Review of: A daily basin-wide sea ice thickness retrieval methodology: Stefan's Law Integrated Conducted Energy (SLICE)

1 Synopsis

The authors present a method of modelling thermodynamic sea ice growth based on the temperature of the snow-ice interface and assumptions involving the latent heat of fusion (SLICE). This was an original and interesting project, but I question whether it has reached the necessary level of completion to be published in The Cryosphere. In particular, I don't think the authors showed that their 'retrievals' (which are not made available) outperform a popular model (for which the data is publicly available): I believe this would affect the impact of the paper were it to be published.

I also take issue with the framing of SLICE as a 'retrieval', when I would argue it is more a model-output. Furthermore, I question the assertion that this exercise can be extended back in time in a useful sense, given that it requires initialisation with a separate product in a way that may not be practically possible pre-2000 as suggested. With regard to this, I also found it slightly strange that the authors stressed the near-term potential to improve the product by extending it back in time and including dynamical/advection based thickening, but didn't present either. Finally, I was concerned with the relatively undocumented application of a 5 K offset to the snow-ice interface temperatures, which are the fundamental data set underpinning the exercise.

Based on the above comments, I believe that the manuscript requires some major revisions and additions prior to any publication. Given the salience of the PIOMAS model within the paper, I strongly suggest the revisions include a comparison of the performance of SLICE against PIOMAS, explicitly evaluated against observations from a satellite product (or OIB). I believe the sea ice thickness community will only use this method where they can be clear about whether, when and where SLICE out-/under-performs PIOMAS. Should the editor move forward after receiving major revisions, I would like to review the manuscript again prior to publication.

2 Significant Comments

2.1 Assessing the performance of a new sea ice thickness product

Two key benchmarks for a new sea ice thickness product are (a) the degree to which the product outperforms a climatology, and (b) whether and in what ways the product outperforms comparable products - in this case probably PIOMAS.

To address point (a), I think the authors could do some fairly straightforward things. The first is to take the first two columns of Table 2 and correlate the anomalies. This would reveal whether SLICE is thicker when the sea ice is observed to be thicker. To put this another way - to what degree does SLICE capture the sign and size of volume anomalies from the mean state/ climatology. You could even break it down regionally into where it does & doesn't show skill.

To address (b) I think the users need to find a unique selling point over PIOMAS. To have impact, this product/method will need to be preferable in at least one way, either in availability or skill. The way I see it, PIOMAS has clear availability advantages: it is open-access so anybody can use it, and it stretches back to 1979. As for skill, it has the advantage of including both the dynamical and thermodynamical components of thickening. On the other hand, SLICE is based on fairly direct observations of the snow-ice interface temperature, whereas PIOMAS has to work this out by first modelling the snow and then calculating the temperature gradient across the snow and ice. To show that this translates into an actual skill advantage (and so to get people to use the method/product), I think the authors need to show that SLICE outperforms PIOMAS in some way, place or time. This is particularly the case because SLICE needs to be intialised by a sea ice thickness data set anyway, which may well be PIOMAS. The best way to assess skill would be by benchmarking against (i.e. assuming as truth) some satellite-altimeter derived ice thickness product, or by combining the work they've done with other in-situ products like Operation Ice Bridge or ULS buoys. Without doing this, I don't think the SLICE-derived ice thickness will be greatly used by the community over PIOMAS.

2.2 Retrieval vs modelling

The term 'retrieval' touches on an emerging issue in the sea ice community concerning what properties we model, and what properties we observe/retrieve. For variables such as sea ice height, there is clearly a spectrum from direct observations (e.g. spot heights from a satellite-mounted laser altimeter) to highly-modelled (e.g. sea ice height output from a CMIP6-class model). Other quantities (e.g. radar-derived sea ice thickness from CryoSat-2, as used in this paper) are synthesised from observations (the timing and waveforms of scattered radar energy) and simple models (hydrostatic equilibrium, radar pulse propagation through snow, etc.). The case in this paper is similarly subjective: on one hand the authors are using observations of brightness temperatures, and what I would say is an observation of the snow-ice temperature. But then they're using a highly-idealised model of latent-heat release and heat flow known as Stefan's Law (which is in many ways is not a law but a series of combined thermodynamic assumptions, which are arguably outdated - see below).

Although I am certain this is not the intention of the authors, I fear that describing these sea ice thickness data as 'retrievals' implies a degree of direct observation that is too strong. I think that this implication may, at worst, lead to users (i.e. those wishing to initialise models) thinking that these data are more certain than they are, and more directly observed than they are. We regularly see this phenomenon with PIOMAS data for instance, which is very much a model but is treated by some as if it were observed because it is fed by reanalysis products. I therefore suggest that the authors be more explicit that they are modelling ice growth, and accumulating the results of that modelling exercise to model total ice thickness. On this basis I also urge them to remove the term 'retrieval' from their title.

2.3 Long Term Applications

The authors state that this method could be deployed several decades into the past. For instance, they do this in both their abstract and penultimate sentence. Their justification for this is that the snow-ice temperature is retrievable back to 1987, but I think that reconstruction back to this date is not usefully possible because initialisation is not available. It seems to me that the only way of doing this would be to initialise the product with an already existing and probably more accurate pan-Arctic sea ice thickness product. If this already exists, what would be the benefit of having this product, that would be dependent on (i.e. initialised by) the superior product? As a side point, I also fear that the authors' 5 K bias correction may not be relevant pre-2000, given than the roughness and snow depth of sea ice has declined, among other geophysical changes.

2.4 5 K bias Correction

On L82 the authors mention that they have performed a 5K bias correction on the Snow-Ice temperature data to make it match the buoy data. It's possible that they didn't do this themselves, but took it from a paper - but if so they should cite it. They certainly need to say whether they've added or subtracted the value. But this seems to be a pretty critical point that is not explored nearly enough. How did they get to this number? How sensitive is it to the data from individual buoys? How much did it improve the match between S-I temperature and the buoys? It also concerns me that they say they've done this 'to produce the best sea ice thickness retrievals'. Evaluated against what? If SIT data at the buoys has been used to tune or train the method, it casts doubt on the whole buoy-based evaluation exercise. The veracity and role of this correction must be quantified prior to publication, and its impact on the validity of the evaluation must be assessed.

2.5 Ice-Ocean Boundary Conditions

Seawater is not always in local thermal equilibrium with the sea ice interface (Schmidt et al., 2004; Mcphee, 2016). I'm not an expert on this, but it's relevant because this paper assumes equilibrium. For instance (as reported in McPhee), Maykut et al. (1971) found that without a steady basal flux of about $2\mathrm{Wm}^{-1}$, ice continued to grow unrealistically large in their model. Indeed Parkinson and Washington (1979) had to use a flux of an order of magnitude higher than this in their model. McPhee reports that observations from Sheba and Aidjex back these model fluxes up. This is clearly something the authors should address, perhaps with a sensitivity analysis to ocean-ice heat flux (which they say they've set to zero). If the 5 K offset discussed earlier was deployed to reduce modelled ice growth, perhaps the authors should consider that it is not the snow-ice temperatures being too low that are causing it, but an underestimation of ocean-ice heat flux?

2.6 Sea ice is a mushy layer

Sea ice is a mushy layer (Feltham et al., 2006) and this should be addressed when discussing heat flow through sea ice and accretion of new ice. Recently formed sea ice has brine inclusions, the phase equilibrium of which alters the bulk thermodynamic properties of the ice even well below the freezing temperature of seawater. Just stating what values you're using for the sea ice geophysical properties (L219) is insufficient. At minimum the values should be cited, and ideally they should be justified based on other previous modelling applications of the values. The constancy (as a function of temperature) of these values should also be considered. I'm not suggesting a multi-phase model of ice as I see that would make the whole situation very complicated and probably non-analytically soluble - the strength of SLICE is its simplicity. However, when presenting a model for ice growth based on heat flow through and phase change in ice near the freezing point, the mushy, mixed-phase characteristics of sea ice should be at least mentioned, and probably discussed.

2.7 Data and code availability

I was disappointed that the code and data used in this project were not made available to either the reviewers or the sea ice community. This is particularly the case given how much the authors have used other open data such as the CS2-SMOS and PIOMAS sea ice thickness data sets. To support this view, It's perhaps useful to refer to the data policy of this journal:

> The output of research is not only journal articles but also data sets, model code, samples, etc. Only the entire network of interconnected information can guarantee integrity, transparency, reuse, and reproducibility of scientific findings. Moreover, all of these resources provide great additional value in their own right. Hence, it is particularly important that data and other information underpinning the research findings are "findable, accessible, interoperable, and reusable" (FAIR) not only for humans but also for machines.

I would recommend that upon resubmission they make their code available on a site such as GitHub, and produce a persistent identifier such as a DOI. I also suggest they place their data product in a persistent archive such as that run by Zenodo, for which they will receive a DOI and the opportunity to reversion the data upon article acceptance. In taking the above steps, I believe the authors will significantly increase the impact of their research.

3 Other Comments

L2: 'Coupling'. I feel that 'coupled' systems/equations generally exchange information with and influence each other. However it seems that in this case you're feeding satellite information on the snow-ice interface temperature to an equation which tells you the growth rate. The satellite algorithm is not dependent on Eq. 7. So I think you should avoid portraying this as a coupled system; perhaps something like 'linking', or 'feeding'?

L34: I think "is also effective" is subjective and should be changed. Perhaps "is also popular"?.

L46: I think the word 'promising' is subjective and should be removed.

L63: Should be polarization, not polarity I think?

L113: "Obvious dynamic effects" - what does this mean? I think you need to be clearer in this paper between dynamic thickening in a Lagrangian sense (i.e. convergence driven ridging and rafting of ice to make it thicker), and dynamical thickening in an Eulerian sense (advection of thicker ice into and thinner ice out of a grid cell).

L127: The snow loading is used before the hydrostatic conversion, in the calculation of the height of the ice surface above the waterline to account for the delay in radar propagation through the snow (e.g. Mallett et al., 2020).

L129 CPOM is not affiliated with ESA

L142: Complementing, not complimenting

L150: It's noticeable that the grid on which data are supplied and applied is consistently described up until the PIOMAS description. This is perhaps the most important data set for which to mention this, because the native grid is very unusual. Worth describing or not describing the grids consistently.

L360: Antarctic sea ice floes often have negative freeboards so you probably won't retrieve get the snow-ice interface temperature. Some floes have had them in the past leading to the formation of snow-ice, and ice lenses also exist in the snow, which I imagine will significantly complicate the retrieval of the snow-ice interface temperature. Indeed the potential for negative freeboards in the Arctic (Merkouriadi et al., 2020) should perhaps be mentioned at some point.

L374: It's my opinion that you'll only be 'retrieving' sea ice thickness when you do actually account for both thermodynamic and dynamic/advective contributions to sea ice thickness at a point. Right now I'd say you're modelling one part of it.

3.1 Figures and Tables

The map projections used in this paper were unusual and not well-suited to the data being displayed. They look a bit like a Near-Sided Perspective projection? In any case, I think a more traditional North-Polar-Stereographic or Lambert Azimuthal-Equal-Area projection would be better. It looks in this case like data nearer the pole is being over-represented in area, and it's concerning that Hudson and Baffin Bay are hidden and highly distorted respectively.

I also think a figure should be displayed complementing Table 1 (perhaps put in a supplement?) with the tracks of the buoys used to evaluate SLICE. This would give the reader a better sense of the geographic/spatial validity of the buoy-based evaluation presented.

Figure 1: The colorbar should be labelled with the variable (S-I Temp), and units (Kelvin) should be stated.

Figure 5: The blue/white plots aren't providing much narrative value here. They're similar in appearance and concept to Fig 4, and the panels often look very similar to each other; I would suggest putting them in a supplement and increasing the size of the difference plots, which are much more relevant and important.

Table 2: I think put this in a supplement and display the data as a timeseries. You could put the Vol. Growth in first two columns on the Y axis and the relative difference in % on a secondary Y axis. I'm not convinced the column with absolute differences adds much value. I think displaying this data as a graph

would give the reader a much better feel for what's going on.

Figure 6: Again, enlarge and focus on the difference plots and put the blue/white plots in a supplement.

Table 3: Same comment as Table 2, and you could probably merge the resulting figures.

References

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