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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 #3

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Pan-Arctic linkages between snow accumulation and growing season air temperature, soil moisture & vegetation

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Luus and colleagues use a novel statistical technique to determine the strength of associations between SWE and variables describing growing season conditions over a 5 year period (2003-08). This is an interesting question, particularly at a pan-Arctic scale, and is suited to the remit of Biogeosciences.

The statistical technique, Alternating Conditional Expectation (ACE), is applied in a novel manner to highlight the strength of non-linear relationships between variables. There is a good explanation of the ACE method, including a useful example (Figure 1) and the difference between ACE and linear regression (Table 5 vs Table 3) are presented. Statistical associations between: 1) SWE vs air temperature, 2) SWE vs soil moisture, and 3) SWE vs vegetation transmissivity, are discussed in turn. Some causes are speculated upon, and in situ field studies are contrasted to support the interpretations made. I regret to say I feel the paper in its current form is undermined by the following points:

1. GlobSnow SWE is primarily derived from passive microwave measurements. While SWE retrievals from passive microwave may be useful in geographically defined areas, where there is vegetation (especially boreal forest) the retrieval signal becomes complicated. Use of the GlobSnow SWE product in such vegetated areas will lead to unconstrained uncertainties and, potentially false conclusions as a result.

2. Passive microwave in tundra areas (non-forested) saturates where the snowpack changes from being a gross scattering medium to an emitter (e.g. see Fig 3 of Derksen 2010) between approximately 50 to 100 mm SWE. This will strongly influence the analysis presented – does this account for the breaks in slope / inflection points in the SWE graphs in Fig 3 – 5? Attention to detail of this kind is required to enable the reader to have confidence that the statistical associations presented are due to real changes in SWE, rather than as a function of the measurement technique.

3. How to describe the strength of a statistical association (weak, moderate, strong etc.) can be a moot point. However I would strongly contend that R-squared values <0.4 are all weak. The presented analysis focuses on small differences between different weak relationships, which are not appropriate.

4. When comparing linear regression (Table 3) and ACE (Table 5) the range in R-squared values vary between 0 to 0.28 (linear) and 0.02 to 0.38 (ACE). All are weak

BGD

10, C616–C619, 2013

values and there is not a great deal of difference between ACE and linear when compared in this manner. Could the paper refocus on this to more definitively show that ACE is a superior analytical tool?

5. Why is mean SWE used rather than maximum SWE? Maximum SWE is of much greater hydrological and biological interest, especially with respect to how snowmelt influences the subsequent growing season.

6. What is meant by the 'start and end' of the snow season. Both right at the start and right at the end of the snow season will show low SWE values. As seasonal accumulation results from a series of temporally distinct precipitation events the seasonal mean (or preferably the maximum) SWE is of more use than SWE values at hard to define start and end periods.

7. Even through I re-read section two multiple times I am struggling to interpret what Figs 3 to 5 actually mean. The patterns in these figures are only briefly discussed in each part of section four. It suggests that the authors are also struggling to interpret them too. Perhaps fewer plots with a more detailed discussion of the patterns for each of the three conditions would be more appropriate?

8. Thirty days as a period to average conditions over seems arbitrary. A more detailed justification should be considered.

9. Causal interpretation of the statistical relationships are exceptionally vague. Vague qualifiers that prefix the suggested causal mechanisms (e.g. likely, may, generally etc.) are indicative of the fact that interpretation using examples from in-situ measurements in published literature are highly speculative and easier to prove wrong than corroborate (e.g. confusion of shrub processes with those in boreal forests).

This is a paper with a useful new application of a statistical technique, which initially appears to be an intriguing method to help us quantify non-linear associations. However, in short, I feel like the paper in its current form tries to do too much too soon.

BGD

10, C616–C619, 2013

I would suggest the authors consider a re-write focussing on temporal changes in the use of ACE over a smaller spatial defined area, where there are enough ground-measurements to enable a more justifiable causal interpretation with hopefully more robust statistical associations.

Derksen, C., Toose, P., Rees, A., Wang, L., English, M., Walker, A., Sturm, M., 2010. Development of a tundra-specific snow water equivalent retrieval algorithm for satellite passive microwave data. Remote Sensing of Environment, 114(8): 1699-1709.

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