Response to Reviewer TCD
Recent changes in spring snowmelt timing in the Yukon River Basin detected by passive microwave satellite data.
KA Semmens and JM Ramage

In this paper the authors use passive microwave satellite data (SSM/I 37 V-GHz) from 1988 to 2010 to try to delineate snowmelt timing trends for the Yukon River Basin (YRB), Alaska/Canada. Their results suggest that there has been a lengthening of the duration of the melt period for the majority this vast basin, and that the melt is beginning earlier than before in some of the higher elevation locations. Of particular note in the findings, though not called out specifically, is that there is a great degree of temporal synchronicity in the melt anomalies across the whole basin (Fig. 5).

We really appreciate the perspective and thoughtful observations provided in this review. The reviewer makes a great point about the temporal synchronicity of melt timing across the basin. We believe that this temporal synchronicity and coherence substantiates the melt timing derivations and trends presented in the TCD paper and provides support that these are reasonable results and not just an artifact of vegetation or sub-grid variability since a wide range of elevations, relief, and vegetation are throughout the study area.

The results are very interesting but are they real? That is the problem with the paper: after reading it twice, I still was not sure. So then I went and read the antecedent papers by Ramage et al., to better understand the method used to set the start and end of melt. Two salient facts are used to support that the passive microwave indices for melt used by the authors (Tb<240 K and absolute value of DAV>10K) are good metrics: (1) a regression of Tb against air temperature on the Juneau Icefield (Ramage, J. M. and Isacks, B. L.: Determination of melt-onset and refreeze timing on southeast Alaskan icefields using SSM/I diurnal amplitude variations, Ann. Glaciol., 34, 391–398, 2002), and (2) the relationship of run-off timing with melt timing (Ramage, J. M., McKenney, R. A., Thorson, B., Maltais, P., and Kopczynski, S. E.: Relationship between passive microwave-derived snowmelt and surface-measured discharge, Wheaton River, Yukon, Hydrol. Process., 20, 689–704, 2006) for DAV plus Tb. The former example is from a maritime icefield where it appears air and physical snow temperatures rarely dropped below -5°C (contrast this with -40°C for the YRB in general); the latter is from a headwaters basin of the Yukon, also mountainous, and at least partially a maritime snow cover. Are these metrics useful for a basin that stretches from the low-lying lake and shrub-covered Yukon-Kuskokwim Delta to the vast boreal forests of interior Alaska and Canada and is by area overwhelming cold boreal forest. I am doubtful.

The reviewer brings up an interesting point about whether the melt detection methods used here are valid across large heterogeneous regions such as the Yukon River Basin. There are several reasons to be confident that this approach can reasonably be applied to the study area: it is a physically based temperature threshold (SSM/I threshold is 246K), the T_b distribution used to detect the melt threshold is widely consistent, it has been successfully applied in other regional studies, and the results are corroborated by ground station, modeling, and reanalysis data (more detail provided later). An example of the application of the approach over a wide domain is the pan-arctic study by Tedesco et al. (2009). The

brightness temperature threshold is physically based because the minimum in the histogram of annual brightness temperatures is the point at which melt initiates, air temperatures are above freezing, and emissivity increases dramatically; as such, there are only a few days with this brightness temperature threshold.

The Yukon River Basin encompasses a wide range of landcover and relief which contributes some uncertainty to the results, however, the temporal coherence of trends across the basin suggests this noise is minimal compared to the signal. Further, the ground station data available for more interior, lower elevation areas support the satellite detections (see Fairbanks illustration in Figure 2 of the TCD paper). These same relationships are seen across the basin in stations from Galena, Tok, Coldfoot, Old Crow, Bettles, and Nenana but all the examples were not included in the interest of space.

Additional validation of the melt detection approach (applied to earlier short melt events in winter) with SnowModel (Liston and Heimstra, 2011), ground, and North American Regional Reanalysis data are shown in Semmens et al. 2013 (Semmens, Ramage, Bartsch, Liston: Early snowmelt events: detection, distribution, and significance in a major sub-arctic watershed. *Environmental Research Letters* , 8, 014020, 2013). Based on the evidence from available ground station, model, and reanalysis data, and the physical nature of the melt detection, we believe the approach utilized in the TCD paper is robust to detecting melt across wide spatial scales including large basins such as the Yukon. The passive microwave channel used (37 GHz) is very sensitive to the large and distinct melt onset signal. The reviewer is right that we need to be aware of the vast size and heterogeneity of the basin – we acknowledge this uncertainty but suggest there are larger, overarching climate processes influencing melt timing resulting in a coherent signal.

To add some of this supporting literature to the TCD paper under review we added the following sentences in the Data and Methods section at the end of the paragraph beginning with "Snowmelt onset was determined from SSM/I data (37 GHz vertically polarized):

"A similar threshold based passive microwave melt detection approach was previously applied successfully over a wide spatial domain in the pan-Arctic study by Tedesco et al. (2009). In addition, the passive microwave derived melt timing signal (onset and melt-refreeze) was corroborated by auxiliary datasets, including ground station data (Global Historical Climate Network), model results from SnowModel (Liston and Hiemstra, 2011), QuikSCAT backscatter change (Bartsch et al., 2010), and North American Regional Reanalysis (NARR) data (Semmens et al., 2013)."

The first problem is that the authors reference little or no direct observational work on how snow melts and where the melt water goes. They tacitly assume the snow across the entire basin melts fundamentally in the same way. It does not. The vast majority of the YRB is covered by thin tundra or taiga snow. Snow in these forests melt in ways fundamentally different than deep maritime snow packs, where the metrics for the melt were developed. I would strongly recommend the authors read (and reference) critical works on the way the melt proceeds in these thin snow packs, for example "Wetting front advance and freezing of meltwater within a snow coverObservations in the Canadian Arctic" in WRR by Marsh and Woo (1987). A chief difference between the two snow packs is there is a significant amount of "patching out" of the snow in the taiga long before the snow is isothermal. Also, the basal 2/3rds of the snow pack can be cold while top few centimeters cycle from wet to dry, then percolation can drain the top and reverse the wetness distribution. While the onset of melt is probably easily detected using passive microwave signals for all these varied snow packs (since it takes just a little melting at the tip), one could question whether the drop in DAV really indicates a fixed time in the melt cycles of all snow packs regardless of snow density, depth, perched ice layers and other stratigraphic aspects of the snow. So I would ask whether the end of melt as defined by ssM/I is a climate metric. . . .or is it more a dynamic value that occurs at different times and places for a variety of reasons?

We appreciate the ground based, stratigraphic perspective offered by the reviewer. We would like to clarify that the intent of the TCD paper is to focus on the timing of melt at a broad spatial scale, as our methods cannot resolve where the melt water goes or stratigraphic variations in melt. The 37 GHz channel used in this study detects surface melt and refreeze, and not melting or refreezing that is going on at depth. The rise in DAV values is consistent with melt onset from T_b after which there is a wet surface during the day that refreezes at night maintaining a high DAV signal. The drop to low DAV after the melt-refreeze period means the surface is consistently wet snow or soil, or consistently dry ground. Essentially, the low DAV signal means that the surface is in a static condition. The dynamic wet/dry contrast measured with DAV is common to all snowpacks as a binary surface measure. The end of melt may be influenced by a variety of factors, but the DAV measure is consistently within several days of snowoff deduced from ground data and other satellite products (MODIS- optical and QuikSCAT-active microwave). DAV is also closely linked to discharge (Kopczynksi et al., 2008). We added the following sentence to the paragraph on the method's limitations (last paragraph of the data and methods section):

"The derived melt timing metrics are measurements of the snow surface and do not account for variation of melt percolation within the snowpack or stratigraphic dynamics such as described in Marsh and Woo (1984)."

Moving to scales larger than stratigraphic, what about the heterogeneity of landscape due to forests and canopy? How does this impact the signals? I would guess that this is the single biggest element varying across the YRB landscape. Tree wells and bare patches are the rule as the forest snow melts out. There are many papers that discuss how snow melts in a mixed patchy forest, but a brief look at Giesbrecht and Woo (2000) (WRR 36, No. 8) would suggest just how varied the wetness state of the snow might be during this type of melt. Given the massive averaging going on in a pixel that is 25 by 25 km, what then do the microwave metrics actually mean? The same mixed pixel signal problem also exists with respect to altitude: in very few places in YRB are there pixels above 2000-m that don't also contain considerable area down in lower-lying valleys, where perhaps the snow has started to melt long before the snow up high (and often on glaciers) as started, or even more likely is just plain gone. Lastly, and about this I know the least but it seems important, there is sensor stability. . . perhaps not a big problem, but the overpass time of F08 is about 12 hours out from the overpass times of the other satellites (later in time). Potentially, when thinking about diurnal freezing and thawing, this might be a big deal and make the early data difficult to compare to the more recent data, making the results in Figure 4, Top Left possible an artifact?

These are important considerations and we thank the reviewer for discussing them. We discuss the vegetation uncertainty in the last paragraph of the data and methods section. To better address this issue we modified the second sentence to read:

"Further, the sources of error for this approach include the coarse resolution of the SSM/I data which does not account for sub-grid variability in vegetation and elevation. However, the resolution of the DEM used is far greater than the SSM/I pixel size which serves to minimize the effects of this uncertainty."

The reviewer also brings up a good point about the range of relief within a pixel. Figure 1 (below) shows that for most of the basin pixels there is relatively low relief (less than 800 meters). There are a few areas that have a larger range of elevation, specifically in the mountains/glacierized areas to the south and the northernmost basin margin. Relief can contribute to the signal with variable land surfaces interacting radiatively to enhance emission (Matzler et al., 1998). However, because the melt detection approach used here focuses on the very apparent increase in T_b related to a change from dry to wet surface, the signal to noise ratio is large. We acknowledge that mixed pixels may contribute to uncertainty, but this effect would be overwhelmed by the distinct melt timing signal in the brightness temperatures that occurs when a large portion of the pixel is melting. Additionally, the paper focuses on trends and changes over time for a pixel, so the relief effect that may influence the signal will be consistent over time and not affect the trend seen.

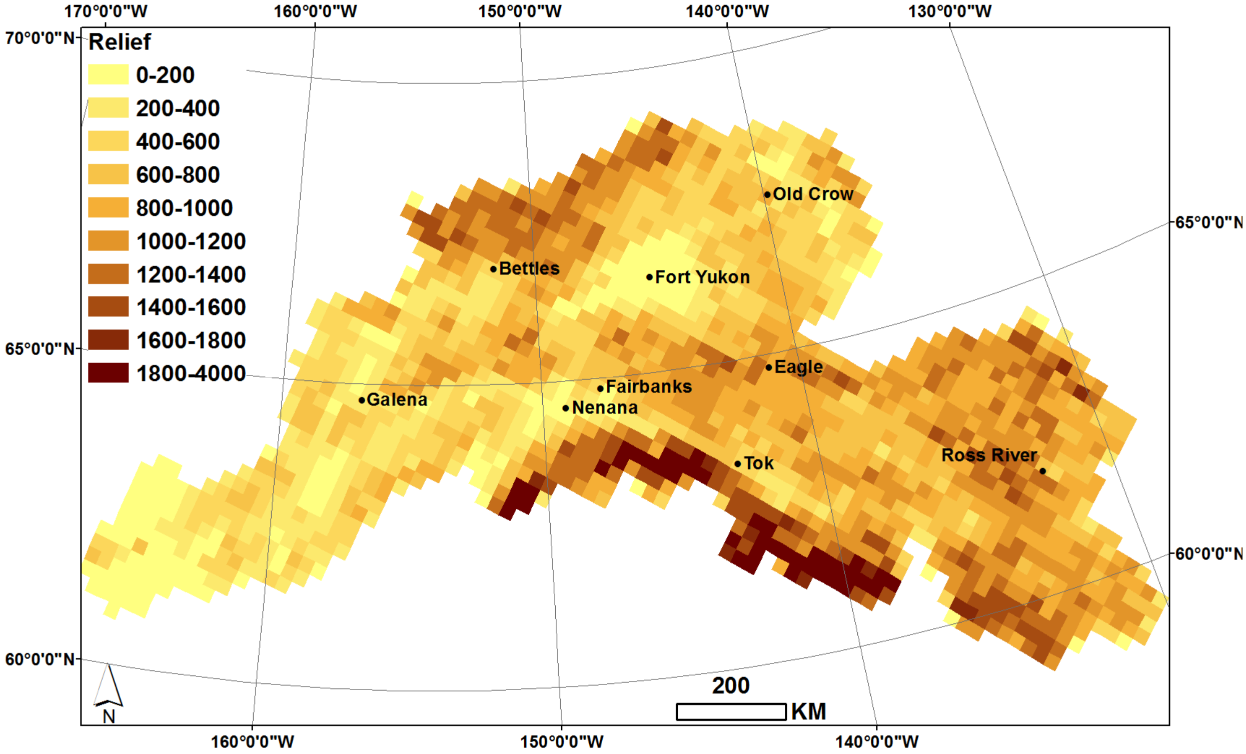

Figure 1. Relief for each SSM/I 25 km EASE grid pixel for the Yukon River Basin from a 30 arc second digital elevation map (Long and Brabets, 2002).

The issue of sensor overpass time is also an important consideration mentioned by the reviewer which we discuss in the data and methods section. Based on the literature and investigation of the dataset we concluded that differences between the sensors were minimal. We decided to refrain from calibrating so that we would not introduce unknown bias. Nevertheless, this is still a source of uncertainty that we acknowledge in the paper. In summary, the reviewer's thoughtful observations and highlighted issues are valid concerns which we consider as contributing some uncertainty to our results, but suggest these effects are minimal compared to the distinct, prevailing melt signal.

Which brings me to the central point: the only way to know if the SSM/I signals presented here are describing a something real and climatic, and not the result of sub-grid pixel affects, differences in melt regimes, effects due to snow stratigraphy and graintype, or perhaps even the intense heterogeneity of vegetation and snow distribution in each pixel, is to show that the trends are correlative or similar to other spatial data fields. In absence of this, the findings are just curiosities. . . .interesting but of unknown reliability?

We appreciate the reviewer's valid arguments related to sub-grid pixel effects and hope that the discussion below will support our interpretation that these are indeed real, coherent signals that are related to climatic factors (applied to the heterogenous landscape and snow characteristics). The melt onset signal and melt-refreeze period are corroborated

with ground station data (for several representative areas), modeling and reanalysis data (see Semmens et al., 2013), MODIS (an optical satellite from which snow cover extent (SCE) is determined) (see Figure 2 in this response below), and QuikSCAT (an active passive satellite that provides backscatter – see Semmens et al., 2013). Figure 2 (below) shows a representative timeseries for Bettles, Alaska (66.9133°N, 151.5225°W), a location that is in a different environment than the maritime environment investigated in previous papers, that nicely illustrates the relationship between the DAV signal, T_b, ground data (from the Global Historical Climatology Network), SnowModel results (Liston and Hiemstra, 2011) and MODIS snow cover extent percent. We do not show more sites (other than Fairbanks) in the paper in the interest of space and to eliminate redundancy, but we can provide more representative sites if it is deemed necessary.

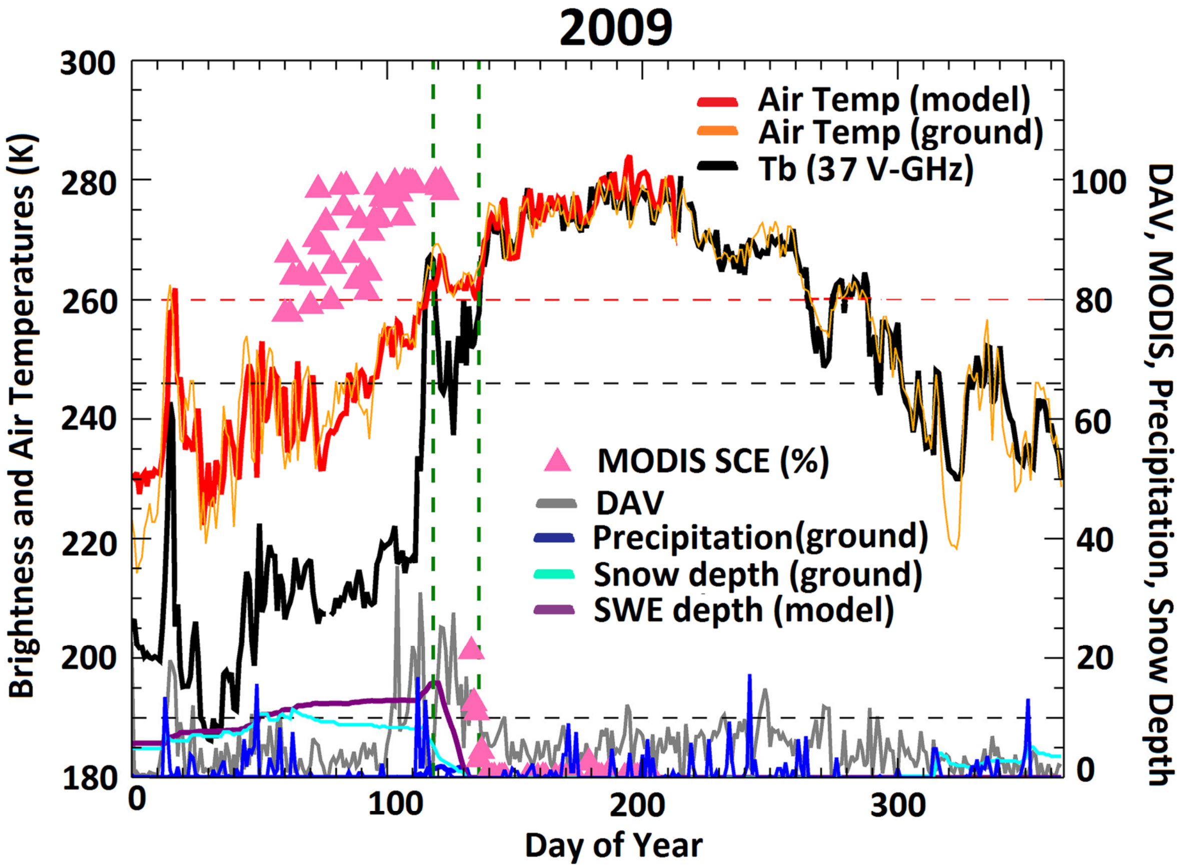

Figure 2. Time series for Bettles, Alaska (66.9133°N, 151.5225°W) 2009 showing the correlation of brightness temperature increase, high diurnal amplitude variation, drop in snow cover extent (MODIS, pink triangles), depletion of snow depth (ground, teal; SnowModel, purple) and above freezing air temperature (ground, orange; SnowModel, red).

What to do? There is another way to write this paper, one in which the authors would show greater self-criticality of their data, an essential view when dealing with remote sensing. It would be to briefly introduce the use of Tb and DAV and explain how they have been used elsewhere to define the melt period (but for smaller more homogeneous domains). Then to apply the algorithm to the YRB (which is already done) without comment on what it means. . . .simply apply it. Then to say “OK, here are delineated some trends in space and time (show us the cool results). The ask of themselves and us the reader “What part of these trends are real, and what are not?” The new part of the paper is to then get clever in sorting out the parts of the signal that is climate and the part that is not. One method might be comparison to space-time series like ECMWF products or large area temperature records. Another method might be to compare results to something like the work of Liston and Hiemstra (J. Climate, 2011) “The changing cryosphere: Pan-Arctic snow trends (1979–2009). The conclusion might be that the SSM/I melt products are good metrics. . . or perhaps they are of mixed value: perhaps the domain size over which they are applied matters. All of these would be a useful conclusions, and better than some uncorroborated results. Then (and only after establishing the degree of validity), the authors might explore what the trends show, what they mean, and why they might arise.

This is clearly a major revision, but to publish this as is, without full confidence in the spatial-temporal patterns depicted, makes little sense to me.

We appreciate the reviewer’s constructive feedback and suggestions. The comparison to a reanalysis dataset such as ECMWF is an interesting idea, which we feel would be significantly different study. We have done extensive analysis between the melt signal and other datasets, some of which is summarized here and also published in another journal (Semmens et al., 2013). In that recently published paper (freely available at http://iopscience.iop.org/1748-9326/8/1/014020), we compare and corroborate the passive microwave melt timing methodology to QuikSCAT, reanalysis data (NARR), ground station data where available, and SnowModel output.

In the TCD paper under review here, the second figure illustrates a time series compared to ground station data from a representative site (Fairbanks). We have additional representative sites across the Yukon River Basin that provide confidence in the melt signal detections (for example, see the Bettles, Alaska plot shown in this response Figure 2). All sites show the same relationships and patterns as that illustrated in the TCD paper’s Figure 2 showing the Fairbanks site. These additional site plots could be added if necessary but were not initially included to avoid redundancy and to save space. Due to the lack of a basin-wide field campaign, we use auxiliary datasets and hope to have demonstrated to the reviewer the validity of the melt signal.

An additional corroboration of the results from this paper comes from community observations. Lengthening of the shoulder seasons is a significant concern noted in the Dawson City (Yukon Territory) climate change adaptation plan (Hennessey et al., 2011). The Yukon River and its tributaries are largely impassable during the shoulder seasons (the melt-refreeze spring transition period is considered a shoulder season), as the rivers are either in the process of freezing or breaking up, and the banks are unstable. These communities report the shoulder seasons have lengthened in recent years similar to the TCD paper’s findings that the melt-refreeze period is longer. We added the following sentence discussing this corroboration at the end of the first paragraph in the discussion and conclusions section,

"In addition, the lengthening of the melt-refreeze period is reported as a current concern for local communities due to the longer duration of the spring shoulder season when river bank instability makes transportation via river difficult (Hennessey et al., 2011).",

and moved the sentence starting with "Melt onset is most strongly correlated with…" to the beginning of the second paragraph

We hope that the multitude of diverse datasets that corroborate the passive microwave signal, melt detection, and trends strengthen our discussion and conclusions. In summary, we are appreciative of the in depth and thoughtful suggestions from the reviewer, and we hope that the changes and discussion that we provide here will lessen concerns about whether the signal is real and important.

References

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