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The Cryosphere Discussions

Interactive comment on "Effects of interannual variability in snow accumulation on energy partitioning and surface energy exchange in a high-Arctic tundra ecosystem" *by* C. Stiegler et al.

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This is useful contribution to the arctic surface flux observation literature as the study documents a nice case study of control of the amount of snow (snow depth) on the subsequent evolution of the turbulent surface fluxes and melt. As such, and also given the amount of work it involves to get the data in such an environment, it deserves to be published. I do have some comments however, some major, some minor, that I hope may improve the paper.

Main comments.

First. The paper contains an awful lot of numbers, and no error estimates at all. In the

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table the SD is given, but if that would be a good measure (and we know it is not), most of the data would fall within the same probability distribution. My first suggestion would be to abandon the use of SD and give the range, and give an error estimate of all your measurements. That helps to assess the significance of your difference.

Second. The energy balance closure is vital to the whole exercise in calculating the amount of energy available for melt as a residual from the energy balance. While I agree that a considerable amount of variability is expected, a value of 67% is very low and needs a little more explaining that referring to site heterogeneity. To show the validity of the eddy covariance measurements I would suggest to include a spectral analysis of the measurements. A good co-spectrum adds to the reliability and acceptance of the data. I also suggest to include this analysis in the description of the methodology, where it belongs, and not include as as an afterthought in the discussion.

Third. Soil moisture. In terms of controlling factors, soil moisture is the key term that controls the partitioning of the energy budget terms. Precipitation and snow depth are just proxies in that sense. I am surprised that only in Figure 6 soil moisture is used. In fact how it is measured is not mentioned at all in $ 2.2. Was it only measured at the dry heath. Please explain and use the data!

Fourth. Overall the analysis is very descriptive, even lacklustre at times. This is a pity as the data are very valuable! For example when parameters like surface resistance of omega are calculated there is no real effort to explain or interpret these (I find the big difference between the yearly wet fen values somewhat worrying though, given the magnitude of the difference; even between different vegetation types you would not expect such a big difference). This really needs some work. For instance if a wetter Arctic would imply less H, would that provide a negative feedback on the warming trend? There are plenty of such questions to ask given the data and I encourage the authors to think these through and by doing so add more meat to the discussion. I am also surprised to find that there is no mention of the Kasurinen et al., 2014, GCB study at all, given some of the co-authors participated in that study. This does provide a very useful benchmark for the present study.

Fith. I would rephrase the title to make it a better aligned with the content. Something like a “A comparison of surface energy budgets of . . . in two years with extreme and little snowfall”.

Minor comments

P1 l10. The use of interannual suggest that many years are used. I would suggest to not use this word and stick to the comparison of a snow rich and snow poor year (see above remark on title as well). P3 l27-30. This should be part of the introduction, not site description. P4 $2.2 Measurements. Have the systems be run side by side in a comparison experiment to show that they provide the same fluxes when at the same site? What are otherwise the errors you expect in the fluxes? P5 l11-13. Is there a way you can quantity the error that this would generate on your estimates of energy available for melt. P6 l28-30. I would guess that synoptic variability and weather dynamics also play a role here. You are expressing an extreme 1-D view of the atmosphere here. P7 28-29. This is an example of my first major comment. Are these values really different, or do they fall within you measurement error (given your energy closure for instance)? P8 l16 gives an another example. P8 l26-27. Is there any way you can relate this to greenness, density of vegetation as well, or is this really just an effect of the relative contribution of snow versus vegetation. I am asking also in relation to Fig 1, where a different colour seems to be visible for the different years. p9. $3.3.4. This part really needs some more work and check on the values of Rs in the dry heat growing season. Also it may be better to define a period of maximum gs, rather that show the average which is biased by the shoulder values of the season. Reference here also Kasurinen et al, 2014. This part is presently pretty shallow, I am afraid to say. P12 l27. I guess you mean soil moisture rather than groundwater? Otherwise how did you determine this? Figure 3a and 3b. Can you add the snowmelt of 3b just to 3a? This avoids repetition of the albedo and snow depth and temperature plot. You adjust the time period to the longer one of 3b. Figure 5. Can you reduce the scale of the

Y-axis in the lower panel plots so that differences are more visible? Figure 6. Can you add a second y-axis that gives the % of use of available energy for the different fluxes? Figure 6b is confusing as it does not fit with 6a (it shows both the dry tundra and wet fen and both years) and the structure of all the other plots. Make it a separate plot.

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