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Interactive comment on "Thin sea ice in the Arctic: comparing L-band radiometry retrievals with an ocean reanalysis" *by* Steffen Tietsche et al.

Anonymous Referee #2

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This manuscript presents a comparison of Arctic sea ice thickness within a range of 0-1 m, both retrieved from SMOS satellite-based L-band brightness temperatures and from a numerical ocean-sea ice reanalysis system assimilating various observational data. It focuses on evaluating regional biases between the two products during the winter 2011-2012 season, but also touches on interannual variations and trends across the full 2011-2016 period. The premise for the study, although unfocused, is valid. Numerical sea ice forecasting systems should unequivocally be more reliable if they can assimilate a greater breadth and variety of observational data, such as low-frequency passive microwave retrievals of ice geophysical properties like those provided by SMOS. Here, the authors appear to be undecided on the main purpose of their study: is the idea to verify/validate the ORAS5 forecasting system using the SMOS data? If so, given

the observational uncertainties discussed in the manuscript, the SMOS data do not appear ready for this. Moreover, why was the enhanced sea ice thickness product incorporating SMOS and Cryosat-2 data not utilized. Alternatively, is the idea to evaluate the root causes of biases within the SMOS data? In which case, this is mostly done qualitatively. Several possible reasons are introduced to explain uncertainties in the SMOS data, but none are investigated in detail so no useful conclusions are made. Given that the premise of validating numerical sea ice forecasting systems is highly valuable, I recommend this paper could be published following major revisions. In line with comments above, the authors should decide exactly what they want the paper to be, to allow them to focus their arguments into quantitative useful conclusions.

General Comments

âĂć Re. Section 2.1, do you have quantitative component uncertainties for each of the contributing factors listed here (e.g. uncertainty contribution from the smos Tb, from the ancilliary T and S data, from using assumptions for linear T-gradient, desalinization scheme etc.)? Are these provided in the SMOS product or can they be provided by the co-authors? In the context of the entire study this would be very useful, as it would allow the authors to better evaluate regional biases in the SMOS data and thus understand how likely identified bias is a product of the SMOS data or forecasting system. An example of where this would be useful is around page 7 line 32.

âĂć It would be valuable to include all or details from Appendix C in the main paper. This extra understanding of where and in what context the SMOS data could be limited would really help to interpret the validity of results from the forecasting system. This analysis could be expanded by examining scales of day-to-day variability between a fast-ice region (e.g. the CAA) and a dynamic region, over the same time period or scenario (like the author's rapid air T change). Equally, more depth to the analysis between ice concentration and smos ice thickness (also in the appendices) and on the effect of ancillary fields on the ice thickness retrievals would be incredibly valuable and relevant, even though the authors suggest this is beyond the scope of the paper.

âĂć Section 5 is quite vague and unfocused. The bulk of the paper would be more useful if this was removed and replaced with more detailed investigation of regional model-obs biases, investigating particular causes for the regional biases the authors touch upon in the previous section.

âĂć You mention at Page 10 line 6 that the smos ice thickness algorithm relies much more on ancillary fields when ice thickness >0.5m. It would therefore be useful to analyse model-obs biases for different categories of uncertainty or for different ice thickness categories. Is there a strong relationship between bias magnitude and smos-sit or uncertainty?

âĂć I do not agree with the statement there is 'reasonable agreement' between observed and analysed ice thickness in the early freezing period. There is systematic nonlinear bias, which has not been explained or properly quantified here.

âĂć To reiterate an earlier point, it is difficult to understand whether the idea of the paper is to verify/validate the reanalysis system (in which case it would have made more sense to use the combined cs-2/smos product from AWI and Hamburg http://data.seaiceportal.de/gallery/index_new.php?active-tab1=measurement&ice-type=thickness&satellite=CS&region=n&resolution=weekly&minYear=2017&minMonth=4&m tab2=thickness), or to verify/test smos (in which case it is diffcult to use a highly simplified model to do this).

Minor Comments

Page 1 Line 22, 'coverage at a'

P2 L18, requires more specific objectives for the study, beyond simply compare observations with model. What exactly are you trying to achieve here? What exactly will the study provide that is useful for future work?

P5 L28, is ORAS5 SIT <0.3 m impossible? In what situations do you get very thin ice? SIC very low? A 'freeze-up threshold' is referred to later on but should be explained

here.

P5 L34, you need to explain this non-linear dependence here or in the discussion. Clear dependence within the LIM2 ice redistribution function? Or from the single thickness class assumption? Or is this some bias introduced from SMOS?

P6 L23, this is a very qualitative description of the relationship. . . Can you explain?

P6 L30, where are they assimilated? Outside the ice edge presumably?

P7 L7, There is lower SIC in BB in April, so this could be caused by the SMOS-SIT assumption of total ice concentration within a grid cell? Tb is biased due to the emissivity of open water.

P7 L8, remove 'also' and add appropriate Tilling citation

P7 L22, this is likely owing to low SIC. Linked to the second major point above, some more involved analysis SMOS-SIT sensitivity and higher frequency emissivity/SIC would be very useful and may allow you to make much more robust arguments for causes of obs/model bias.

P8 L5, close to 100%, but not at it, whereas most other regions have total ice concentration. Another thing to consider is that sea ice in BB is fairly low latitude so could be melting some years in April and affecting the L-band penetration depth. What do the PMW data suggest in terms of melt onset date for BB in 2012? Crucially, do you observe this clear bias every year for BB?

P9 L14, change 'than' to 'then'

P10 L16, this would be a much stronger argument if you could provide reasonable evidence as to why this happens. Do you even see the same biases every year? Could you test the interannual persistence of your regional biases? Again this would be highly valuable to the community.

P10 L23, surely more relevant here is the need to improve the rheology and add formulations to the numerical scheme to allow for polynya development, rather than just assimilating observations and the model re-equilibrating to incorrect/overestimated ice thickness?

P12 L18, this is an important limitation that could have been examined in greater detail within the main paper.

P13 L 13, this is a very useful finding that could be represented better in the main paper and given as one of the paper's main conclusions.

Fig 2, explain what 'unc', 'sic' etc. mean within figure caption.

Fig 2, is it impossible to get forecast SIT below 0.3 m when SIC is low (i.e. when smos-sit is around 0)? Why?

Fig 3, does (c) show saturation in the smos-sit signal above approx.. 0.5 m? Plateaus above this value, so no sensitivity from L-band signal?

Fig 3, doing this for only one year's winter enhances the possibility for anomalous ice conditions to explain the departures between observed and predicted IT. What do these look like for multiple years? Your arguments would be more convincing if similar patterns of regional biases were found in several/all years.

Fig 4, Mark on a map either here or on fig 1. Adding a panel of SIC would be very useful for analysis.

Fig 4, 'added and subtracted' what? Uncertainty?

Fig 5c, why does snow depth appear to drop considerably throughout the season?

Fig 5e, ice emissivity masked by overlying snow?

Fig 6, remove 'none'.

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