

Interactive comment on “Longer spring snowmelt: spatial and temporal variations of snowmelt trends detected by passive microwave from 1988 to 2010 in the Yukon River Basin” by K. A. Semmens and J. M. Ramage

Anonymous Referee #2

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General comments:

The authors present an analysis of spatial and temporal trends in dates of melt onset and end of melt over the Yukon River Basin from SSM/I passive microwave brightness temperature data over the period 1988-2010. The literature has seen a number of papers exploring this theme in recent years (e.g. several papers by Takala and colleagues not referenced in this paper; Tedesco et al, 2009; Wang et al. 2011...) so this reviewer was wondering what new insights this paper might possibly provide. In the introduction the authors' explanation of the uniqueness of the paper is that the focus on a river basin

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permitted “a more detailed investigation of trends and governing factors, especially ... elevation differences”. The authors hypothesis, if I understand it correctly [lines 10-11, page 719], is that trends will differ based on elevation, and that documenting this is important for understanding the hydrological response of the region to warming (the latter connection is nicely summarized and presented in the Introduction).

Ok so now we have a glimmer of hope that this paper will do something more than just report trends over a short period of satellite data. However, there's a bit of a problem. ... the authors divide the basin up into 200 m elevation classes but the satellite pixels are 25 km x 25 km. We then learn that the methodology employed [lines 19-25, page 721] assumes that the terrain is relatively homogeneous. The authors dismiss most of these problems as mainly pertaining to passive m/w estimates of snow water equivalent and happily continue without any attempt to ground-truth the ability of their method to function correctly over the study domain and resolve the elevation-dependencies they are looking for. At this point, further discussion of the paper is largely irrelevant because I now have serious questions about the credibility of the results.

I have added a few specific comments below, but you cannot expect to publish a paper focused on documenting elevation-dependencies in trends when there are known issues with the data in mountainous terrain, when your study resolution is two orders of magnitude smaller than the satellite, and where no attempt was made to ground truth the data.

Specific comments:

- There is no evidence of any ground-truthing, a major oversight that you share with Yang et al (2009).
- The definition of the variables is not clear. A diagram showing the temporal evolution of the brightness temperature and the corresponding snowpack properties would help. You also need to be more precise in the text. For example, the following phrase gives the impression that the end of the melt-freeze period corresponds to the snow-off

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date. It corresponds to the date when the snowpack is isothermal.

[Page 717, lines 5-9] The timing of this high diurnal variation period of melt-refreeze affects the progression of meltwater through a basin, corresponding to the snow off date. . .”

- The methodology was not presented for the power spectrum analysis e.g. what windowing was used, what method was used to identify significant frequencies. It would be instructive to show an example plot of the computed power spectrum with the associated confidence intervals.

- There is no justification provided for the selection of 7-years for the moving trend analysis. I do not find this to be very useful as it amounts to a verbal description of a filtered time series. This is the first time I have ever encountered the term “sub-trend”. It might be more interesting to look for break-points using statistical tests for homogeneity but even then your time series is rather short for trend analysis. You could get around this shortcoming by placing it in a longer context using downscaled climate station or reanalysis data.

- Fig 1: I had to zoom this 500% to read anything. The plot titles are confusing as they are placed along the x-axis. What does the black colour mean? It is not shown in any legend. I think the results would be easier to understand if you provided contour maps of the gridded output (it would also get rid of the need to identify all the sub-basins).

- Fig 2a is too small and does not readily convey information on the absolute elevation ranges over the basin

- Figs 2b and c. I suspect the differences in peak cycle are probably noise, given the relatively short period of data, the lack of information on the method for determining significant frequencies, and no clear physical explanations for the differences in frequencies. If the authors really want to make a contribution to understanding the elevation response of snow to climate variability and change there is a large volume of

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literature they should consult, none of which is cited in this paper e.g. Beniston, Hantel, Phil Mote, Stewart, Rangwala. . .

I'm also unclear how useful this information is. The frequency analysis was not identified as a key objective in the introduction and there are no examples given in the text outlining what this information contributes to. Over northwestern NA one might expect to see PNA and PDO signals in melt-related variables based on previous work. However, my experience with frequency domain analysis of snow cover is that the results are dominated by decadal and multi-decadal variability with few robust relationships between snow cover and atmospheric teleconnections.

- Fig 3. This also looks like noise. . . again, how robust and useful is this information?

Interactive comment on The Cryosphere Discuss., 6, 715, 2012.

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