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Biogeosciences Discussions

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 #1

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The aim of the paper is to investigate the relationship between snow accumulation and growing season air temperature, soil moisture and vegetation at pan-Arctic scale using satellite data. These relationships are investigated using the Alternating Conditional Expectations (ACE) method, an approach which allows to identify non-linear relationships between predictor and response variable. Following a clear description of the methods and datasets used, the authors present and discuss the results in 3 sections, each dealing with one of the investigated relationship. The authors conclude that, as the ACE method corroborated linkages which had been identified through field measurements, it should be used in collaboration with field scale studies to identify larger scale relationships between variables.

The manuscript is well written and the authors clearly guide the readers through sections which may have been otherwise difficult to understand, most notably Section 2. However, while the method used is novel and therefore deserves to be published, the explanations to justify the often weak relationships identified by the ACE approach fail to be convincing. There are two main weakness in the paper: The first one lies in the authors failing to acknowledge that most of the relationships are weak and have long and often erroneous explanations to justify the results. The format of the paper probably does not help either. It would make matters clearer if the results were separated as they currently are but if a discussion section followed a results section. A section dedicated to presenting the results would most probably force the authors to acknowledge the weakness of the relationships and to have a more focused discussion as to why these relationships are weak. The second, which is very clear in Section 4.3.1. is that it seems that the weak relationships force the authors to "guess" why these relationships may occur but they often misinterpret previous studies, leading their conclusions and explanations to be incoherent.

As a consequence, I would suggest major revisions before the manuscript be considered for publication in Biogeosciences but would encourage the authors to pursue as the ACE method is little known but has potential to be useful in understanding the complex relationships at the Earth surface.

Issues to be addressed:

Section 3.3: "The distributions of mean 1982-2000 net primary productivity (in grams of carbon/m2/y) from the Global Production Efficiency Model (GloPEM) (Prince and Goward, 1995) appear to correspond with the spatial patterns of vegetation classes selected for this study". Is this a visual analysis using Figure 2a-f? If so, should it really be called "analysis"? If there has been a statistical analysis, it would be useful to have the qualitative results. However, assuming that the analysis is indeed visual, I disagree with the statement. I don't find that Figures 2e and 2f obviously match. A different use of colour for the vegetation classes which would correspond to NPP may help (e.g.

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high NPP values are found in the EVGREN class which may be more clear if EVGREN were in red). Besides, given the otherwise statistical nature of this manuscript, it seems strange that the analysis is purely visual and "appear to correspond" is very vague. I don't think that statistical analyses are required, the authors could simply reference previous studies which relate NPP to vegetation classes, as they do to explain Figs 2a-d. Unless the relationship between 2e and 2e can be made visually clearer, I don't think that Fig 2e is necessary / useful. If the authors would prefer to keep Figure 2e then a descriptin of the GloPEM data is required

Results and Discussion Section: What are the boundaries between weak and moderate? While there may not be universally accepted boundaries and would thus accept 0.25 as moderate, many would argue that 0.17 does not constitute a "moderate" relationship but a weak one. I believe that it would be wiser to describe some of these relationships as relative relationships e.g. simply "these associations are weaker over forested regions (ACE R2=0.04-0.15) than north of the treeline (ACE R2=0.12-0.17)". Overall, all the relationships in Table 3 would be considered weak by many at the exception of the 4 that are greater than or equal to 0.3. While the results should indeed be put in a wider scientific context, it does little to convince the reader that the ACE approach can provide us with "new" information. Section 4.2 (and its sub-sections) are in effect discussing that the ACE approach finds results which agree with other field observations but that, without these field observations, the ACE approach could not explain what drives these (weak) relationships.

Figures 3-5 and Table 3: It would be helpful to have the plots showing the relationship between the variables before the ACE approach (as in Figure 1a) to see how much of the relationship between each 2 variable could be assessed before the ACE approach. Equally, it would be helpful to have the R2 before the ACE transformation. Some of them are given in the text (e.g. 4.1 first paragraph) but others aren't. Are the ones not provided close to the ACE R2? Also, it is difficult to visually see the differences in correlation strengths between vegetation classes because of the differences in the y

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axis. If all were on the same scale, the difference would be more visually striking.

Section 4.1.2, last paragraph: "High latitude Arctic sites undergo rapid snowmelt due to the low albedo (...)of Arctic vegetation, and similarity in the timing of the summer equinox and date of snowmelt (Bonan, 2002). As these effects are stronger at high latitude sites, it is reasonable that the most rapid snowmelt would be observed in regions north of the treeline." This is a very simplistic and somewhat erroneous explanation. During the snow season, when there is snow on the ground, the higher the canopy, the lower the albedo (see Loranty et al., 2011, Figure 3 and Table 1 for remotely sensed data and Pomeroy and Dion (1996) for measured albedo above forests and Pomeroy et al. (2006) for albedo of tundra and shrub-tundra). I suspect that, as also suggested by the authors, "low shading" is far more likely to be the reason for this difference (see e.g. Pomeroy and Dion for effect of shading on below canopy shortwave attenuation and snowmelt)

Section 4.2: "an examination of the shape of these non-linear associations indicates a positive association between mean annual SWE and soil moisture at sites with very low SWE (<90mm) and low soil moisture (<0.17),". How much stronger than the ones shown in Table 3 are these relationships? Given that the relationships in Table 3 are all weak, if the ACE R2 are considerably more significant for low SWE and low soil moisture it would strengthen the discussion in Section 4.2 to show them.

Section 4.3.1. paragraph 1: "indicating that locations with slightly more vegetation received much more snow annually". Did they "receive" more snow or did they lose less to sublimation? Areas where shrubs protrude above the snowpack in winter lose less snow to sublimation than low vegetation barren or graminoid tundra because shrubs increase the snow-holding capacity of the tundra by decreasing the near-ground wind speed downwind of and within shrub patches (see Sturm et al., 2001a). This process is mentioned later in this Section in the first sentence of paragraph 3 (although "vegetation biomass" is too vague and should be replaced by "shrub cover") but should probably be mentioned earlier.

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paragraph 4: "the primary driver of snow accumulation in shrub-dominated regions is topography rather than the height or density of aboveground biomass." Is it? Could there at least be one reference to back up this statement? While some studies have found correlations between shrub distribution and topography (e.g. Ostendorf and Reynolds, 2003 and Naito and Cairns, 2011), I am not aware of any that has explicitly stated that snow accumulation in shrub-dominated regions was dominated by topography rather than shrub cover (density and height) on the contrary (Sturm et al. 2000, Essery and Pomeroy, 2004). "Snow accumulation at shrub-dominated sites (...) tends to be greater in flatter valleys than on hill slopes". I can't see that any of the referenced paper corroborate the statement they support. From MacDonald et al. (2009):"Pomeroy et al. (1997) found that a total winter snowfall of 190 mm SWE (snow water equivalent) in a low Arctic catchment produced maximum snow accumulations of 68 mm, 252 mm and 617 mm SWE on tundra, shrub tundra and steep slopes, respectively. McCartney et al. (2006) measured maximum seasonal snow accumulations of 102 mm, 229 mm, 164 mm and 201 mm SWE in short shrub, tall shrub, windward slopes (south-facing) and leeward slopes (northfacing) in a subarctic alpine tundra catchment. (...) Pomeroy et al. (1997) applied an empirical monthly index blowing snow model to landscape units in the low arctic, redistributing snow from sparse tundra to shrubs and topographic depressions and hillsides using a source-sink approximation." Don't these suggest the opposite of the statement?

"Arctic sites with tall, thick-stemmed shrubs have been observed to undergo snowmelt more slowly than regions with shorter vegetation, as shorter plants remain buried within the snowpack at the end of the snow season, resulting in greater albedo and less absorption of solar radiation (Sturm et al., 2005)." Doesn't this sentence contradict itself? If "slowly" should be replaced by "quickly", as suggest findings by Sturm et al., 2005, Pomeroy et al. 2006, Bewley et al, 2010, Marsh et al., 2010 then this sentence should altogether be removed as it does not corroborate the previous sentence in the manuscript.

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"Field studies have indicated that interception and sublimation by dense canopies diminish snow accumulation (Pomeroy et al., 1999; Lundberg and Halldin, 2001; Pomeroy et al., 2002)"...yes, over boreal forests because trees intercept snow which is then sublimated..." and it is likely that these processes likewise limit initial snow accumulation over regions containing greater densities of shrubs"...No! Shrub-snow interactions largely differ from tree-snow interactions. Shrubs do not intercept snow, they either protrude above the snowpack and shed the snow or become bent and buried. Boreal forest lose snow through sublimation, shrubs prevent sublimation. There are dozens of papers dealing with either boreal forests or shrub processes, some of which are referenced below. For example, if the interactions between snow and trees or shrubs were identical, Menard et al. (2012) would not have had to write a shrub bending model but would simply have used the Pomeroy et al. (1998) snow interception model.

Section 5: "Field studies have found that snowmelt occurs more gradually over regions with more vegetation due to shading and slowed wind speeds (Metcalfe and Buttle, 1998), and that snow accumulation is diminished over regions with greater canopy density due to interception and sublimation (Pomeroy et al., 2002)." This is only true if one compares boreal forests and tundra. If one considers shrub-dominated landscape, i.e. the vegetation class between these with regards to "more vegetation" or "greater canopy density", it is not. As mentioned in the previous comments, snow accumulation and snowmelt rates are greater in shrub-dominated landscapes than in the tundra or boreal forest.

Appendix A: Potential errors in the datasets are clearly identified but at no point in the discussion are the uncertainties in the ACE R2 related to potential errors in the datasets. For example, the soil moisture product was found to be correlated with precipitation $0.2 < r < 0.8$. Does this show potentially large errors in the dataset where $r = 0.2$? The NTSG air temperature product has "[l]arger errors were observed in regions with sparse vegetation, higher elevations and higher fractional cover of open

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water on land.". How can these errors affect results from the shrub vegetation class as shrub-tundra is by definition sparse vegetation and often situated at high elevation (as well as high latitude)? How reliable is the soil moisture product where vegetation is high if "Vegetation acts as an attenuating layer that diminishes the transmissivity of passive microwave radiation." Should areas where there are potentially large errors in the remote sensing products be masked out of the study? Given the weak ACE R2 and sometimes surprising relationships, it is somewhat odd that the authors take the datasets as "truth" and never question the effect of their potential errors on the ACE results.

Minor comments: p3, sentence 2: "(...) and that the presence of vegetation increases snow accumulation near shrubs due to wind trapping (...)". This sentence is confusing and needs rephrasing. Snow accumulation near shrubs occurs because shrubs reduce wind speed downwind of the patches, thus causing blowing snow particles to lose their momentum and settle.

p3: "The passive microwave data products analyzed in this study were created by researchers at the Finnish Meteorological Institute (FMI) (...)". The Globsnow products were developed by an FMI-led consortium (http://www.globsnow.info/index.php?page=Home), not single-handedly by FMI researchers. This should be made clear.

p4: Sentence starting by "The aims were to examine". This is a very long sentence (8 lines). Could bullet points be clearer?

p4: "Findings indicated that the linkages and thresholds (...)". Which findings? If from this study, this should not be in the introduction. If from previous findings, there should be references and a clearer description of what these "findings" are.

Section 2. This section is overall very clear and the example helpful.

Section 3.2, last paragraph: Define NPP and give details of the NPP dataset.

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Section 3.4: "associations between the mean values of SWE over the last thirty days of the snow season". Do the authors mean "associations between the mean values of SWE over the last thirty days preceding the onset of snowmelt" or "(...) preceding the first snow-free day". The sentence suggests the latter in which case, how did the authors calculate mean SWE given that p23 the authors mention that "In the GlobSnow algorithm, regions with thin snowpacks, snowmelt or wet snow are masked out, as wet snow acts as a microwave emitter (Armstrong and Brodzik, 2001)."

Section 4.1.2, last sentence: "Furthermore, as air temperatures similarly show a strong gradient [Figures 2a-f], it is reasonable that the regions in which snowmelt occurred most rapidly would also be those in which air temperatures following snowmelt were coolest. " This sentence is confusing: snowmelt occurs more rapidly North of the tree-line but the sentence suggests that this process is the reason why air temperatures are coolest after snowmelt. They are cooler because of the latitude, not because snowmelt was fast?

Table 1: "Prostrate dwarf-shrub" is found in both SRBTD and GRMTD CAVM-derived vegetation classes.

Figures 1 and 3-5: The y and x labels are very small and hard to read.

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