Paper Review

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A monthly Arctic sea ice thickness product from 1995 to 2023 using multiple radar altimetry data

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General comment

This paper presents a new time series of Arctic sea ice thickness over nearly 30 years based on altimetry observations with an unprecedented resolution of 5km. This is an important subject because only one other series of this length exists to date, and it may help to confirm the initial results and the relevance of this type of monitoring of the state of the sea ice. The comparison is even more interesting as the method used is new and totally independent of the previous ones. It relies on the multiplication of altimetric freeboard measurements in each 5km x 5km grid cell, to determine the thickness of the ice without preconceived ideas about parameters such as density or snow depth. For these reasons, I believe that this work deserves to be published.

Nevertheless, some important concepts have been overlooked, leading to dubious interpretations of certain results. In particular, the authors do not distinguish between the freeboard measured by Ku-band radar altimeters and the freeboard of the ice, which leads to erroneous equations that mainly affect the validation part. Because snow reduces the speed of propagation of the radar wave, the radar freeboard is necessarily smaller than the ice freeboard, and these 2 freeboards cannot be compared directly. The comparisons with OIB freeboard Figure 3 are therefore questionable. For the comparisons with ULS the used ice density is not specified. No correlation is calculated when comparing with other solutions or with in-situ measurements.

While ERS2 is a hard point because of the problem of blurring of its waveforms, no resulting map is shown. Although the data supplied with the paper seems to give consistent results, it would be useful to present comparisons with Envisat over a common month and to discuss any problems encountered, such as the extent of filtering used (Pauta criterion). Also, very strangely, the quality of the results deteriorates sharply, with extremely moth-eaten maps for the Envisat period from October 2002, even though the Envisat measurements are of much better quality than those of ERS2 and their orbits are the same. Coverage became good again with CryoSat-2 from November 2010, but visually the resolution seemed closer to 25km than 5km. This seems to indicate that you forgot to apply the coarse resolution to Envisat, but also that the 5km resolution is far too low to cover the Arctic basin.

All this raises questions about the choice of such a small resolution (5km), compensated by a relatively coarse resolution (25km). For a future study, wouldn't a resolution of

12.5km be more appropriate? Or do you think that 5km really adds value (relevant signal at this scale)?
Finally I would recommend analysing and exporting in the data the parameters a5 and a6, which depend on the densities and the snow depth. This should allow evaluating the consistency of these parameters, and thus the model used.

From a practical viewpoint I would recommend to use the Lambert-Azimuthal-Equal-Area projection with lon0=0, which becomes the reference in our domain, but it's not mandatory as it represents a lot of work without changing the results (see EASE v2 in https://nsidc.org/data/user-resources/help-center/guide-ease-grids). Strangely, while the figures in the paper show maps with lon0=0, the data are centred on lon0=-45.

Following all these considerations I recommend a major revision of this paper.

S.Fleury

Detailed comments

l.24 : The only mention of the projection used. Should be specify within the paper with all the requested parameters to use this projection, in particular the True Latitude (lat_ts). Also we would recommend to use the EASE v2 https://nsidc.org/data/user-resources/help-center/guide-ease-grids, which becomes the reference and which is much more convenient as the resulting grid is a regular Cartesian grid in meters and allows to compute directly distances, surfaces and volumes in ISU, which is not the case for the stereopolar projection.

l.46: please specify that the Arctic could become ice free in less than a decade in summer

l.95: I was surprised by these snow depth reduction announced over FYI and MYI but in fact in Webster et al. 2014, they do not speak about FYI/MYI but about specific seas: *"For the 2009–2013 period, the products show that snow has decreased by 37 ± 29% in the western Arctic and by 56 ± 33% in the Beaufort and Chukchi seas, compared to the 1954–1991 snow depth climatology produced by W99."* Please correct it or provide the original citation if any.

l.148: I suppose there is an error in this sentence: *"coarse across-track spacing of 25 km at 75° and 4 km at 60° provided by ERS-2 and Envisat"* as the across track spacing decreases with the latitude.

l.152: CryoSat-2 orbit is not any more a repeat cycle since several years, however there is still a sub-cycle of about 30 days.

l.185,248,270,285,286,293,294: It's very pertinent to recall the uncertainties for each product. I would just recommend to always use the same units (meters or centimetres).

l.195: CryoSat-2 ICE baseline-E L1B is the official ESA product, not an AWI product. Please specify if you use the AWI product or the ESA product.

l.238: This long time series is described in Bocquet et al. 2024 https://doi.org/10.

1029/2023JC020848 . Bocquet et al. 2023 explains the methodology and is only over Arctic from 1995 to 2021 (without ERS1).

l.249: "RMSE of 12-28 cm for Envisat period and 15-21 cm for CryoSat-2 period (Guerreiro et al., 2017)." Guerreiro or Bocquet? It's not the same time series.

l.300: DTU18MSS is endowed with discontinuity problems close to the MIZ and you should use DTU21MSS. See: "The DTU21 global mean sea surface and first evaluation" in Earth System Science Data 15(9):4065-4075 10.5194/essd-15-4065-2023

l.366: The MSS includes the geoid (MSS=geoid+MDT). Also I'm surprise that you don't correct for the usual altimetry corrections such as the wet tropo, the dry tropo, the ocean tide, load tide, pole tide, DAC, etc. You would get even more flat measurements.

l.367: I did not know about Pauta criterion, I thing it would be interesting to explain it here in few words.

l.375: *interpolated* is between 2 measures, here I would say *extrapolated*.

l.380-388: this part is not clear at all because you mixed-up the ice-freeboard (FBi, the real freeboard of the ice), the radar-ku-freeboard (FBku, the freeboard measured by the radar) and the total-freeboard (FBt, ice+snow freeboard that is measured by the lidar of OIB or IceSat-2). They are linked by the following relations: FBt = FBi + SD and FBku=FBi- $(c_v/c_s - 1) * SD$, where SD is the Snow Depth and cv/cs is the ratio of the speed of light in vaccum and in snow. This ratio depends on the snow density ρ_{snow}. From Ulaby 1986 we have : $c_v/c_s = (1 + 0.00051 * \rho_{snow})^{1.5}$ *(Tiuri et al. 1984 suggest:* $c_v/c_s = (1 + 1.7\,\rho_{snow} + 0.7\,\rho^2_{snow})^{0.5}$ *).* Each time you speak about freeboard you must specify which freeboard you are speaking about:

l.381: *"The OIB* ***total (or lidar)*** *freeboard was modified with snow depth".* Here you should also specify if you have just removed the snow depth to get an **ice freeboard** or if you also have corrected for the speed propagation to get a **radar-ku-freeboard** for the following comparisons.

l.382: *"The mean* **radar? ice? yours from OIB? yours from satellite?** *freeboard along this track in this study"*

l.383: *"while the mean* **radar** *freeboard from the Baseline E" .*

l.384: *"The mean value of the modified OIB freeboard was 0.261" -> The mean value of the* ***ice? radar?*** *freeboard obtained from OIB was 0.261m.*

l.387: The following sentence is wrong: *"the waveform threshold method leads to an underestimation of the freeboard ...".* The threshold method some times over estimates the FB and sometimes underestimates it as you show it later on in this paper. It mainly depends on the roughness of the ice, i.e. on ice type (FYI/MYI).

"… which explains why the freeboard in the Baseline E product was smaller than our estimates and the modified OIB freeboard." No, this is explained by the following equation FBku=FBi- (c_v/c_s - 1)*SD, which shows that the **radar freeboard is always smaller than the ice freeboard** and it can even be negative for small FBice and large SD.

l.406: This equation is not the equation of the hydrostatic equilibrium between FBi, SIT and SD!

Here it is: FBi = SIT(1- ρ_{ice}/ ρ_{water}) - SD * ρ_{snow} / ρ_{water}

What you have written is the equation that links FBku with SIT and SD (ie, what you call h_{fb} is in fact FBku and what you call theta is cv/cs including possibly a penetration factor P*cv/cs).

l.407-409: Please specify which are the inputs and which are the unknown. It looks like that h_{fb}, x and y are the inputs and a0-a7 and h_{si} are the 8 unknown, right?

l.410: x and y are really lat and lon? It's strange with your grid projection. Using EASE2 they could be directly in meters ;-)

l.418: I suppose it's 25km here, not *10km*. as you have just computed the 25km x 25km grids. However,, when you look at the data supplied, the maps appear very patchy. Can you explain this? (not enough measurements, even at 25km resolution?).

l.426: typo: *antimeres* -> altimeters

l.427*: "The pulse-limited altimeters have a large footprint of 2–10 km"* radius or diameter ?

l.441: typo: *Bocquest* -> Bocquet

l.442: *"for calibrating freeboard measurements from Envisat and ERS-2."* -> for calibrating Envisat freeboard measurements from CryoSat-2 and ERS-2 from calibrated Envisat.

l.443 : *"Tilling et al. (2019) developed a physical-based approach to correct Envisat SIT …"* Could be confusing with the retracker physical-based approach and it's not more physical than considering the ice roughness as it is usually done, I would avoid this term.

l.453: I do not agree with this conclusion :*"Compared with CryoSat-2 thickness, Envisat thickness showed an overestimation of 0.19 ± 0.67 m in January 2011*." As it is shown in your maps and histograms, LRM gets thinner ice over thin ice and thicker over thick ice relatively to SAR as it was explained in Laforge et al 2021 https://doi.org/10.1016/j.asr.2020.02.001

l.473, Table 5: Is 'Mean' the Mean Bias ? Is STD the STD of the difference or of the product? Would be very pertinent to add the Correlations.

l.480: The main problem with ERS-1 and ERS-2 is related to the blurring of their waveforms over sea ice. You don't mention it and it looks like that you don't have

applied specific correction for this problem. Any other calibration between ERS2 and Envisat, as for Envisat versus CryoSat2? As it is an important problem it would also be important to see the maps you obtain in front of Envisat map for the same period. And once again the correlation is an important criteria that should also be added.

l.484: typo: *gird* -> grid

l.487, caption Fig 8: what is kermesinus ?

l.497: *"The sea ice extent did not show any significant changes during this growth."* How do you determine the sea ice extent? It's from NSIDC-0051 with concentration>75%? Would worth to recall it here. It is a very important point as the mean SIT highly depends on it (if you consider or not the thin ice in MIZ).

l.505: what do you mean by "normal distribution"?

l.510: what do you mean by "sinistrality"?

l.535: *"The mean MYI thickness decreased by 0.017 m/yr during the research period,".* Please provide explicitly the period.

l.538-570: I'm not fully convinced of the interest of mean SIT for all the Arctic as this value mainly depends on the considered sea ice extent. For instance it can remains about no ice but only some remaining fast ice at the coast to obtain large mean SIT but it means nothing. To make this type of comparison meaningful you could for instance always consider the same mask (region) for each given month. Or an alternative would be to compute to total volume instead of the mean SIT. However I will not ask you to change this, but at least you should explain specifically how you define the mask, for instance do you always use the area provided by NSIDC-0051 with concentration>75% for each month of each year?

l.574: Please specify if you use the same mask for all the products. It's important to make them comparable.

l.582: The information of the mask is even more important for the CS2SMOS product because it can cover larger region as it also considers thin ice at the margins thanks to SMOS.

l.592, Table 6: The mean bias and the correlation should be added.

l.598: it's really important to know which ice density and which snow depth you have chosen to convert the draft to SIT. If these values are not coherent with the product you compare you will necessarily get higher differences for this product. Please also provide the used equation and the input parameters (mainly ice density, SD is based on WC or MWC?).

l.606: typo: *"The STDs of WHU were close …"* -> *"The STDs of WHU **are** close …"*

l.618-620, Tables 7 & 8: Please add the Mean Bias and the Correlation in these tables.

Indeed, if there is a significant bias, both the MAE and the STD will be high, but if the correlation is good it will indicate that the tendencies are coherent, which is the most important point to study change rate. (Also it is not necessary to recall the units in each column ;-).

l.624: "*Then, the mean thickness of the OIB within the grid was compared with the corresponding grid values.*" OIB products do not include SIT, how do you compute it? Please provide the equation and the input parameters used from OIB data.

l.636, Figure 16: For some products the count reaches nearly 1500 and for others it is lower than 300. The shape of the histograms being similar, it means that the number of measurements from one product to another can differ by a factor 3. How can you explain it? Is it because of the resolution of the original product?

l.643, Table 9: Please add the correlations.

l.647: *"… error propagation of the input uncertainties including* ***radar*** *freeboard, ice density, snow depth …"*

l.655: "*Thus, the uncertainties of the SIT can be calculated by the difference of hsi in the last two iterations*.". I don't understand why the last two iterations are more relevant than the previous ones. To me, this is more a reflection of the speed of convergence of the LSA than the uncertainties. Please justify this solution. For example, you could more naturally assess the distance between the model and the measurements by calculating the STD or MAE between the model and the measurements.

l.678: Please replace the link https://www.legos.omp.eu/ctoh/fr/produits-ctoh/ by a more direct one: http://dx.doi.org/10.6096/ctoh_sit_2023_01