

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

Anonymous Referee #1

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The paper uses the SAMSIM 1D sea-ice salinity model with new parameterizations of melt-water flushing and snow-ice formation to examine salinity in the Arctic. In general the paper contains some interesting results and should be published subject to the corrections listed in this review. The main problem with the paper is that large parts of the description of the parameterizations are hard to follow and confusing and should be rewritten for clarity.

Detailed comments: _____

p1723: The authors may wish to change the title to : "A 1-D modelling study of Arctic sea-ice salinity"

p1724:15-17: "We conclude that for earth system models the impact of fully parametriz-

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ing the Arctic temporal salinity evolution is too small to justify the increase in computational cost and model complexity" - Remove, as described more fully later the authors can only conclude that it is not justified with their model not everyone elses.

p1728:9: "is rainfall into snow" - don't understand this - explain more clearly what you mean.

p1728:13-14: Explain more fully how this is different from other models. Presumably it is that z_0 is fixed for the duration of the model run.

p1728:25: italicize "alb" and "pen" and put commas before them. p1728:26: comma before κ .

p1731:5-7: Describe in what ways this sacrifices physical consistency.

p1731:10: How would shortwave absorbance not affect melt water formation??

p1731:17-18: Why is the surface melt speed not available?

p1732:10: Clarify that the convective overturning is within the ice and not the top layer of ocean.

p1732-p1734: The section entitled "snow melt" is unclear and confusing in its description of the parameterization.

p1732:20: "and a scientific field of its own". Meaningless phrase - remove.

p1733:3-5 "In Fig. 2 ... area depicted." Unclear - rewrite to make it clearer what you mean.

p1733:8: Clarify whether the slush layer thickness exists outside a timestep. i.e. can it be meaningfully diagnosed or is it not clear what is slush and what is top ice layer.

p1733:21: "compacted" - does the snow really increase in density while conserving mass? That's what compaction is. Replace if this is not what you mean, if it is explain more clearly what is happening.

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p1734:1: "slight delay". Explain explicitly what delay you mean and why it occurs.

p1734:7-8: "As a result...Eicken et al(2002)" - Explain more clearly why this is the case.

p1734:9: "we trust" - Demonstrate this. Its up to the authors to convince the reader, not for the reader to take it on faith.

p1734:24-25: Don't understand this.

p1734:26: How can flushing cause surface ablation?

1734:17-1735:7: Unclear how this ties in to the rest of the description of this section. Seems unneeded.

1735:18-19: "melt-water film" - what is this?

1735:22: "solid fraction" -> "solid fraction of the top ice later" or something like that.

1736:4: "figure" - which figure? State explicitly

1736:11: "compacting" - does the ice really increase density while conserving mass? Is this observed in nature?

1738:4-5: Justify the assumption that the ice flaw distance grows linearly with ice thickness, or demonstrate that the model is not strongly influenced by this assumption.

1740:6-9: Dont understand this

1740:20: "a fully liquid layer is impossible" - in the model or reality? Isn't this just a meltpond if the top layer melts? And dont rotten ice layers exist in antarctic ice?

1737:19-1739:22: Why has the direct flushing of ponds down Macroscopic holes as seen by Polashenski et al 2012 been neglected?

1740:2-4: Justify this assumption

1740:5-14: This section is confusing and needs rewriting

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1740:12: "is then applied the next" -> "is then applied to the next"

1740:16: "flooding can occur when snow" -> "flooding can occur when the weight of snow"

1741:15-20: This seems inconsistent. You say the flow is not through the mush but then use the mush permeability to determine the flow rate. Or is this another permeability, maybe of the cracks? If so state.

1743:5-10: Give some justification for this profile.

1743:21-24: "To keep...Notz (2013)" Dont understand this.

1744: Give actual compute times for the various methods if you are going to discuss numerical efficiency.

1744:8-10: This is routinely done with global climate models running BGC components. Incremental remapping handles increases in tracer number efficiency. Generally I find the discussion of computational cost and especially code complexity poor in this paper. It is generally poorly explained and does not fit into the rest of the paper particularly. I would recommend removing it.

1744:22: "bottle-neck": either do a proper job of talking about the efficiency aspects of sea-ice models or cut it out. For instance fully explain the various bottle necks you talk about in sea ice modelling.

1744:24-:Remove the sections on code complexity. It is up to the reader to decide whether they want to implement the scheme in their climate model. I doubt any of these schemes if carefully writtem with correct code conventions in a well thought through climate modeling framework would significantly add to the complexity of a full earth system model. Since no actual numbers of code length of these parameterizations is given the reader must just take the word of the authors that they are too complex!

1745:19: 15Wm⁻² and 380Wm⁻²: Justify these values

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1745:24: "a strong flushing signal" - why did you not just use typical field conditions?

1746:5-8: Is this a flaw in the model then or a physical thing?

1746:23: "meltwater to accumulate" - describe the mechanism

1746:26: Show a figure of the ratio

1747:1: "does not reach an equilibrium" - in what sense?

1747:11: "liquid reduces" -> "liquid fraction reduces" presumably

1747:27: "which we trust" - demonstrate this

1748:3-5: Run this experiment with snow and see whether this is true

1748:section "Free parameter β " - state what a value of β means in terms of crack spacing versus ice thickness. Presumably with $\beta = 1$, crack spacing = 4 * ice thickness

1748:13: "the higher β is the weaker" -> "the higher β , is the weaker"

1748:

1748:25: "manifold" - use clearer language.

1749:2-3: "changes...flow separation" - don't understand this. Expand fully. Ladder circuits etc are unfamiliar in the sea-ice literature so fully expand and explain this.

1749:4-7: Demonstrate convergence (it's only a 1D model!) or state that the model does not converge. If convergence is achieved state that the oscillations are still present. If convergence is not achieved make an attempt to determine why.

1749:12: "fluxes vs. shallower" - "fluxes to shallower"

1751:3-4: What function of time is the oceanic heat flux.

1755:16: "lead to a zero salinity" - When? Not always zero at the surface in fig. 11.

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1755:17: "it is likely that" - state why.

1755:19-29: So, would it be better to explicitly track the slush layer. Why is this not done?

1755:25-29: How can the surface layer become truly zero? Wouldn't the permeability decrease as the salinity asymptotes to zero preventing it from ever becoming zero?

1756:2: "high surface salinity" - contradicts earlier statement that it had zero salinity.

1756:3-8: Run at higher resolution to determine whether it is due to a numerical artifact or wicking. If this can't be done then is this a flaw with the semi-adaptive grid.

1756:10: "change from" -> "change significantly from"

1756:11: "non-ideal" - how is it non-ideal

1756:17: "less strong" -> "weaker"

1756:19: describe how the neglect of frazil or pancakes could cause this.

1756:23-24: "unknown yet relevant...ice-snow interface" - couldn't your model be wrong?

1754:9-1756:26: It is unclear from where the model forcing data comes from for this section. Since you compare to Barrow ice cores you should use Barrow atmospheric forcing data if you already don't.

1757:28: "non-trivial result" - this is for the reader to decide not to be told by the authors.

1758:4: How does the Schwarzacher profile compare to the Schwarzacher data set?

1758:18: "and Barrow" -> "and from Barrow"

1758:27-1759:2: "From comparison...Arctic sea-ice" - What does this mean? Haven't you already been using it to study arctic sea ice. Some what meaningless statement.

1759:9: "between 0 and 1" - You mean the depth was normalized?

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1760:3:"ice have a" -> "ice has"

1760:18-19:"is visible...normalized profile" - how so?

1764:3-11: Prognostic salinity is also needed for sea-ice BGC, but this isnt mentionned. It is up to the reader to decide whether to use the authors parameterization not to be told by them. As mentionned previously the authors havent even given model timings or a qunatative estimation of code complexity but we are expected to just trust them even though they do not know what application we might use their model for!

1766:3-10: As stated previously the discussion of numerical speed and code complexity is poor. It should be removed or hard numbers given and the reader decide if they want to use it. They can also only state that the reader shouldn't use their progostic salinity model - they really cant state that the reader shouldnt use any prognostic salinity model!

1769:1-4: Remove section on computational cost/code complexity.

Fig 1-2 - Make these clearer.

Fig 6,7: The colour bar is very poor with very little variation across the range. Change to one which more clearly shows the chnage in quantity. (e.g -3 -> -1 deg C almost same colour, 5-10 ppt almost same colour!)

Fig 16c: the results appear bi-modal between 0.2 and 0.4 ice fraction. Comment on this in the text.

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