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The Cryosphere Discussions

Interactive comment on "An analysis of present and future seasonal Northern Hemisphere land snow cover simulated by CMIP5 coupled climate models" *by* C. Brutel-Vuilmet et al.

Anonymous Referee #2

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General comments:

The authors examine the CMIP5 model ensemble of historical snow cover extent simulations and future projections to provide insights into how well the models capture key characteristics of the current NH snow cover climate, as well as the projected rates of change in snow cover for different emission scenarios. An important finding is that the models underestimate the temperature sensitivity of NH spring snow cover extent (SCE) which combined with underestimates of boreal warming leads to a large underestimate of the rate of decrease of NH spring snow cover over the past ~30 years. Unfortunately the paper is rather haphazard in its organization, contains a number of significant errors and inappropriate analysis methods, and requires major revisions before being suitable for publication. In particular, the model evaluation is not comprehensive and only addresses the models' ability to capture the seasonal cycle in SCE and observed trends in hemispheric SCE. Important metrics of model behaviour such as maximum snow accumulation, snowpack density, and snow-albedo feedbacks are not examined, nor is there any assessment of the models' ability to capture interannual variability in snow cover. There is also no attempt to determine whether factors such as model resolution or the treatment of snowpack processes (e.g. single layer vs. multi-layer snowpack model) influence snow cover sensitivity to warming. The net result is that the paper raises an important issue but provides no insights into what is causing the models to systematically underestimate recent spring SCE reductions in response to warming.

Detailed comments:

1. Abstract line 3: The conclusion that the models reproduce observed snow cover extent (SCE) "very well" is rather generous. The models underestimate interannual variability in Arctic SCE by about a factor of two (Derksen and Brown, 2012) and the paper made no attempt to evaluate annual maximum snow water equivalent (SWEmax) or snow-albedo feedbacks. The ability to capture the mean seasonal cycle of snow cover extent is a fairly weak test of model performance.

Derksen, C., and R. Brown (2012), Spring snow cover extent reductions in the 2008-2012 period exceeding climate model projections, Geophys. Res. Lett. doi:10.1029/2012GL053387 (in press).

2. Abstract line 5: It is completely unrealistic to expect a global climate model to capture an observed trend over a precise 27 year period. The results will be dominated by the internal climate variability.

3. Abstract lines 15-17: This statement is not logical. The temperature sensitivity of SCE to temperature (dSCE/dT) depends on both SCE and T. What you are referring

to is the rate of change in SCE, not the sensitivity. The model and observed values for dSCE/dT should be reported in the abstract as these were one of your main findings.

4. Introduction: Needs to be focussed on the goals of the paper e.g. the opening paragraph discusses general aspect of snow cover that are not examined in the paper. Material appearing later in the paper (e.g. 3.1.2) should be moved to the introduction.

5. Introduction lines 18-20 (and page 3324 lines 18-20): The conclusions of Roesch (2006) are incorrect and based on an erroneous method for estimating snow pack density as a function of snow depth (see Brown and Frei 2007).

Brown, R.D. and A. Frei, 2007: Comment on “Evaluation of surface albedo and snow cover in AR4 coupled models” by A. Roesch, J. Geophys. Res., 112, D22102, doi:10.1029/2006JD008339.

6. Section 2.1: Please include a description of the emission scenarios used.

7. Section 2.2.1 lines 16-17: this statement is incorrect. The NOAA snow cover dataset has some missing data prior to 1972 (mainly in the summer months) but is complete from 1972. Roesch and Roekner (2006) made no statement about missing data in their paper.

8. Section 2.3.1 line 12: the statement that “observations are more reliable” is incorrect. If you check the papers you will find that the selection of March and April has more to do with the spatial distributions of the available observations than their reliability.

9. Page 3325 lines 23-25: It is inappropriate to compare trends from climate models over a specific 27 year period and expect them to replicate the observed trend. The results will be dominated by internal climate variability. Why didn’t you use the longer 1922-2005 time series from Brown and Robinson in this analysis? At least there will be some global warming signal in the longer series.

10. Page 3326 line 24: 1979-2001 is definitely too short to talk about trends in precipitation.

11. Page 3326 lines 27-29: The statement that the overestimation of snowfall in the models "might cause the modelled snow cover not to be limited by snowfall as strongly as in reality" is difficult to understand.

12. Section 3.1.2: This is one of the key parts of the paper and some of this material should be moved into the Introduction to help give the paper more focus. Groisman et al. (1994) showed that NH snow cover and air temperature were closely coupled in particular regional temperature sensitive regions (TSR's) that vary seasonally and in response to modes of atmospheric variability such as NAO and PNA. The more you average temperatures spatially (e.g. continental, zonal, NH, global) the more noise you introduce into the relationship dSCE/dT. It is therefore not surprising that the relationship with globally averaged annual temperatures is rather noisy (page 3328 lines 6-8). The finding that Arctic amplification is underestimated in the climate models needs to be highlighted and discussed in more detail. There has been a substantial body of literature on this topic in recent years that again can be incorporated into your introduction. It would also be of interest to look at the model range in the amplification factor to see if there were any patterns related to model configuration, dSCE/dT values, model temperature biases etc.

I checked your computation of SCE temperature sensitivity with globally averaged annual air temperatures from GISTEMP over the 1922-2009 period and come up with a number that is quite a bit lower than what you cite ($\sim$-10%/deg C). This is without any 5-year smoothing which artificially inflates the slope to -11.3%/degC. If the results are that sensitive to the definition of the global average temperature (and the smoothing) then this is not a very robust metric. This is still double the model average sensitivity but it sounds like some of the models had sensitivities that were of this order. It would be instructive to know if there were particular model configurations that favoured higher sensitivities.

Page 3328 Line 10: The weaker representation of interannual variability in the models is a fact which should be documented as part of a basic evaluation of model performance.

Groisman, P. Ya, T. R. Karl and R.W. Knight (1994), Observed impact of snow cover on the heat balance and the rise of continental spring temperatures, Science, 263, 198-200.

13. Section 3.2.1 needs to be more focussed especially the 2nd paragraph.

14. Page 3333 line8-9: you are confusing SCE temperature sensitivity (dSCE/dT) with temperature changes.

15. Page 3333 line 10: suggest you change “wrong” to “inadequate”

16. To play devil’s advocate, your claim that future snow cover extent can be expressed in terms of globally-averaged annual mean temperature must fall flat on its face when there is no longer any snow cover! For a fixed seasonal window the interannual variability in SCE will eventually be reduced under a warming climate and dSCE/dT must get smaller.

Interactive comment on The Cryosphere Discuss., 6, 3317, 2012.