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Interactive comment on "Evaluation of Sea-Ice Thickness from four reanalyses in the Antarctic Weddell Sea" *by* Qian Shi et al.

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In this study, Shi et al. evaluate sea ice thickness (SIT) in the Weddell Sea from four reanalyses of coupled ocean and sea ice models, against two in-situ observations and two remote sensing datasets. Their results show that these reanalyses have limited success compared with the observations, and they stress the importance of sea ice motion and deformation on the SIT simulations. Modeling and observations of the Antarctic SIT, even for a single region such as the Weddell Sea, are very challenging. This study provides useful insights not only in the SIT reanalyses but also in the SIT observations, which will be a benefit to the sea ice research community. The Introduction, and Data and methods are well written. However, the Results part needs significant improvement. I suggest the manuscript accepted after major revision.

General Comments:

1. There is inconsistency during the comparison in terms of the data. In the "Data and methods" part, the authors state "Comparison are made using monthly means", however, when in 3.3 Comparison with sea-ice thickness from ICESat-1, they are using seasonal mean. This inconsistency must be fixed. It will be much better that the authors describe how they make the comparison in the exact sections.

2. Section 3.1. It remains unclear what kind of mooring data are using here. According to the statement "The aggregate temporal span of ULS observations in AP, CWS, SC and EWS is 148, 79, 185 and 272 months", and consider the numbers of the mooring in these regions, there should be large difference in the mooring data regarding the time duration. I suggest the authors add a plot in their Figure 1 showing the temporal evolution of the mooring observed SIT that are actually used in their comparisons.

3. Figure 4. Not sure why the authors use SITs from different locations in the three reanalyses (Figs. 4b-d). This means they also use different ASPeCT SIT when comparing with the different reanalyses. What can we infer from such different comparisons? I suggest the authors use a consistent comparison: Use the same ASPeCT SIT, with reanalysis SITs interpolated to the same time and same location.

4. Section 3.3. It is not clear what kind of manipulations here for the reanalysis SIT. The authors state "we use October and November to represent spring . . .". However, according to Table 2, the ICESat-1 measurement is irregular, and no full month measurements. Do the authors use the same dates as the observations, or just simply use the full two-month reanalysis data? As the authors here try to compare the mean, it is very important to compare the exact corresponding data in terms of time and locations. Also the authors need give a test with confidence level for the comparison.

5. Line 280-281. "Compared with ICESat-1, only NEMO-EnKF has a similar variation of modal ice thickness from Autumn-FM to Spring-ON, while GECCO2, SOSE and GIOMAS have monotonically increasing model ice thickness". It seems to me 2005 &

2006 for SOSE, and 2006 for GIOMAS have similar seasonal variations in Figure 5. Table 4 looks somewhat misleading as its modal SITs not necessary in the same year.

Specific Comments:

1. Line 40. Not clear. Rephrase it.

2. Line 62. "... it is also reported to have uncertainties due to ...". All observations have uncertainties. Perhaps more important to note what kind of uncertainty it is.

3. Line 104. "all available daily records around specified model grids are averaged monthly". How far away from that model grid?

4. Lines 110 & 114. I think the adjoint method and 4-D Var method are the same here. Can the authors give a brief of their differences?

5. Line 195. "Figure 1b", better remove "b".

6. Line 217. "It is noted that the relatively short ICESat-1 record (13 months) limits the accuracy of this assessment". Not sure how the authors arrive to this statement.

7. Line 233. "Envisat has the lowest CC (-0.19) and highest RMSE (2.06 m) among all data sets, and its STD is comparable with GIOMAS". These numbers are almost unbelievable. The authors have any explanation of this? Is there any reference supporting similar findings? Or is there some error in the manipulation of the data?

8. Line 238. "But in the regions with large amounts of newly formed ice (the central Weddell Sea and the eastern Weddell Sea), SOSE tends to underestimate sea-ice thickness with lower STDs than the other reanalyses". It looks to me no data here to support that SOSE underestimates SIT.

9. Line 331. "Figure 7" should be "Figure 8"?

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