with highly significant role in CO2 exchange. "other salts remain in solution. . .below -

50C". First of all these temperatures are rarely , if ever achieved in natural sea ice, so the temperature range of interest is the NaCl eutectic (down to -21 C) when 99% of the brine is converted to solid salts. The other salt that comes out is Mirabilite, sodium sulfate, at -7 to -8C (if I had the phase diagram I could be more accurate. . .) Above temperatures of -10C are of more interest since the cutoff in permeability occurs (typically) in that range and the highest volume of sea ice will be above -10C for most of its existence. Why give a qualitative description when real values are easily mentioned?

Response: We will add the Eicken 1992 phase diagram with discussion as requested.

S. Ackley: p.1952 Mention of the carbon cycle and the precipitation and dissolution of CaCO3 as a pathway for CO2 exchange, however It's quite disconnected from the incomplete description in the previous page where the highest temperature salt precipitated as calcium carbonate is not identified.

Response: This comment will be resolved by including the the phase diagram and discussion as mentioned above.

S. Ackley: p. 1952 on iron accumulation, dissolved iron is mentioned. but a more recent paper by Lannuzel et al 2011, shows that particulate iron accumulated outweighs dissolved iron by 23:1.

Response: Thank you for pointing out this paper, with which we were unfamiliar (actually Van der Merwe, Lannuzel et al., Marine Chemistry, 2011). Understanding why particulate iron accumulates more than dissolved iron, and similarly why dissolved iron accumulates in sea ice more than salt (Lannuzel et al., JGR-BGC, 2010), will rely on a better understanding of multiphase physics and interactions with biogeochemistry. We will include this new information.

S. Ackley: p.l1954 Brine salinity is therefore only a function of temperature T. (see Cox and Weeks for empirical functions). In Willy Weeks new book, there is a whole chapter on the Phase Diagram, which is based on empirical data but follows fundamental principles, since Gibbs, of physical chemistry. Yet, it's not worthy of mention, never mind showing, in this review. Also, it would be helpful to actually give the empirical functions of Cox and Weeks.

Response: We will reference Weeks' phase diagram discussion in association with the Eicken 1992 diagram, and we will add the empirical functions of Cox and Weeks or, alternatively, the formulation of Notz 2005.

S. Ackley: P.1954 Brief overview of mushy-layer theory. I think there is bit of overemphasis on Mushy Layer Theory as providing all the answers, especially considering the later sections on numerical modeling which, for the most part, do not include mushy layer theory. Most of the advances using mushy layer theory deal with the case, using laboratory results, corresponding to constant heat flux from above resulting in one dimensional freezing of the ice. This can be used to predict the formation of brine channels, the release of salt from the ice, and the conduction of heat as the ice is freezing.

Response: We will clarify our stance that mushy-layer theory captures most of the underlying physics and can hence be used as a foundation for improving current models. We believe that our improved understanding of the fundamental physics that is derived from laboratory experiments is also useful for the modeling of real sea ice, as is shown by ongoing work in our research groups.

S. Ackley: However, the authors mention Feltham 2006 for a derivation of classical parameterizations of sea-ice properties from mushy layer theory. Is it therefore necessary to use full mushy layer theory in numerical calculations, or are these parameterizations

more efficient to apply computationally?

Response: The aim of Feltham, 2006, was to show that classical parameterizations of sea-ice thermodynamics are largely compatible with mushy-layer theory. However, mushy-layer theory additionally allows us to model the salinity evolution of sea ice and hence extends the classical parameterizations.

S. Ackley: Similarly, I personally think that many of the problems worth looking at are those dealing with temperature cycling, when the ice flops between cold and impermeable to warm and permeable, or in the warming case, when the structure laid down by the freezing process is already there. If the main channel structure is already there, is it necessary to use mushy layer theory to determine the fluid flow paths again? Or can some statistical ensemble of pipes of varying sizes be used with simpler, more efficient, equations?

Response: In terms of developing a new sea-ice model, it might well be that at the end of the day, such model will use a simplified representation of sea ice, as for exmple a statistical ensemble of pipes. However, to test the accuracy and applicability of such simplified models, more complex models based on mushy-layer theory are very helpful (in addition to improved lab- and field measurements, of course).

S. Ackley: p. 1960 For the sake of discussion, I would like the authors' to comment on the following point. An argument is given (P.1960) that mushy layer theory somehow invalidates the Burton etal sea ice rejection (which uses keff , the effective partition coefficient, and does not depend on gravity), since salt rejected would be reincorporated when the mushy layer equations are applied without gravity. Sure, and all those oceanographic parameterizations that use the "eddy diffusivity" (eddies are not diffusive, are they?) are no good either. But, does keff actually implicity include gravity, through the brine densification, so the argument the keff is actually the same as k,(the salt molecular partition coefficient) is analogous to calling the eddy diffusivity the molecular diffusivity? Isn't it what numerical modelers would call a "parameterization"?

Response: While keff might prove a usefully simple parameterization for some of the brine drainage, the physical idea of Burton was that salt was actually rejected from the growing interface and was never incorporated into the mushy structure. This is now known not to be the case physically. It is not clear whether Burton's theory remains applicable in situations encountered in today's models, for example with biogeochemical tracers or high vertical resolution. We will address this in the paper as requested.

S. Ackley: p. 1960 and on it goes, now we recapitulate the inferiority of the brine diffusion or brine pocket migration argument, but we knew that in 1968 (Untersteiner). p. 1961 This argument on brine expulsion, now refuted by mushy layer theory (Notz and Worster 2009). What isn't mentioned however is the possible role of brine expulsion in pushing brine upward in thin sea ice, possibly accounting for high salinities at the top layer of the sea ice?

Response: This is true and will be incorporated in a new version of the paper.

S. Ackley: p.1964 Approaches. After about a 15 pg exposition of the merits of applying mushy layer theory to sea ice, we are given a short paragraph on how it is applied to direct numerical simulations of individual crystals in the metallurgy fields, divided into sharp and diffuse interface solutions. After these descriptions of the numerical setup, with references, the following statement is made: "The heavy computational burden of explicitly tracking the microstructure makes these techniques unsuitable for modeling a complete sea-ice layer, although they may prove useful in determining appropriate subgrid scale parameterizations, such as the permeability, for averaged models." The next section then deals with Volume averaged simulations, where individual ice crystals

and brine inclusions no longer need to be explicitly modeled, so "the problem becomes much more computationally tractable". Then, simulations specifically for forming sea ice, compared to the lab results of Cox and Weeks 1975 and Wettlaufer 1997a. results in "The ice growth rate and brine drainage rate were comparable to the experimental results, but the simulations lacked the observed delayed onset of drainage." Are the authors implying that the physics is validated, and that the deficiencies are a result of the need for volume averaging for computational efficiency? Maybe volume averaging instead causes some important physics to be overlooked? Is it good enough to use numerically if the onset of drainage is delayed?

Response: The authors are not implying that mushy layer models are not useful because of imperfections in some implementations. It is unclear why this particular model does not produce the delayed onset of drainage, and this result should not be generalized to all mushy layer models. However, modeling the critical Rayleigh number transition indeed may be difficult to do in this type of model.

One of the authors (DN) has just carried out some new tank experiments on sea-ice formation in open water. From these experiments it seems that much of the observed delay in the Wettlaufer et al. paper was related to the fact that they cooled the ice with a metal plate, hence prescribing the surface temperature. In the new experiments, the ice surface is in contact with cold air and hence much warmer at the initial stages of sea-ice formation. In these experiments, his team found only a very short delay in the onset of convection. These results are not final, and although we mention them in this reply to the review, we will not include this information in the paper.

S. Ackley: One dimensional simulations p. 1966 "For computational efficiency, the thermohaline description in the the sea ice component of global climate models is one-dimensional." I think this statement indicates the primary disconnect in the review between the modeling and the theory. The review in the section on Multi-Layer Physics of Sea Ice has dealt nearly exclusively with the Mushy Layer theory, i.e. a medium with convecting chimneys separated by a porous medium. If most of the numerical simulations are dealing with other than Mushy Layer Physics, wouldn't the audience be better served by a description of the physics that is actually used, rather than one that isn't applied?

Response: Historically, the sea ice thermohaline description in global climate models is one-dimensional because the first-order thermodynamic effect (conductivity) is primarily vertical in nature. It is true that convection is fundamentally multi-dimensional, and to avoid excessive computational expense, 1D approximations will continue to be used in GCMs. However, 1D parameterizations of brine flow suitable for climate models can be based within the framework of mushy-layer theory, using Darcy's Law to simplify the complex flow of brine to one dimension, as discussed in the theory section of this paper. New thermohaline models based on this approach are now being developed, and this fact is the primary reason for writing this paper. We will make this clear in the revised version.

S. Ackley: p. 1966-1970 These descriptions of the various model results would be helped by some figures that show some of the important results.

Response: We will provide several figures for specific modelling questions such as: "What is the potential impact of the salinity profile on ice thickness?" and "What is the large-scale potential impact of an interactive salinity description?"

S. Ackley: p.1970 "lack sufficient observational data to narrow the range of model parameters, a potential hindrance for further development." (puzzling statement, very little of the observational data that is available has been used or cited, so weakens the case that sufficient observational data doesn't exist)

Response: The authors of this paper have utilized as much of the available data as possible for model development and validation. Because they have so many degrees

of freedom (spatial and temporal resolution, for instance), models require information that is not sufficiently available in observed data sets. A simple example is the vertical profile of brine velocity, which is critical to the evolution of the salinity profile and tightly coupled with the permeability of the ice.

S. Ackley: p.1972, figures needed to show the impact of salinity variations on sea ice from Vancoppenolle et al 2009a from their sensitivity simulations.

Response: We will include more figures as mentioned above.

S. Ackley: p.1972 Couple of statements that are a little misleading. "only brine convection in the lowermost ,porous sea ice (Reeburgh, 1984) and the flooding of the surface by seawater (Fritsen et al 1994) seem intense enough to provide the required nutrients to sustain biological growth in the ice. " Fristen et al actually showed that later freezing in flooded layers drove convection that caused the near-surface intense biological growth. So convection in both the uppermost, (or full thickness convection) and lowermost layers can both sustain biology. The mechanism for surface flooding itself to have high biology is still somewhat ephemeral, e.g. may require some waves, floe surging etc to mix surface seawater into flooded layers. The next statement that "vertical profiles of dissolved macronutrient concentrations and salinity have a similar shape" is also misleading. Several of these studies have shown for example that nutrients in the sea ice do not scale with salinity and are either depleted or enhanced because of biological activity. So similar shape, with the implication that nutrients are passive tracers of salinity ,masks the possible true behavior.

Response: The reviewer is correct that our description is somewhat inaccurate. We will rewrite this part using more precise wording.

S. Ackley: p.1973, algae, nutrients, trace metals, gases, are incorrectly here identified as passive tracers. Some of them might be, but for the most part none of them really are, e.g.algae stays within the ice while brine is rejected, nutrients are taken up proportional to biology, and CO2 and DMS gases have radically different profiles from salinities.

Response: The two papers mentioned in conjunction with this discussion do not treat these quantities as strictly "passive" tracers moving with the flow that only change because of the flow itself, but as passive tracers in the sense that they do not affect the flow by changing the ice microstructure, for example. We will clarify the terminology.

S. Ackley: p.1973 Some success is ascribed to Vancopenolle et al2010 and Jeffrey et al 2011 in one dimensional models based on transport equations "containing some of the mushy-layer physics", again here some display of results would have been helpful.

Response: We will add figures as requested.

S. Ackley: p.1974 "There appear to be potential nonlinear interactions between brine dynamics and biogeochemical sources and sinks" Would this statement and the following couple of sentences have been a much better introduction to this section than the previous couple of pages which instead lead the reader into believing that there is merit in considering biogeochemicals as passive tracers?(See above comments.)

Response: We will consider this comment when revising the paper.

S. Ackley: Observations p.1975 Mention is made of the time series of ice salinity evolution and the lack of temporally and spatially resolved data from ice core studies. However, as reviewed by Weeks in On Sea ice, studies by Kovacs have determined functions that describe ice thickness and salinity that use a large quantity of observed cores from both the Arctic and Antarctic. As well," the lack of data to test numerical

models that describe the microstructure of sea ice with high spatial resolution", given the various parameterizations described here that obviate the "high spatial resolution" for "computational efficiency" seems a bit harsh? While data may be sparse in relation to the widespread areas covered by sea ice, don't the similarities in the data from many areas suggest that high spatial and temporal resolution may not be quite as necessary? Doesn't Malmgren's data from a handful of cores in 1927 look quite similar to the detailed profiles of Nakawo and Sinha in 1981? Doesn't Eicken's (1992) classification of salinity profiles from Antarctic sea ice give some of the generality of this behavior as well? Its generally, however, hard to argue that better and more observations are needed, particularly in a time-series sense, for better comparison to numerical models. But their case is weakened when known observations and classifications are ignored.

Response: We will discuss the Eicken 1992 classification in the context of model validation. As mentioned before, in this paper we describe all of the data that we know of. If the reviewer can point us to additional data sets, we would be grateful!

Even though climate modelers might use a 1D thermohaline parameterization in the end, their relatively high-resolution results (from many vertical columns of ice that are transported horizontally) will still need to be compared with data to determine whether the various processes are working in correct relative proportions across different regions of the polar oceans at all times of year.

S. Ackley: p.1977 Bit misleading to refer to ESA's Soil Moisture and Ocean Salinity satellite, which, as far as I know, would have no application to determining sea ice salinity and microstructure.

Response: The reviewer is generally correct in that it will not be possible to retrieve sea-ice salinity from SMOS. However, there is some work going on to retrieve sea-ice thickness from SMOS, for which the algorithms depend crucially on knowledge of sea-ice salinity (http://www.the-cryosphere.net/4/583/2010/tc-4-583-2010.html). We will clarify this point.

Interactive comment on The Cryosphere Discuss., 5, 1949, 2011.