Thank you for your constructive comments. Reviewer comments (in black) are addressed by author comments (in blue) below.

General Comments: The manuscript is well-written and nicely presented but is very light on new information. The melt onset algorithm by Drobot and Anderson (2001) has been updated and the trends have been re-calculated but I'm not sure if that is sufficiently new information to warrant publication. My major concern is that Stroeve et al. (2014)-GRL just recently provided a new and thorough assessment of the links between melt, freeze and changing Arctic sea ice that includes trends and driving factors. Markus et al. (2009)-JGR also just fairly recently published a paper on melt and freeze trends. I think the authors need to add some new information to this work other than just updating the trends. I offer a few suggestions that hopefully could improve this contribution.

1. What about investigating the factors influencing melt onset? Drobot and Anderson (2001) looked at the relationship to the Arctic Oscillation. Does this relationship still hold? What about looking at some synoptic weather events driving melt? Else et al. (2014)-JGR (DOI: 10.1002/2013JC009672) provided a detailed look at the transition to melt onset over landfast sea ice which could be scaled up using NCEP or APP-x data.

2. Have the authors thought about comparing melt onset dates to climate model out- put? The operational ice forecasting community is very interested in knowing how well their models represent the timing of melt (and freeze). Wang et al. (2011)-JGR compared satellite derived melt onset to the Canadian Coupled Global Climate Model and noted model biases so perhaps other models could be looked at? Mortin et al. 2013-Climate Dynamics (DOI: 10.1007/s00382-013-1811-z) provide a very thorough approach of which some aspects could be used in your analysis.

3. What about adding a section to the manuscript comparing the AHRA approach with other melt algorithms? I realize the Markus et al. (2009)-JGR approach uti- lizes the AHRA as an indicator but I'm not sure about if there has been a compar- ison between the two approaches. There are also melt onset dates available by QuikSCAT and ASCAT that could also be used for comparison. See Mortin et al. 2014-RSE (http://dx.doi.org/10.1016/j.rse.2013.11.004) and Mortin et al. 2012-JGR (doi:10.1029/2012JC008001).

First we would like to disagree that this is light on new information. This data set has been used by many and the new extended dataset is an important part of the literature. Nevertheless, we have now included in this publication a more detailed discussion of the evaluation that was conducted in making this new data set and how the new data compares to the older version (see Section 3). This discussion highlights the spatial and temporal changes in the data. The three possible topics suggested by the reviewer are interesting and should probably be done. And at least one, atmospheric comparisons during melt, is a research project that this team is currently conducting. Preliminary results show that storm tracks are changing over time, however, the amount of analysis and statistical relationships between melt and the atmosphere needed are way beyond the scope of the current paper and will need to be published independently. There is also more discussion regarding the other recent melt onset studies (Stroeve et al. 2014) in section 5, so hopefully this will fulfill the request for more work included in this publication.

Specific Comments: Title Suggest changing it to "Melt onset over Arctic sea ice..." or "Snowmelt onset over Arctic sea ice..." because that is what the Tb's are actually detecting.

We agree that the title is confusing. We have taken your suggestion and have changed the title to "Snowmelt onset over Arctic sea ice from passive microwave satellite data: 1979-2012".

Page 3040, Line 14 You could probably add a reference or two for the increases in liquid water that increases Tbs. I also think the authors should move the Drobot and Anderson (2001) reference to the start of the paragraph.

The Drobot and Anderson (2001) reference has been moved and two references have been added for the statement about liquid water increasing Tbs.

Kunzi, K. F., Patil, S., and Rott, H.: Snow-cover parameters retrieved from Nimbus-7 Scanning Multichannel Microwave Radiometer (SMMR) data, IEEE Trans. Geosci. Remote Sens., GE-20, 4, 452-467, doi:10.1109/TGRS.1982.350411, 1982.

Livingstone, C. E., Singh, K. P., and Gray, L.: Seasonal and regional variations of active/passive microwave signatures of sea ice, IEEE Trans. Geosci. Remote Sens., GE-25, 2, 159-172, doi:10.1109/TGRS.1987.289815, 1987.

Page 3041 Line 11 I assume the author's mean NASATeam ice concentration estimates?

We use Goddard merged sea ice concentrations developed by Meier et al., 2013. These concentrations are developed from a method that combines sea ice concentrations from both the NASA Team and Bootstrap algorithms. We have added a sentence to the following to clarify that the concentrations we use are a combination of both the NASA Team and Bootstrap methods: "The concentration data used here are Goddard merged sea ice concentrations available as part of the NOAA/NSIDC Arctic Sea Ice Climate Data Record (Meier et al., 2013). The Goddard merged sea ice concentrations are based on an algorithm that utilizes a combination of sea ice concentrations from the Bootstrap and NASA Team sea ice concentration algorithms."

Meier, W., Fetterer, F., Savoie, M., Mallory, S., Duerr, R., and Stroeve, J.: NOAA/NSIDC climate data record of passive microwave sea ice concentration, Version 2, National Snow and Ice Data Center, Boulder, Colorado USA, doi:10.7265/N55M63M1, 2013.

Page 3041, Line 10 Based on the 50%> ice concentration threshold, when trends for the marginal ice zones are calculated they will not always be for the same number of years. I think this needs to be shown visually because it influences the rates of change and perhaps your statistical significance because of reduced degrees of freedom. An iso-melt line showing spatially where the concentration is always greater than 50% would be useful and show where you have confidence in the trends.

We show an example of the changing ice edge as defined by the 50% ice concentration threshold in Figure 1, which compares the annual MO date map for 1979 with the map for 2012. We intended this figure to provide an example of the year to year variability in the sea ice edge, but have added a clearer description of the differences in these two maps and how they are a result of the 50% threshold. Sentences 2-3 in the paragraph following Page 3041, Line 10 now reads "The annual MO date maps for 1979 and 2012 in Fig. 1 illustrate the changing sea ice mask based on the 50% sea ice concentration threshold described above and serve as sample data from the V3 data set. Some noticeable differences in the ice edge between the 1979 and 2012 MO date maps occur in the Sea of Okhotsk and in the Baltic, Greenland, Barents, and Bering Seas (Fig. 1)."

Further, the first line of Section 2.3 noted that "All statistics reported here are calculated from pixel locations where a MO date exists in all 34 years of the data record." Figure 2 shows a climatology mask of the pixel locations where a MO date exists for all 34 years in the data record. In this figure, white pixels indicate locations that are open water (concentration < 50%) or where a MO date does not exist for one or more years during the 1979 to 2012 (inclusive) study period. Grey pixels are the land mask. The pixels that are not white or grey represent the sea ice locations where a MO date exists for all 34 years (the different colors only define the boundaries between Arctic sub-regions). The colored pixels in Fig. 2 serve as both an indication of where we have confidence in the trends calculated and along the marginal edge of the ice cover show where the sea ice concentration at the beginning of March is always ≥ 50%.

To make this point clearer, we have added more description to the caption for Figure 2 and have changed the first five sentences of Sect. 2.3 to read:

"All statistics reported here are calculated from pixel locations where a MO date exists in all 34 years of the data record. The sea ice locations shown in Fig. 2 show the MO date climatology mask used in the calculation of statistics. Grey pixels representing land and white pixels representing open water and locations that do not have a melt date for one or more years and are excluded from all calculations. Statistics are calculated for all of the Arctic sea ice cover (hereafter called the Arctic Region) and for smaller sub-regions of the Arctic that are identified by different colors in Fig. 2. The area (in km^2) for each sub-region of the Arctic is not equal in this work because we restrict calculations of statistics to the MO date climatology mask and implicitly the sea ice extent."

Page 3041, Section 2.2 I think there is value to be added from a more detailed comparison between V2 and V3. The authors discuss the improvements made with V3 but could they be quantified?

We have added a new section (Section 3) and new figures (Figures 3-5) which show a detailed comparison of the primary differences users will see in the data between V2 and V3.

Page 3046 Perhaps it would be useful to look at the Bering Sea more closely? This could add another component to the manuscript (see General Comments). The authors could look at the ice concentration anomalies with respect to melt onset timing. Plotting air temperatures for the region might also lend some insight. A section on explaining regional melt onset variability could also be a useful addition.

Again this type of analysis while interesting is not within the scope of this publication.