The revised manuscript by Patilea et al. has improved regarding clarity and writing and also the differences to the paper by Huntemann et al. (2016), which covers a very similar topic, are now made clearer. However, I still have major concerns regarding the paper:

1) Compared to the study by Huntemann et al. (2016), the authors here
a) use a newer version of SMOS data, which leads to a mean deviation in sea ice thickness (SIT) of 0.22cm & RMSD=1.35cm for TBs in the Arctic for 1 Oct - 31 Dec 2015 (this time period is also used for the following comparisons).
b) use a slightly different RFI filter and choose the SMOS TBs somewhat differently than before: They fit the SMOS TBs to 45deg incidence angle instead of using the 40-50deg TB mean. This results in a mean deviation of -0.3cm & RMSD=2.0cm compared to a).
c) fit SMOS TBs to 40 deg incidence angle (which is the incidence angle provided by SMAP) and perform a linear regression between SMOS and SMAP TBs to apply the SIT retrieval to SMAP , which does appear to be a more "straightforward" approach for combining SMOS and SMAP for the SIT retrieval as compared to the approach in Huntemann et al. (2016). The SMOS and SMAP SITs differ on average by 0.2cm with RMSD=2.4cm. The mean deviation between the combined SIT product and the SMOS SIT derived from 40deg incidence angle is <0.1cm & RMSD=1.4cm.

>Conclusion: The modifications made to the already existing retrieval seem to be relatively small, and as none of these data sets is compared to independent data, the presented modifications to the joint retrieval from SMOS and SMAP may be more of "technical importance" showing small changes (theoretical improvements?) to a previous suggestion to combine SMOS and SMAP data. From the presented study it is also not clear whether there is any advantage in using a combined SMOS and SMAP SIT retrieval as compared to the existing SMOS-based retrieval. By using TBs at 40deg incidence angle instead of 45deg, the usable polarisation difference range for the retrieval reduces from Delta_TB=32K (22...54K) to Delta_TB=26K (17...43K), which can be a disadvantage. On the other hand, as one of the advantages the authors claim that the data coverage by the joint product is 6% larger (for the area north of 55.7deg N). However, from Fig. 6, I would infer that the gain in data coverage is mainly achieved at the edge of the selected area, i.e. in areas that are covered by ocean and not sea ice, which would make them somewhat irrelevant for the SIT maps...

2) In the revised manuscript version, the authors have added a section and new figures on the uncertainty assessment. For the uncertainty computation, the authors assume that TB uncertainty of SMOS is equal to the RMSD resulting from fitting the SMOS TBs to 40deg incidence angle. Firstly, as the authors also recognize, this is an underestimation because the RMSD in the fitting iteration is limited to 5K. And secondly and more importantly, this is NOT the only source of TB uncertainty! The relationship between SIT and TB depends on the ice conditions: For example, the salinity of the ice (or more precisely: brine volume fraction), the snow cover, the ice type (these are not mentioned at all), and the ice concentration (mentioned later by the authors but not used to estimate the uncertainty). This variability (and thus uncertainty) is (partly) reflected in the scatter of the training data for the retrieval curve (only partly because the training was done only for Oct-Dec 2010 and in two specific areas of the Arctic). The variability of TBs at the same place and time at different incidence angles should only reflect a small part of the uncertainty...

Fig. 7 shows SIT as function of TBH and TBV, although in the retrieval, SIT is a function of polarisation difference and intensity. This makes the figure and its implications hard to interpret. It is also not clear which of the shown combinations of TBH and TBV (or better: polarisation difference and intensity) are actually seen in the satellite observations. This raises also the question how the retrieval is actually performed using the retrieval curve. In Huntemann et al. (2016), I found: "The minimum euclidean distance of the fit to the data in the I-Q-space defines the retrieved ice thickness." Is the retrieval performed no matter how large this "minimum euclidean distance" is?

If so, how representative are retrieved SIT with large distance from the retrieval curve? What is the distribution of distance values encountered during the retrieval? In Fig. 7, I think, we can see how not restricting the "minimum euclidean distance" to a maximum value leads to an odd behaviour, as seen for TB combinations below the TBH-TBV 1:1 line (where a small change leads to completely different SIT to be retrieved).
Also, from the text (Sect. 3.2) I understand that the retrieval is performed when at least two TB values are found (at least one below 40 deg and one above 45 deg). This issue is, for example, discussed in the paper by Schmitt et al. (2018), which also deals with combining SMOS and SMAP data for sea ice applications. They perform the incidence angle fit only if at least 15 measurements are found, which seems more appropriate considering the relatively high scatter of SMOS data...

> Conclusion: I do not agree with how the uncertainty is estimated and I would suggest to show sensitivity of polarisation difference and intensity instead of TBH and TBV for the values actually encountered during the retrieval, which have to be analysed/shown first.

3) Another issue is that the authors have added some more statistics based on a three month period (Oct-Dec 2015) as compared to presenting mainly statistics based on one day of data as was done in the first manuscript version. However, in the revised version, there is still conclusions based on the analysis of one single day of data (Sect. 3.3 & 5.1). I think, example maps for one day can be ok/useful, but, as far as I can see, statistics based on one day of data are not necessarily useful (unless their representativity is shown or at least discussed).
In addition, the authors use the expression "bias" for comparisons of data sets (e.g. SMOS v5.05 vs. v6.20 SIT retrieval in Sect. 3.1, fitted to 45 deg vs. 40-50deg average SMOS SIT in Sect. 3.3 and SMOS vs. SMAP in Sect. 4.1). As far as I know, "bias" is not used (and is indeed very confusing) for comparisons of two data sets, of which it is not clear which one is more realistic/better etc.). I think "mean difference" would be more appropriate here.

Further comments:

-p. 1, l. 4: "SMAP observes ... which makes thin sea ice thickness retrieval more consistent" -> This sounds like SIT retrieval from SMAP is more consistent than from SMOS, while you probably aim to say (as we have already discussed and agreed on in the first review round) that the retrieval is "easier" (or more consistent if you like) if a fixed incidence angle is used instead of an incidence angle range, which is, in principle, also possible with SMOS data (by choosing TBs at this incidence angle only). This should be expressed unambiguosly.
- p. 2, l. 28: "Its resolution is..." -> better: The grid spacing is...
- p. 4, l. 2: And which RFI filter are you using? Either write it here or refer to where you describe this in the paper.
- p. 5, l. 11-13: First, you write about "bias" and "RMSD", then about "bias" and "standard deviation". Are RMSD and standard deviation the same here?
- p. 6, l. 3 : Reference to "Eq. 3" is misleading here. Eq. 3 has not been given yet and could mean that you refer to Eq. 3 in Zhao et al. (2015). Also at p. 6, l. 8: First give Eq. 3, then refer to it.
- p. 6, l. 8-18: It sounds like C is determined first and then only the other parameters (ah, bh,...) are determined? Is this / how is this done? Are ah, bh, ... determined differently than C?
- p. 6, l. 19-20: To make this clearer, I suggest to add something like: In this case, the least squares method to fit the parameters is repeated.
- p. 6, l. 8-24: It would be helpful to clearly state that you do not take into account the measurements that do not meet the defined criteria after a maximum of five iteration steps (if this is how these cases are handled...).
- p. 6, l. 28 & p. 7, l. 4: You probably refer to Fig. 1 here instead of Fig. 2.
- p. 7, l. 20: "This is ..." -> What is "this" here? (bias and/or RMSD?)

- p. 7, l. 20- 23: Complicated/unclear description of what you do here... E.g. "selecting all grid cells with that thickness..." -> what is "that" thickness?
-p. 7, l. 22-23: " Only grid cells that contain at most 50 cm and non-zero in at least one of the two algorithms are used." -> Make it clearer whether the condition "in at least one of the two algorithms" applies also to the selection of grid cells with at most 50 cm SIT.
- p. 7, l. 27 & l. 28: "always generating" & "will generate" -> For clarity, maybe add "falsely"/'spuriously".
- p. 7, l.35: What is the "absolute bias" (as compared to just "bias"?)
- p. 9, l. 6: Does the reference to "(Sect. 4.1) refer to Sect. 4.1 in Huntemann et al. (2016) or what is meant here?
- p. 11, l. 1-2: "a weighted standard deviation... is used" -> weighted for what?
- p. 11, l. 3: "The correlation was calculated for a period of seven days." -> Why only seven days?
- p. 11, l. 22-24: Not very clear description.
- p. 11, l. 30-31: "As we have observations at two polarizations at each grid cell available, it should in principle be possible to retrieve SIT and ice concentration simultaneously." -> "In principle", this is only possible if TB varies independently with ice concentration and ice thickness, which would have to be shown before stating phrases like this.
- Sect. 5.2: Why are these considerations not used to estimate SIT uncertainty?
- throughout the paper:
a) Use "an" instead of "a" before abbreviations RFI, RMSD, L-band.
b) The text still contains some typos / language issues (e.g. mixed usage of singular and plural).
c) TB is introduced as abbreviation for brightness temperature, SIT for sea ice thickness, but sometimes the authors use these abbreviations, sometimes not (can even vary within one sentence).
-Is the area used for training and inter-calibration of SMOS and SMAP data defined somewhere in the paper?
-no (clear) references for statements on p. 1, l.15; p. 2, l. 4-8; p. 3, l. 5-8 & l. 13 & l. 14 & 16-19.

Reference:
Schmitt, A. U. and Kaleschke, L. (2018): A Consistent Combination of Brightness Temperatures from SMOS and SMAP over Polar Oceans for Sea Ice Applications, Remote Sensing, Vol. 10 (4), 553.