The Cryosphere Discuss., 6, C2978–C2981, 2013
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The Cryosphere Discussions

Interactive comment on "Recent changes in spring snowmelt timing in the Yukon River Basin detected by passive microwave satellite data" *by* K. A. Semmens and J. M. Ramage

Anonymous Referee #1

Received and published: 11 February 2013

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).

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 pa-

pers 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℃ (contrast this with -40℃ 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 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?

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?

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 grain

type, 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?

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.

Interactive comment on The Cryosphere Discuss., 6, 4455, 2012.