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Discussions**

Interactive
Comment

Interactive comment on "Pan-Arctic linkages between snow accumulation and growing season air temperature, soil moisture and vegetation" *by* K. A. Luus et al.
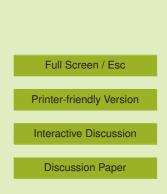
Anonymous Referee #2

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This is an interesting paper that uses the ACE technique to try to reveal relationships between annual snow metrics and various land surface properties.

I must admit that I find it hard to fully understand the relationship and conclusions revealed by the ACE technique, though this is probably due to my lack of familiarity with the technique. Therefore much of what follows may not necessarily be relevant if it is due to an obvious misunderstanding of the ACE technique on my part.

The relationships found are interesting, but reasons for them are only tentatively suggested. It should be possible to examine these suggested mechanisms in more detail using other techniques or datasets. This would probably be a lot more work and may

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not be possible, but some discussion of this would be useful (and has been carried out in some sections).

The research is justified in terms of predicting future changes, but no attempt is made to do that here (understandably as it's a tricky subject). But could you make some comment on how useful these ACE derived empirical relationships, a single variable at a time, are likely to be for predicting future behaviour. I would argue that mechanistic models would have far more predictive power, especially given the weakness of the pairwise relationships found.

Inter-annual variation in snowmass is very large. You state that your results show that a climatological approach is appropriate, but I still wonder how different the results would be if each year were treated as a separate point? I think, given the large inter-annual variation in the snow-metrics, the conclusions may well be different.

This might be me being naive, but in section 2, page 1754, you say that ACE allows you to reveal relationships that would not be visible from plots of the two variables against each other. I can't imagine what situation that would occur in with only two variables, unless the relationship were so weak that a scatterplot or mean/median values did not show a result, in which case is the relationship significant? This might be due to my lack of familiarity with the ACE technique, but some further illustration of this would be helpful.

Globsnow retrievals below 15 mm SWE are considered unreliable (Solberg et al 2010). Do you think that using this threshold rather than 0 mm might lead to a more accurate result?

The differential microwave transmission through snow used to derive Globsnow is known to start to saturate around 100 mm SWE (Chang et al 1987). Whilst Globsnow's use of ground data make it a far more accurate product than the pure Chang algorithm based SWE products, the EO portion of Globsnow will still be less reliable above 100 mm SWE. The majority of figures 3, 4 and 5 show a kink in the associations

around 100 mm SWE. Can you be absolutely certain that this is not due to an artefact in Globsnow? As I understand it, as the microwave derived accuracy reduces, Globsnow relies more heavily on ground data, therefore we might expect the error to be related to the distance to the nearest ground station and so pixels close to a weather station would be less likely to suffer from any EO artefacts than those further away. A full analysis of this would not be trivial, but some discussion of it and perhaps small scale tests at a subset of sites would be interesting and could give much more confidence in your conclusions.

In section 3.4 you state that all NTSG variables can be considered non-collinear. Can you give more details about this conclusion? I would expect some variables, certainly temperature and vegetation density, to be strongly related (by whatever non-linear function). You conclude that SWE has a very similar relationship to both temperature and vegetation density. Can you provide more proof that you are not seeing a common relationship due to correlation between the NTSG variables? If some correlation is found I think it highlights the issues of using techniques like ACE to try and explain linkages and especially for helping predictions. Could some discussion, or disproof, of this be included?

Some of the relationships are very weak. Could the paper be shortened and tightened by simply stating that these relationships are very weak and leave it at that?

The conclusions on the relationship between vegetation density and SWE are very interesting.

Minor points:

In section 3 you state "results from preliminary analysis indicated that ACE is well suited...". Are these the findings of sections 3.2, 3.3 and 3.4? If so perhaps wait until then before stating the conclusions. If not could you give any details on how this preliminary analysis indicated ACE's suitability?

The official paper to reference for Globsnow is;

Takala, M and Luojus, K and Pulliainen, J and Derksen, C and Lemmetyinen, J and K{\"{a}}rn{\"{a}}, J-P and Koskinen, J and Bojkov, B, 2011. Estimating northern hemisphere snow water equivalent for climate research through assimilation of space-borne radiometer and ground-based measurements. Remote Sensing of Environment, v 115, p 3517-3529

You talk of snow blowing causing a link between SWE and vegetation density; do you believe that is significant at the 25km scale?

You've used Globsnow v0.9. Version 1.3 is available on the ftp site and v1.0 was released some time ago. There is no need to redo all the analysis, but could some comment on this be made and perhaps a quick look to see if any differences between v0.9 and the operational releases (v1.0 - v1.3) might impact on your conclusions (hopefully they won't).

Figure 1 is very small and hard to read, could it be made bigger? The same for figures 3, 4 and 5.

Section 3.1: "Regions with fractional water content >0.5" - do you mean land surface cover or a SWE or vegetation water content? Please make it clear.

Section 4.3: Is very low 90 mm low SWE? It's not very high, but it's certainly not low for most Arctic catchments. At different points you describe very low SWE as 75 mm, 90 mm and 100 mm. Globsnow is not very reliable above 150 mm and you show hardly any values above 200 mm SWE. I'd therefore argue that these are "lower" values of SWE but not "very low".

References: Chang, A T C and Foster, J L and Hall, D K, 1987. Nimbus-7 SMMR derived global snow cover parameters. Annals of Glaciology, v 9, p 39-44.

Solberg, Rune and Amlien, Jostein and Koren, Hans and Wangensteen, Bjorn and Luojus, Kari and Pulliainen, Jouni and Takala, Matias and Lemmetyinen, Juha and Na-

gler, Thomas and Rott, Helmutt and Muller, Florian and Derksen, Chris and Metsamaki and Bottcher, Kristin, 2010. Global snow monitoring for climate research; design justification file. European Space Agency contract report, ESRIN contract 21703/08/I-EC, deliverable 1.7.

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