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Interactive comment on "Carbon stocks and fluxes in the high latitudes: Using site-level data to evaluate Earth system models" *by* Sarah Chadburn et al.

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This paper evaluates the ability of 3 land surface schemes used in earth system models to simulate carbon stocks and fluxes at 5 Arctic sites. The exercise is timely and welcome, given that permafrost carbon feedbacks are not included in most climate projections. The breadth of activities dealt with in this paper is very large and includes Arctic field measurements and observations, model developments and model runs. Topics include physics, chemistry and biology, and this work needed the varied competences of tens of authors. An in-depth review of this paper by a single person therefore appears extremely difficult and I first need to state the limits of my review. Although I do

use land surface schemes to model Arctic permafrost and snow, I am mostly a field scientist focused on performing detailed and sometimes complex field measurements and analyzing them. I also focus on snow and thermal processes in snow and soil, as well as on vegetation-snow-soil interactions. My review should therefore be complemented by that of a modeling expert, preferably focused on carbon aspects. Overall, I found the paper very interesting in that it shows that the current state of the art in modeling Arctic carbon cycling is clearly below our needs for reliable projections and it identifies key aspects where progress is most needed. Given the form of the paper, performing additional simulations would probably be difficult, but I recommend minor but significant changes in text organization, in the use and critical evaluation of field data, and perhaps minor modifications to the conclusions.

Model description

How about a Table summing up the 3 models main features? This would allow significant text shortening. Also please make sure equivalent information is given for all 3 models. For example, vegetation details are lacking for JSBACH. By the way, PFT is defined nowhere and some institute abbreviations are not explained (IPSL, NCSDC). I let the editor decide whether that is necessary. Lichens are not mentioned in any model description, from which I assume that they are not considered. Yet, they can be very abundant at some Arctic sites, sometimes covering most of the ground. They have physical and biological properties very different from mosses, for example a much lower thermal conductivity and different hydrological properties which strongly impacts the ground thermal and hydrological regimes. Please consider specifically mentioning this omission. A couple of sentences or a line in the future model Table to describe the snow scheme would be nice (single layer, multilayer. . .). Please also specify here that nutrient aspects are not treated in any of the 3 models.

Site description

The description of all sites should really be homogenized and considerably shortened, by at least 3 pages. What is in Table 1 need not be repeated in the text. Incomparable data are often given in the text. For example, some sites have mean annual temperature, others January and July, please be consistent. Also detail the snow fraction of precipitation in all cases: "most" is vague and not very useful. All plant Latin names must be in italics. By the way, line 219, what are the Salix? Richardsonii, arctica, other?

Field data

Measuring snow precipitation and snow depth in a reliable and representative manner is always a problem in the Arctic and the text does not convince me that this aspect was treated properly. Moreover, its impact may be understressed here since it conditions the permafrost thermal regime and therefore all carbon processes. How about details of the precipitation measurement, such as the presence of a wind shield around the gauge? I understand that precipitation measurements were not used, but since snow depth measurements are not convincing, as detailed below, perhaps analyzing precipitation data in more detail would be useful. Was there any attempt to correct measured snow precipitation as described in (Forland et al., 1996)? This can double estimates of precipitation amounts and considerably improve agreement with snow accumulation. Measuring snow depth in a representative manner is difficult. Certainly using one point measurement is inadequate. In particular, in low-centered polygons, variations are huge and at least 100 measurements are required for a representative value. Please detail the representativity of your snow depth measurements. In case the data are found to have limited representativity, this should be clearly stated and perhaps a sensitivity study would be useful (if it is still possible to perform it): what is the impact of snow amount on permafrost temperature and carbon cycling? Perhaps looking at data from reanalyses would also be helpful for an extra evaluation of precipitation and snow depth data. Are there any field measurements of snow density to validate model assumptions of this variable? By the way, snow temperature measurements at several heights can be very useful to evaluate the validity of snow schemes, and implementing those at the sites described here may be valuable for future work (Barrere et al., 2017).

Lines 327 and 329: please use "snow depth" throughout.

Results and discussion

Lines 431-432. The snow depth model output at Abisko is not "reasonable". It just does not seem to work there. Please consider representativity of field data and modify discussion.

Line 433. "snow often melting a little too early" in simulations. Ambiguous as written.

Lines 436-437. How do models account for vegetation effects on snow albedo?

Lines 457-458. How about thermal conductivity values obtained by JULES, and how do they compare with other models? Perhaps also compare with values obtained at a comparable high Arctic sites in low-centered polygons (Domine et al., 2016) if you think this supports your case. Note by the way that stratification of thermal conductivity can have an important effect, as suggested by (Barrere et al., 2017), so that one-layer snow models can give the correct mean thermal conductivity value while making a large error on atmosphere-ground heat fluxes. Incorrect snow thermal conductivity stratification can also lead to incorrect timing of ground freezing and thawing. Arctic snow often has a very low thermal conductivity layer at the base, which delays freezing and thawing. This process is missed if the snow scheme gives a high thermal conductivity to the basal snow layer.

Line 559. A word on nutrients here?

Line 574-575. "GPP depends mostly [. . .]on shortwave radiation in the second half of the season". How about moisture? For example, (Frost and Epstein, 2014) stated that "rates of shrub [. . .] expansion were not strongly correlated with temperature trends and were better correlated with mean annual precipitation".

Conclusion

The impact of mosses is stressed, but as mentioned above, I really think that lichens can have a huge impact. I gather that they are not very important at the sites studied here, but on a pan-Arctic scale, this is probably different.

Since you are talking about landscape dynamics, you may talk about the impact of lakes and ponds caused by landscape dynamics such as thermokarst lakes formation. These lakes are often hotspots of GHG emissions. See e.g. (Bouchard et al., 2015) and references therein.

Figure1. What is the meaning of mean snow depth? Spatial mean? Temporal mean? Over what period? The Abisko graph does not seem to match the mean value.

Table 1: What is summer? What is winter? Permafrost T, at what depth?

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