Thank you for your helpful comments. We have revised our paper accordingly and feel that your comments helped clarify and improve our paper. Please find our response (in blue) to reviewer's specific comments (in black) below.

Specific Comments

– p. 3038, line 2. I would be inclined to add "(Version 3)" after "An updated version"; but if the authors disagree, they should leave it as is.

We have added "(Version 3)" to this sentence as it does add clarity for readers.

– p. 3038, line 2. I'm concerned about the mismatch between the title of the article, which mentions "sea ice melt onset", and the first sentence of the abstract, which instead mentions "snow melt onset".

The title has been changed to "Snowmelt onset over Arctic sea ice from passive microwave satellite data: 1979-2012".

– p. 3039, line 22. I'm fairly certain the authors meant a resolution of 25 km x 25 km rather than the much finer 25 km^2 resolution that is indicated.

Yes, this is correct. We have changed the resolution to "25 km x 25 km".

– p. 3040, second paragraph. This paragraph is extremely important in terms of letting the reader know what criteria were used in the calculations. However, it should be more explicit regarding the following two points:

(a) On lines 19-20, the text indicates that "the date is recorded as the day of melt onset." I'm guessing that at that point that particular location has received its melt onset date for the year and is not examined further for that year. If so, it would be best for the text to state that; and if not, the text should state what further is done for that location for that year.

(b) On lines 21-22, where it says "If the difference between Tbs during the periods prior to and following the day in question is > 7.5 K . . ." it should be made clear exactly which difference is being referred to. My guess is that it's the difference between the average of the 10 days prior and the average of the nine days following the day in question, but the current version of the text is not explicit enough to make that clear.

This paragraph has been edited to clarify these two points. You are correct that a MO date is only determined once per year for each pixel. That is, the algorithm does not examine a pixel further once a MO date is assigned. In response to point (b), an average of the 10 days before and 9 days following is not used, but rather the difference between minimum and maximum daily horizontal ranges (HR) are calculated before and after the date in question then compared.

– p. 3041, lines 3-7. The two main sentences on these lines – "The previous version of the data set (V2) was masked to the climatology of locations where a MO date had been calculated for every year in the 20 year period 1979–1998. This climatology mask was static and determined the pixels for which a melt date was calculated every year." – are not nearly as clear as the sentence that follows them on lines 7-9. I suggest rewording the two sentences on lines 3-7 to something along the lines of: "The previous version of the data set (V2) was masked in such a way that a melt date was calculated only at those locations where a MO date could be calculated for every year in the 20-year period 1979–1998." As an additional comment: The shift from using a static mask is a major conceptual improvement over the Version 2 data set.

These two sentences were changed as suggested to improve clarity.

– p. 3041, lines 22-23. I think it would be more accurate to say that the difference in the data gap at the North Pole is due to the difference in the Nimbus 7 and DMSP satellite orbits as well as to the difference in the swath widths.

This has been corrected.

– p. 3042, lines 5, 8, and 10. On line 5, I think it would be much better to say "the Tbs are adjusted to improve . . ." rather than "the Tbs are corrected to improve . . .", the reason being that the original Tbs aren't "wrong", they are simply not yet intercalibrated with the Tbs from the other sensors. Similarly, on line 8, "the F17 Tbs are also corrected back to F8 Tbs" would be better as "the F17 Tbs are also adjusted for intercalibration with the F8 Tbs"; on line 10, "regression correction" would be better as "regression adjustment"; and on p. 3040, line 10, "Tbs are corrected" would be better as "Tbs are adjusted for intercalibration".

References to Tb "correction" have been removed.

– p. 3042, lines 13-14 and Figure 2. The fact that the statistics are only calculated at pixels where a melt onset date exists in all 35 years of the data set is very unfortunate, as it leaves out pixels whose trends could be of great interest, namely, both those that had no melt at the start of the record but melt by the end of the record and those that had melt at the start of the record but by the end of the record had no ice and hence no melt. I am definitely not recommending a change, as it's important that the reader be told of this limitation, but it is an unfortunate limitation. Looking at Figure 2, it is clear that this limitation has greatly limited the area being considered in the Sea of Okhotsk, the Bering Sea, the Gulf of St. Lawrence, the Greenland Sea, the Barents Sea, and the Baltic Sea. In fact, for both the Gulf of St. Lawrence and the Baltic Sea the area for the statistics is so restricted that it seems to be largely near-coastal points that likely have land contamination issues. However, despite that concern, the statistics by and large seem quite reasonable, suggesting that the algorithm is good.

We agree that the climatology mask used to calculate the statistics in this paper is a limitation, however, we opted to use the 34-year climatology mask to be consistent with past work (e.g. Drobot and Anderson 2001). Comparisons between locations without a melt onset date in the earlier years of the data record and locations that now have observed melt onset in recent years would not have been possible using the old Version 2 data set. With Version 3, we believe that comparisons such as this would add valuable information about trends in melt onset for future work.

– p. 3043, lines 11-12. This sentence introducing the melt onset maps states that the MO dates "vary highly". To me, it is at least as important that they vary systematically, as the melt onset progresses northward (Figure 3d). The text mentions the latitudinal dependence later in the paragraph, and so on line 11, I would simply recommend changing "vary highly" to "vary greatly but systematically".

This has been changed in text.

– p. 3043, lines 20-23, Table 1, and Figure 3e. (Note: It's important to add a comma after "Beaufort Seas" on line 22.) I can see from Table 1 that the East Siberian Sea comes out with the largest standard deviation, as stated on lines 22-23, but I encourage the authors to double-check those results in view of Figure 3e, where the northern Barents Sea has much higher values (though counteracted by the low values in the southern coastal strip of the Barents Sea) and even the Kara and Greenland Seas appear to have overall higher values than the East Siberian Sea. Everything might be correct, but I think the authors should check it. Also, assuming everything is correct, I recommend that the authors change "greatest standard deviation" to "greatest average regional standard deviation" in line 22, just for extra clarity, as Figure 3e clearly shows much greater individual pixel values elsewhere.

We have rechecked the statistics presented in Table 1 and the calculations are correct. However, it is important to note the values have not been weighted to account for the differences in region area. Since the Barents Sea region area is small (3.5×10^5 km^2) compared to other regions such as the E. Siberian Sea (12.6×10^5 km^2) or the Kara Sea (8.3×10^5 km^2) the average regional standard deviation is large (Table 1) especially considering that MO occurs on the sea ice in the southern Barents Sea consistently early in the year, near DOY 60. The standard deviation for only the southern Barents Sea would be small, but the higher variability in the northern Barents Sea coupled with the small region is a contributing factor to the large regional average standard deviation shown in Table 1 when compared to slightly smaller standard deviation for larger regions such as the E. Siberian Sea.

We have changed the wording on line 22 as suggested, added a comma following "Beaufort Seas", and have also added a note to clarify that the region sizes are variable and no weighting for region size was implemented when calculating statistics.

– pp. 3043-3044 and Figure 3. Figure 3 is extremely informative. However, in the figure caption and the text, part (a) is the earliest MO date, (b) is the latest MO date, and (c) is the range of MO dates, whereas in the figure itself, (a) is the range, (b) is the earliest, and (c) is the latest. The figure needs to be fixed to match the caption and text (or vice versa).

Figure 3 (now Fig. 6) has been corrected to match the text and caption.

– p. 3044, lines 14-17. Here the later mean MO dates in the Central Arctic seem to be tied to the ice being more compact as well as to the latitude and temperature. Offhand, I don't see why the ice compactness would matter. I recommend rearranging these two sentences to: "Conversely, sea ice in the Central Arctic is typically thicker, more compact, multiyear ice. Furthermore, air temperatures would warm later in the year than further south, leading to the later mean MO dates observed."

This wording change has been made.

– p. 3044, line 18 – p. 3045, line 17. These two paragraphs could be shortened, by assuming that most readers will realize that in general, other things being equal, the MO dates along a given longitude should generally be later at higher latitudes. My inclination would be to delete at least lines 5-9 (and the last few words of line 4) on p. 3045, in view of the sentences preceding and following those lines.

This sentence has been removed.

– p. 3046, second paragraph. It would be relevant in this paragraph regarding the positive trend in the Bering Sea to reference one of the studies showing the complementary trends toward increases in sea ice extents in the Bering Sea. This would lend support to the validity of your result. One possible reference would be the following, which shows a positive trend in the Bering Sea and negative trends in each of the other eight Arctic regions discussed, for the period 1979-2010:

Cavalieri, D. J., and C. L. Parkinson, 2012: Arctic sea ice variability and trends, 1979-2010, The Cryosphere, 6, 881-889, doi:10.5194/tc-6-881-2012.

This reference has been added in support of our finding.

– p. 3047, lines 7-8. To me, the fact that the timing of melt onset "has some depen- dence on latitude" is not much of a finding, as it seems it would be expected. Hence I recommend that lines 7-8 be reduced to: "Based on this 34-year record of MO dates on Arctic sea ice, we have shown that typically the sea . . ." However, it the authors prefer the original, they should leave it as is.

The suggested wording is better and has been changed in the text.

Apparent Typos

– p. 3042, line 18. It seems "differing colors" should be "different colors".

Corrected

– p. 3046, line 24. "... spring and can lead ..." should be "... spring can lead ..."

Corrected

– p. 3048, line 17. "indicate" should be "indicates".

Corrected