

Interactive comment on “Snow mass decrease in the Northern Hemisphere (1979/80–2010/11)” by Z. Li et al.

Anonymous Referee #1

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General Comments

From the title, this paper claims to provide a definitive assessment of trends in the total amount of water stored in the seasonal snowpack (SWE) over land areas of the Northern Hemisphere. What the paper actually does is provide an estimate of SWE trends from essentially a single source (satellite passive microwave) evaluated against surface snow depth observations made at open locations (a biased sample), that systematically underpredicts SWE for values above 100 mm, and that ignores mountainous regions of the NH with the largest snow accumulations. SWE is a challenging variable to monitor at the hemispheric scale: in situ observations are variable in space and time (tend to be concentrated over mid-latitude watersheds); satellite-derived estimates of SWE from satellite passive microwave data have well documented problems (wet snow, areas with

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extensive depth hoar, deep snow); reanalyses and reanalysis-derived reconstructions are strongly dependent on precipitation which varies considerably across datasets. The bottom line is that any attempt to provide a definitive estimate of how hemispheric land seasonal snow water storage has changed over the past 40 years really has to look at multiple sources of information to generate some sort of consensus (and estimate of uncertainty) as no one source is likely to be give a completely reliable picture. As it stands, this paper is not publishable because the results are not supported with independent results. One potential solution may be to clearly delimit the study and results to NH land areas where the PMW retrievals can be shown to reproduce observed variability and change in SWE (i.e. non-forested regions with SWE < 100 mm) and modify the title accordingly e.g. “Snow mass trends in shallow seasonal snowpack. . .” The paper also requires extensive revisions to address gaps in the methodology and improve readability.

Detailed comments

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Abstract:

Line 2: Climate change is driving the snow cover changes not the other way round.

Lines 4-5: The statement that “reliable data on trends in snow cover is lacking” is misleading. This may be the case for SWE but snow cover extent trends for NH are published in the recent IPCC assessment.

Line 6: The statement “Here we verify the accuracy” is too definitive given the limitation and uncertainties in the surface observations. Suggest you replace this with “Here we evaluate three existing. . .”

Line 8: The term “new SWE product” is misleading. You have merged information from two existing products which is not the same as creating a new product.

Lines 14-15: Suggest you replace this last sentence with something like “Tempera-

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ture increases over NH land areas of 0.17°C/decade over the 1979-2011 period are considered to be one of the main drivers of the observed SWE declines.”

Introduction

Line 17: Suggest you replace “The world’s snow cover... and plays...” with “Snow cover plays a crucial role in the global climate system and regional water supply...”

Line 20: Why do you stop at 2006? The evidence from the recent IPCC assessment and BAMS State of Climate suggests the hemispheric SCE is continuing to respond to warmer temperatures.

Line 22: SWE is a more comprehensive than what?

Line 24: Please use the definition for SWE from the International Classification for Seasonal Snow on the Ground “the depth of water that would result if the mass of snow melted completely (Fierz et al., 2009)”

Fierz, C., Armstrong, R.L., Durand, Y., Etchevers, P., Greene, E., McClung, D.M., Nishimura, K., Satyawali, P.K. and Sokratov, S.A. 2009. The International Classification for Seasonal Snow on the Ground. IHP-VII Technical Documents in Hydrology N°83, IACS Contribution N°1, UNESCO-IHP, Paris.

Lines 24-26: Liston and Heimstra (2011) provide one estimate of pan-Arctic SWE trends from a reanalysis-driven reconstruction. This is not definitive and has to be placed in the context of other estimates.

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Line 1: What do you mean by “the variation in SWE differed in place and time”? Are we still talking about Liston and Heimstra? I think you need to start a new paragraph here on data sources for monitoring SWE. You also need to modify your numbering scheme for the three datasets [you mention three but only have two numbers]. Why do you only consider PMW data? You do not provide any justification for using solely PMW-

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derived SWE. There are extensive in situ SWE data available from Russia (Bulygina et al. 2011) for evaluating SWE trends over Eurasia.

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Line 10: Suggest you modify this to read “In situ snow depth measurements from 7388 surface stations in the GHCN-D dataset were used to evaluate the monthly SWE products”.

- How did you convert the snow depth to SWE? Did you assume the same fixed 0.24 density as GlobSnow?

- In situ snow depth observations are mainly made at open sites and may not be representative of snow conditions under the prevailing vegetation cover. This means your evaluation is based on a biased sample.

- You do not provide any evaluation of the ability of the merged PMW data to capture interannual variability in SWE i.e. comparison of regionally-averaged SWE time series. You could use the available in situ SWE observations from Russia for this purpose. They do not cover the entire NH but allow an evaluation of SWE retrievals by snow climate region.

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- You need corroborating information from other sources to support your trend estimates (e.g. in situ estimated SWE trends from GHCN-D snow depth obs, trends from Liston and Heimstra (2011) reconstruction, published trends from other studies such as Bulygina et al. 2011).

Bulygina et al. (2011) Changes in snow cover characteristics over Northern Eurasia since 1966. Environ. Res. Lett. 6, 045204 (10pp), <http://dx.doi.org/10.1088/1748-9326/6/4/045204>

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- Citing Gan et al. (2013) as validation for your results is not acceptable as this is not an independent data source. You should also be aware that Gan et al (2013) did not validate the PMW derived SWE estimates used in their paper.

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Line 1: I do not understand what you mean by “and the 30 mm threshold is acquired.”

Section 4.1 needs rethinking by the authors as it does not relate to any specific analysis carried out in the study and there are no clear conclusions presented.

Line 13: What global monthly gridded datasets? There can be large differences in precipitation trends between datasets.

Section 4.2: This entire section is shaky. The authors should read Raisanen (2007) before trying to attribute the SWE changes to temperature and/or precipitation.

Räisänen (2007) Warmer climate: less or more snow? *Climate Dynamics* February 2008, Volume 30, Issue 2-3, pp 307-319 <http://dx.doi.org/10.1007/s00382-007-0289-y>

Conclusions:

Line 13: Suggested rewording “. . . are evaluated with in situ snow depth observations”

Line 16: The sentence starting “Based on the validation results. . .” is not clear.

Note that the presence of regions of SWE increase and decrease under conditions of increasing trends in temperature and precipitation is an expected result (see Brown and Mote, 2009).

Brown, R.D. and P. Mote, 2009: The response of Northern Hemisphere snow cover to a changing climate. *J. Climate*, 22, 2124–2145.

Interactive comment on The Cryosphere Discuss., 8, 5623, 2014.

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