The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-279-RC1, 2019

The Cryosphere
Discussions

Interactive comment on "Recent changes in pan-Antarctic surface snowmelt detected by AMSR-E and AMSR2" *by* Lei Zheng et al.

Anonymous Referee #1

Received and published: 3 June 2019

The presented paper is addressing the melt season in the Antarctic on the Antarctic ice shelves and the Antarctic sea ice cover. The research uses methods for the melt detection from AMSR-E and AMSR2 which are well established and the correction for ice concentration is promising idea to improve the melt onset detection. However, some of the analysis seems shallow and not well documented. Many information on how the results were obtained are missing.

General Comments:

1. The definition of melt is unclear in the manuscript. What exactly is supposed to be detected and discussed?

2. Some of the results regarding the melt onset and length are in line with other pa-

Printer-friendly version

Discussion paper

pers, however the melt onset of sea ice seems quite early in comparison to the cited references and other observations. Thus these early detected melt onset (July, August) need further physical investigation and should probably not directly interpreted as the real melt onset.

3. The optimal local acquisition time of AMSR-E compared to SSM/I repeatedly stated by the authors needs further explanation or investigation. It should be critically discussed whether other influences (maybe sun influences or instrument temperature) can alter the results (lead to too early snowmelt detection)

4. I'm surprised to see shelf and sea ice melt in the same way analyzed. They are so different in their nature and also physical properties that I would not even have expected that the same method would work adequately on both. For example there are brine and flooding effects in sea ice which are not present in the shelf ice. It should be more clearly stated in the Manuscript why it is useful or desired to combine the analysis.

5. The vast amount of references makes it very hard to find the the real sources for certain statements. This makes the manuscript appear cluttered and lacking a concrete direction and purpose.

Some specific Comments:

P1, L16: "DAV" should be directly introduced as TB_v difference of ascending and descending swaths either in the abstract or at the very first occurrence in the text

P2, L16 & L23: first statement is "passive microwave remote sensing works in all atmospheric conditions" and then "altered by clouds, atmosphere, ..." what do you want to say here?

P3, L14: see General point 3.

P4, L15: SIC>15% was used and only SIC>80% was used in melt detection? Does this mean a pixel never exceeding 80% SIC is never melting? And pixels exceeding

80% SIC only later can only melt from this point on? I would expect that this gives you a negative bias in MDF (since it counts as frozen even in melting conditions).

P4, L20: please state the exact field of the ERA interim dataset used including timestep, are you using “Air temperature at 2m height” from the surface analysis? Also: in how far was the data used to “assist” with the AMSR-E/2 melt detection? Is this is described somewhere else in the text?

P4, L29-P5: It is unclear what the MEMLS simulation is for. In Kang et al. (2014), which you are citing four times in this paragraph, this is discussed in very detail. The variation of snow grain size is barely discussed in this paragraph and from what I got, never really picked up again in the manuscript. I would probably just remove the Fig. 1.

P5, L5-6: specify the interface you are talking about, probably the snow-air-interface

P5, L10-15: Since you employ the method, can you show that this signal is consistent characteristic for melt? For a longer constant melt under full sun illumination, there is probably not much difference between day and night wetness in the snow. Also in Fig. 3, under constant positive air temperatures, there is not constant DAV>10 which indicates that the melt indicator from DAV and the positive temperatures are not strictly connected.

P5, L15: see General point 3.

P5, L21: see General point 3.

P5. L31: I cannot see this in Figure 2. There are at least 3 years (2005, 2007,2009) where there is day in mid-winter with positive air temperature where DAV does not exceed the threshold nor shows any signal.

P5. L32: Accuracy and Kappa should be defined somewhere.

P6. L9 (Eq 4): This is only true under the assumption that SIC did not change within the

~12h from ascending to descending overflight. This should be mentioned. The method could be optimized in this regard by using the Tbs to retrieve the ice concentration in ascending and descending separately and then calculate the DAV with the aid of an open water tie point (which does not cancel out in case SICs are different for ascending and descending overflights)

P6. L20: accuracy and Kappa definition again

P6. L22: Why is a spatial median needed here, what are erroneous microwave signals?

P6. L25: "Melt freeze-up and duration. . .." - I don't understand what is meant here

P6. L29: extend -> extent

P7. L7: if below -5°C means frozen state (P6. L24) and above -1°C means melting, what state is there in between and how is that classified?

P7. L16: Discussion about Fig. 5, see also General 2.: the mid July melt onset around -60 to -65 latitude is quite surprising and needs discussion. Also the later melt onset in the more outer parts are interesting. Is it because there was no ice at the melt onset of the more southern regions and ice drifted there later so that melt occurs later in these regions? However, than the MDF should be even higher in these regions, probably close to 100%. I would also suggest not using the parula but a diverging colormap for the difference plot.

P7. L31: "Fig. 5k-o" -> "Fig. 5g-i"

P8. Discussion about Fig. 6: I would suggest splitting the histograms to maybe a 7 by 3 plot to be able to discuss the particular regions better. Also bin size in the histograms is too small, i.e., the histograms are to noisy to comfortable read their data.

P8. L11-12: The comparison of the melt extent of AIS with the sea ice area makes no sense in my opinion. The AIS is a much smaller region. What is the purpose of this comparison? I suggest to remove Figure 7 completely. However, the melt extent is quite small in the early months like July/August on sea ice which actually contradicts the early melt onset in Fig. 6. This also indicates that the early melt onset is probably just a random noise effect since it does not cover a large area apparently.

P8. L17: with “mean maximum MEF” you mean the “Mean anual Maximum MEF” right? should than be changed in the text.

P9. L2: I actually do not understand how the trends are calculated. Fig 9 indicates that you calculate the trends pixel based. One would expect that neighbouring pixel having similar melt onset dates (Fig 9a). If the shown pixel based trends have any significance also the spatial pattern should be coherent.

P11. L 18: I suspect the values and discussion to change in case you reconsidered the early snowmelt onset

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-279, 2019.

Printer-friendly version

Discussion paper