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The Cryosphere Discussions

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# *Interactive comment on* "A 1-D model study of Arctic sea-ice salinity" *by* P. J. Griewank and D. Notz

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Final author response to review comments C1279 on the TCD manuscript "A 1-D model study of Arctic sea-ice salinity"

We would like to thank the referee for taking the time to read and comment on the manuscript. Most of the comments raised arise from misunderstandings of the intended scope of our paper. In particular, many of the descriptions that the referee misses were published in other papers or are indeed addressed in the current manuscript. Most of the comments on the proposed parametrizations seem to arise from misunderstandings of how the proposed parametrizations work. The misunderstandings show that the current parametrization descriptions are insufficient. To address this we will revise and expand the subsections describing the parametrizations



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as detailed below in comment 4, after we address the referee's other comments.

Comment 1: Lack of differential equations and underlying model physics.

The core model description is indeed not included in this paper. This is intended, as the core model description is in the Griewank and Notz 2013 paper. We explicitly state in the beginning of chapter 2 page 1727:20-21: "We provide a very brief description of the fundamentals of SAMSIM in Section 2.1., a detailed description can be found in Griewank and Notz 2013". Here the definitions of mass fractions, volume fractions, and so on can be found. The current paper is already lengthy, and we are not convinced that the paper quality would be increased by adding a lengthy appendix of already published material. However, to reduce the chance of readers thinking that the fundamental description is missing we will sprinkle section 2 with references to Griewank and Notz 2013.

Comment 2: The parametrizations are not presented independently of the model numerics.

The surface melt and brine parametrizations we present were developed explicitly for SAMSIM, and can not be easily separated from SAMSIM's numerics. There are no basic underlying equations which can be solved with a variety of numerical methods. For example the treatments of snow melt and surface ablation rely on both the snow and top ice layer being homogeneous finite volume layers with a liquid, solid, and gas phase. The proposed approach is already not applicable to any model with multiple snow layers, no top ice-layer gas fraction, liquid water free snow, or a differently defined ice-snow interface. We will add some sentences to discuss what the numerical requirements of the various parametrizations are, so readers can judge if and how the approach could be adapted to suit their needs.

Comment 3. "The simulations and results of this paper do not provide validation for any of the melt processes discussed previously. Before the model can hope to inform about Arctic sea ice, a rigorous comparison of model and lab/field data needs to be



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performed."

We do not understand this comment for two reasons. The first is that no sea-ice model has been evaluated with lab data of melting sea ice, because no such data is available. To our knowledge the only lab measurements of melting ice are the temperature measurements conducted during the Master thesis work of Wiese 2012. These data are currently being published in a paper which includes a comparison to SAMSIM, but they can only provide a very limited validation of the model as they were conducted under highly idealized settings (such as no snow, solar radiation, wave motion, .....) and only measured temperature. The second reason why we do not understand this comment 3 is because subsection 4.3 is a dedicated model-field data comparison over 7 pages which is accompanied by figures 9, 10, and 11. Given that validation with lab data is impossible and validation with field data already included in the paper, we do not believe that our manuscript is lacking in validation.

Comment 4. The assumptions of the salinity parametrizations are flawed and do not confirm with physical reality.

We believe that we have a different understanding of what constitutes a parametrization. Given the lack of both theoretical understanding and data on flooding and flushing, our parametrizations are not based on physical first principles. Our parametrizations are intended as physically plausible approaches which seek to imitate reality in an internally consistent matter. In regard to specific comments:

-"The expression for ice permeability as function of permeability by Freitag is only valid for vertical flow"

The function of Freitag is a very rough approximation which completely neglects many aspects of ice such as micro-structure. Using a different horizontal flow permeability is highly unlikely to improve results, especially given that to our knowledge no function of horizontal permeability based on porosity has been published. We will mention that the permeability of Freitag is a rough approximation which does not take ice type or





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direction into account. Thank you for noticing that the citation is missing, it will be included.

-"The horizontal pressure force from positive freeboard will depend on the vertical resistivity"

The horizontal pressure force in the model does depend on vertical resistivity. The vertical and horizontal hydraulic resistance (page 1738 and 1739) of each layer affect the pressure force in the layer as well as all layers below it.

-"For example, highly porous ice will have almost entirely vertical flow, while an impermeable layer will possibly allow for runoff at the surface (though not in the interior). The “horizontal” terms in this scheme do not capture these features and obscure the impact of the vertical flushing term making it extremely difficult to validate."

This behavior is well captured by the hydraulic parametrization. If there is an impermeable layer horizontal flows can occur, and they will be strongest at the surface. And there is no data that indicates that horizontal flows in the interior of permeable ice do not occur, even if they are most certainly weaker than those at the surface. To make the hydraulic parametrization easier to understand we will include a few simple examples at the end of section 4.3.2 which illustrate how the resistivity, pressure force, and flows interact with each other. A figure with subfigures similar to figure 5 will be introduced to make comprehension easier. This will also address a demand of the first referee to introduce ladder circuits more thoroughly.

-"The simple flushing scheme (section 2.4.3) is also problematic. It imposes a stability criteria that has no experimental or physical foundation and is, in fact, violated during sea ice growth and gap layer formation or the freeze melt cycle. Density and not volume determines a stable profile. In addition, the description of the scheme needs an equation or two."

As the simple flushing parametrization is not explored in detail we did not spend much



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time on introducing it. As flushing is not driven by buoyancy a stable profile is not determined by density. Instead the stability of melt water at the surface is determined by the ratio of gravitational force to capillary suction. This plays no role during winter when the permeability off the ice is much lower, and so we need not worry about our assumption being violated during growth. The simple assumption the parametrization is based on manages to achieve a number of flushing characteristics. The simple parametrization limits the effects of flushing above impermeable layers and ensures that the salinity decreases towards the surface. We will add some text on the physical reasoning behind the parametrization and include a simple equation.

-"For the complex flooding scheme (section 2.4.5), it is not clear why a hydraulic flow is not considered. In line 15 (p 1741) it is stated that “upward brine displacement through the whole ice (would result in) desalination (that) would quickly turn the ice impermeable”. However upward flushing has been observed through the ice interior. It will desalinate some ice layers while increasing others and produce, in fact, a net increase in ice salinity. The authors need some physics based reason to exclude a process (upward driven hydraulic flow) that they include without justification for flushing (down-ward driven hydraulic flow)."

We have conducted tests to see what happens when an upward hydraulic flow through the ice is considered and as stated it quickly turns the ice impermeable. We did not include this in the paper, as the same result was already observed by Maksym and Jeffries in 2001. As they have already made (and published) the result, we see no reason to reproduce their results. We refer to them in the paper with the following wording: "as Maksym and Jeffries (2001) showed that if flooding resulted in an upward brine displacement through the whole ice the resulting desalination would quickly turn the ice impermeable." However we will add a sentence that our experience was the same as Maksym and Jeffries.

The proposed hydraulic flushing parametrization does include an element of down-ward driven hydraulic flow, but the proposed ladder circuit splits up the flow. Furthermore, the almost uniform temperature profile which occurs during ice melt reduces salt reduction via advection, and the atmospheric heat input into the ice decreases the available heat sinks needed to form impermeable layers. We will include a few sentences to the flooding description why the situation is different during melt.

Regarding the upward flushing that has been measured, we know that brine reaches the surface, but we do not know how the flooding water reacts with the brine in the ice. Until we have a salinity profile time series during flooding from a non destructive measurement (ice cores would provide flooding path ways) we can not say for certain what is happening inside the ice.

"The parameter, maximum negative freeboard, is set a 5 cm. Why isn't this dependent on ice thickness?"

We have toyed with the idea of making the maximum freeboard a function of ice depth, but given the scarcity of available data we have no reason to believe it would provide a clear improvement. While the current constant assumption enables the rather unlikely scenario of having very thin ice (e.g. 3 cm thick) to have a negative freeboard of 5 cm, this will only occur in the most unlikely of scenarios for two reasons. Snow fall rates would have to be extremely high to accumulate enough snow on newly formed thin ice, and thin ice with a (relatively) thick snow layer will have a high bulk salinity and temperature, which in turn ensures the ice is highly permeable. The high permeability ensures that the ice is flooded quickly and the negative freeboard is quickly resolved.

Regards, Philipp J. Griewank and Dirk Notz

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Interactive comment on The Cryosphere Discuss., 8, 1723, 2014.