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The Cryosphere Discussions
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Interactive comment on "New insight from CryoSat-2 sea ice thickness for sea ice modelling" *by* David Schröder et al.

Anonymous Referee #1

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General comments:

GC1: The authors assume that CS2 estimates are unbiased towards thicker sea ice and superior to CICE in general, and that there is a negative sea ice thickness bias in CICE estimates. Given the substantial uncertainties in CS2 estimates, it would be better to phrase comparisons with ambiguity regarding which is more accurate: e.g. "Comparing the CICE simulation with CS2 reveals that CICE default underestimates the mean monthly sea ice thickness" would become "Comparing the CICE simulation with CS2 reveals that CICE default produces thinner mean monthly sea ice". Furthermore, given the importance of CS2 estimates, its uncertainty and bias should be quantitatively described, early in the paper, for each of the five sea ice thickness classes used. As shown in Tilling et al. (2018), CS2 biases for ice < 1.4 m are generally pos-

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itive, whereas for ice > 3.6 m biases are generally negative; and Ricker et al. (2017) provide an assessment of relative error by thickness. The uncertainties and biases, as a factor of ice thickness, should be more prominent in discussions of the results.

GC2: The region (grid cells) chosen for CICE - CS2 comparison is an area usually dominated by multiyear sea ice. This is evident in Figure 1 where the ice does not melt fully during the summer, indicating primarily multiyear sea ice. This is also evident in that the September model results for the grid cells show a mean ice thickness > 1.57 m (Table 4). Please describe the proportions of multiyear versus first-year sea ice that make up the comparison grid cells. If first-year sea ice constitutes a significant proportion, the vertical shift in the scattering horizon due to snow salinity (Nandan et al., 2017) could account for most of the 0.8 m CICE - CS2 difference observed. However, for multiyear sea ice with its non-saline snow this would not apply. If the analysis is primarily limited to multiyear sea ice, this should be reflected throughout the paper. Please include a figure showing the frequency distributions of ice thickness in November and in April.

GC3: The authors attempt to force the model to fit the CS2 estimates by making cumulative changes to the model that are biased towards thickening the sea ice. While these are reasonable for assessing which variables may be incorrectly parameterized, they are not really sensitivity studies; they are rather specific model perturbations intended to produce thicker sea ice. A terminology other than “sensitivity study” should be used.

GC4: When comparing the CICE-mw-form-e-sd and subsequent models with CS2 estimates, the snow distributions will be different, assuming W99 is used for CS2 estimates. W99 for the comparison grid cells in April is approximately 30 cm, whereas Figure 7b suggest values of half that thickness; the resulting CS2 sea ice thickness estimates would be substantially thicker. If the substantially thinner snow cover distribution (as per Figure 7b) were to be used for the CS2 estimates, then a direct comparison would be more valid.

Specific comments:

P2, L3: Please include a short description of CICE, with references.

P2, L20: Discrimination of sea ice type is also a principle challenge (in order to assign ice densities and snow loads); especially when the effects of snow salinity for first-year sea ice are also considered.

P2, L32: Can it be called a “full ice thickness distribution” if values < 0.5 m are not included?

P2, L35: Given that “A realistic ITD is critical for simulating ice growth and ice melt rates correctly”, an explanation should be included here on the impact of omitting thin ice points.

P3, L31: In order to compare the uncertainties of CS2 estimates with the uncertainties of CICE model results, please summarize previously reported (if any) uncertainties and biases, for sea ice thickness, of CICE-default.

P4, L3: See GC3.

P4, L25: Multiyear sea ice is mentioned, but please include a statement regarding the accuracy of snow on first-year sea ice.

P4, L26: In “Therefore, we apply a climatology for the CS2 period 2010 to 2017”, what is meant by “a climatology”? Is this W99 as per P2, L23?

P5, L10: The language used is too strong: e.g. “This indicates that the winter ice growth is underestimated in the model.” See GC1. Recommend: “This suggests that the winter ice growth may be underestimated in the model.”

P5, L10: If the CS2 ice thickness is overestimated when thinner and more-accurately estimated (or underestimated) when thicker (as per Tilling et al., 2018) then this may mitigate the CICE April “underestimation”.

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P5, L29: This statement is rather broad, given that only air temperature and incoming longwave were assessed, and then only for two specific perturbations. Please reword.

P6, L11 and Table 1: For CICE-mw-form-e, what is the justification for increasing the longwave emissivity of sea ice from 0.95 to 0.976.

Technical corrections:

Figure 1 caption: "(mean values for November to April vary between 99.4 and 998. %." Is missing a ")" and 998 is likely 99.8%.

Figure 3: Change North pole latitude to 90, or omit. In the caption, would not "grid cells" be more accurate than "region".

P3, L26: Define "SIT".

Table 1 caption: ". . .model changes. . ."

Table 1: surfaceaography = surface topography?

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