

Interactive comment on “Sea ice in the Baltic Sea – revisiting BASIS ice, a historical data set covering the period 1960/1961–1978/1979” by U. Löptien and H. Dietze

U. Löptien and H. Dietze

uloeptien@geomar.de

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We thank reviewer 1 (R1) for his time, effort and encouraging comments. R1 agrees that facilitating the access to the BASIS data set is in itself excellent. R1 is, however, concerned that the data maybe taken at its face value even though there is considerable uncertainty associated with it. More specifically, R1 asks for a more systematic assessment of the reliability of the data. R1’s request involves two levels. Level 1 concerns the uncertainty of the original data set. Level 2 concerns uncertainty that was added by us during the process of deriving measures such as e.g. percentage of water covered with ice.

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Concerning level 1 (uncertainty of the original data set): We shared the sensation of R1 finding that the Udin et al. report leaves some questions unanswered. We tried to clarify the matters prior to publishing in ESSDD by searching for reports and by contacting personal that was involved back then. Our experience was that those who were involved have been retired in the meanwhile or work now on different tasks. Our major aim is to preserve at least the knowledge that is still accessible. The publication in ESSDD is intended as a resort to summarize the information we could gather and, via the ESSDD open discussion hub, to provide a platform for others to contribute information. That said: we will provide a more comprehensive discussion of uncertainties in the revised version of the manuscript.

Concerning level 2 (uncertainty that was added by us): As concerns ice thickness: we did not add any uncertainty (make any assumptions). This will be clarified in the revised version of the manuscript. As concerns our assumptions we made to derive other properties: we will add information regarding the spatial patterns of how often we applied each of our assumption in the revised version of the manuscript. This will give guidance on the question of how much uncertainty was introduced by us.

- R1: A strange omission is that the use of BASIS to prepare two climatological ice atlases is left unmentioned. This was after all the goal of the effort and the atlases have served as a basic reference of Baltic ice conditions. A chapter describing the atlases should be added, and the authors should compare their assumptions to get concentration with those used in the atlases.

-A: We will add a reference to the joint initiative of SMHI and FIMR (today FMI): "Climatological Ice Atlas for the Baltic Sea, Kattegat, Skagerrak and Lake Vanern (1963 -1979)". We lack, however, essential details for a direct comparison (e.g., the temporal inter- and extrapolation scheme).

As concerns the other climatological ice atlas that emerged from BASIS: we would be grateful for a specific hint where it can be obtained.

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Specific comments: - R1: 1. Discuss Baltic charting practices and make clear how the variables and types of the code relate to the hatchings and symbols of the charts, and what can the presence of different ice types in a cell mean (as there are only limited possibilities to describe mixtures of ice types as understood when eg. onboard ice observations are made, make clear that these are two different things). There are also differences between Swedish and Finnish chart symbols, how does this affect. Also the charting styles have changed – older charts have detail especially on the floes.

- A: We will describe the Baltic charting practices in the revised manuscript and highlight that an occurrence of more of two ice types is neglected, and only the two prevailing ice types are accounted for (cf. Page 425, line 3ff). In our derived product we do not distinguish consolidated ridged ice and non-consolidated ridged ice, further we do not consider any specific information about floe sizes. We will highlight this in the revised manuscript. Overall our major aim was to stick to more or less standard ice properties that might be of interest also for modelers.

- R1: 2. Consolidated ice is the same as compact pack ice (C 100%) so this can be treated as 'pack ice' and not as a separate type ('consolidated' may be misleading; terminology in the charts varies as well).

-A: We will adjust the description of this ice type.

- R1: 3. If there is only one type the concentration is probably mostly 100% (although this is an assumption as well). Discuss more the interpretation problems that may arise when there are several ice types, and check what the atlases have assumed. It would be good to have some statistics on the amounts of data with one/several ice types and an estimation of the errors that may arise from the adhoc assumptions. I have understood that the concentration given to a type refers to the type (ship point of view) and not to the relative coverage within the cell (modeling view, however I am not quite sure about these). Maybe it would be good systematically separate concentration (of an ice type) and the coverage of the type in a cell.

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-A; We agree and will add a statistics on the amount of data with one/several ice types. Also, we will highlight how often total concentrations were given explicitly, and how often we rely on assumptions.

- R1: 4. Ridged ice is normally superposed on the dominant ice type and rarely appears as own ice type (I assume as ridge triangles are modifier symbols in the charts), so if the ridged ice is secondary type so this does not change primary type concentration. Was this your interpretation also (very unclear in Udin et al, what is the meaning of consolidated/not consolidated ridged ice there?) or did you separate 'ridged' and 'nonridged'. Does the ridge concentration refer to the type, ice with ridges present, or the actual area from ice surface covered by ridges? Some cautious discussion is due as even the present state of art is hardly yet able to retrieve ridging quantitatively.

- A: True. Ridged ice, on it's own, is rare and might only occasionally occur as an own ice type in late spring (as it generally melts slower than the surrounding ice). We separate in our data set only "ridged" and "non-ridged" ice. The percentage of ridged ice is explicitly provided in BASIS and we attribute the remaining percentage to the second dominant ice type (cf. page 424, line 21ff). We will clarify this issue in the revised manuscript. We agree that the percentages given in BASIS are rather vague and uncertain, and will discuss this in the revised manuscript.

-R1: 5. Compact slush and shuga I assume refer to what is now called windrows or brash barriers, describe shortly these features.

-A: Slush and shuga is not identical with brash ice barriers or windrows. Slush is a mixture of small ice crystals, e.g. from snow, and liquid water (often a viscous floating mass in water after heavy snowfall), while shuga is an accumulation of spongy white lumps, a few centimeters across. We will add the respective descriptions.

- R1: 6. I do not quite understand where is the difficulty in interpreting the surface temperature code. Does not patching the shorter strings with zeros in the beginning do the trick?

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- A: Together with leading zeros, apparently also leading minus signs disappeared. We will clarify this.

- R1: 7. The ice thickness is undiscussed although it is the other basic variable. What is the percentage of existing thickness readings in the code? The ice charting practice of characterising large areas with rough thickness ranges should be expounded and the fact that only level ice thicknesses are described (not total thicknesses including deformed ice).

-A: We did not make any assumptions when deriving ice thicknesses, as they were explicitly coded in the original data set. We did not interpolate the missing values at the ice edge (cf. page 426, line 1ff). We agree that ice thickness should nevertheless be discussed in more detail. Among the information we aim to add is e.g. that ice thickness refers to level ice thickness and not to total ice thickness.

- R1: 8. Table 1 can be omitted as it only repeats Udin et al.

- A: As Udin et al. is gray literature we would like to keep the table included.

- R1: 9. Granskog et al, Sea ice in the Baltic Sea: A review would be a good additional reference in Intro.

-A: True. Thanks!

Technical corrections: - R1: The period of the BASIS in the abstract misses end year.

-A: True. Thanks!

Interactive comment on Earth Syst. Sci. Data Discuss., 7, 419, 2014.

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