The Cryosphere Discuss., 6, C2154–C2161, 2012
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The Cryosphere Discussions

TCD

6, C2154–C2161, 2012

Interactive Comment

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.

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Replies to referee #2

General reviewer comments:

...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. multilayer snowpack model) influence snow cover sensitivity to warming. The net result is that the paper raises an important issue

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Interactive Discussion

Discussion Paper

but provides no insights into what is causing the models to systematically underestimate recent spring SCE reductions in response to warming.

Our replies:

âĂć Concerning maximum snow accumulation: We did not use this to evaluate the models because grid snow mass data are subject to caution.

âĂć Concerning snowpack density: snow density is not available from the CMIP5 database for many models, and as gridded data of snow height are subject to caution and snow density data are linked to snow mass and height, the use of snow density data are is subject to caution, too.

âĂć Concerning the snow-albedo feedback: This is indeed an important topic, but this clearly exceeds the scope of this paper. There is important work, for example by Hall and Qu (2006) for reference.

âĂć Link between model formulations and snow sensitivity: we studied the influence of the models resolution and the link between solid precipitation, temperature and the sensitivity of snow to temperature changes. None of these factors provided clear indications on reasons for model-data misfits. The additional results will be presented in the corrected version of the paper.

âĂć "The net result is that the paper... provides no insights into what is causing the models to systematically underestimate recent spring SCE reductions in response to warming.": The reviewer is right, we provided no insights into this because we did not even write that the models' snow response to warming is too weak. We have written that the boreal warming (particularly in spring) is too weak, and that this might be the most important reason for the underestimate of recent SCE trends.

âĂć In general, we feel that trying to link the boreal temperature trends to the structure of the snow models included in the GCMs will very probably lead nowhere. The snow module in a coupled climate model is only a minor component of the whole GCM (which treats processes such as radiation, cloud formation, boundary layer turbulence, etc.) and is unlikely to be the dominant cause of such model misfits. This is perhaps also one of the reasons why the paper by Derksen and Brown (2012), cited by the reviewer, does not make any such attempt either.

Detailed comments:

1. Reviewer comment: 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.

Reply: It is true that the models do underestimate the interannual variability of SCE, and we also stated this in the submitted version of this paper. We can quantify this misfit in more detail in the revised version. However, our statement that the climatological snow cover extent is very well reproduced is true even if the interannual variability is not captured. We note that Derksen and Brown (2012) made no attempt at evaluating SWEmax and snow-albedo feedbacks, either.

2. Reviewer comment: 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.

Reply: Not necessarily in the context of a strong global warming, and definitely not in the context of a multi-model study. We show that almost all simulations are inconsistent with the observed trend. And we do not go as far as Derksen and Brown (2012) who even speak about a (very very short) 5-year period of snow cover trends and conclude that the models do not capture this trend.

3. Reviewer comment: Abstract lines 15-17: This statement is not logical. The temper-

ature 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.

Reply: The reviewer has probably read our sentence too fast. There are two different temperatures in the sentence : 1) global mean temperature ; 2) land surface air temperature change north of 50°N. The relationship between SCE and the global mean temperature has the wrong slope in the models because the relationship between global mean temperature and land surface air temperature change north of 50°N in spring is not correctly represented. We will try to formulate this clearer because the reviewer will probably not be the only person who might misunderstand the sentence. And dSCE/dT values will be given in the abstract, as requested by the reviewer.

4. Reviewer comment: 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.

Reply: Ok. The introduction will be more focussed on the goals of the paper and the introductory parts of section 3.1.2 will be moved to the introduction.

5. Reviewer comment: 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).

Reply: Ok. We will explain that the conclusions of Roesch are incorrect, and cite the mentioned paper (Brown and Frei, 2007).

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

Reply: OK. We will refer to the relevant papers (Moss et al., Nature, 2010; van Vuuren et al., Climatic Change, 2011).

7. Reviewer comment: 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.

Reply: OK, this was not quite exact. Roesch and Roeckner (2006) write: "Data prior to 1979 has been omitted due to inhomogeneities in the time series caused by different satellite generations." We will correct this sentence. In any case, restraining our analysis to the period after 1978 remains perfectly justified.

8. Reviewer comment: 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.

Reply: That is what we meant with "reliable": More available observations correspond to a better spatial and temporal description, and so to lower uncertainties. We will reformulate this sentence to prevent misunderstandings.

9. Reviewer comment: 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.

Reply: For one realization of one model, yes, it is inappropriate to compare trends from climate models over a 27-year period. But in this study we compare multiple realizations with multiple models to observations. This is not inappropriate. See our reply to this reviewer's general comment and our reply to comment #1 of reviewer #1. We do not choose a longer period such as 1922-2005, partly because of concerns with lack of spatio-temporal coverage for this period, and particularly because the data are very noisy for this period. The 1922-2005 period does not coincide with the period of strong anthropogenic warming, so unforced and possibly uncorrelated interannual variability of precipitation and temperature will prevent a coherent picture from emerging.

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

Reply: Indeed, this is very short, and we state this. However, we show that snowfall trend, not the total precipitation trend. The snowfall trend will be affected by strong warming (no snowfall above 0°C), so there is a reason to imagine that there could be a robust signal even for such a short period. Nevertheless we will stress our caveats about snowfall trends over this period in order to prevent misunderstandings. Note that we also talk about the average snowfall rate which is very clearly overestimated by the models independent of any trend.

11. Reviewer comment: 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.

Reply: We will reformulate this paragraph. Moreover, we checked the relationship between snowfall rates and SCE trends and found no very significant correlation.

12. Reviewer comment:

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.

Reply: OK

Reviewer comment: 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.

Reply: There has indeed been much literature about Arctic amplification, but we are not talking about the Arctic amplification here, but of the amplification on northern continents (north of 50°N). The processes are not necessarily the same. In particular, there is no sea ice on continents. We will refer to pertinent literature (e.g., Wallace et al., PNAS 2012; Sutton et al., GRL 2007).

Reviewer comment: 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.

Reply: We do not find any significant link between SCE and the model resolution. We looked also at the link between SCE, mean temperature and the boreal amplification, but we do not observe a relevant correlation. Further analysis would indeed be of interest, but beyond the scope of this work which would completely lose its focus.

Reviewer comment: 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 (-10%/°C).

Reply: We had made a little error in our calculations. We now find -12 % per °C (instead of -14), and very similar results for GIStemp (-11.3% per °C), which is expected because it is well known that these global mean temperature series are very similar.

Reviewer comment: 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.

Reply: Yes, we will stress this more than in the first version. Note however that Derken and Brown (2012) already document this.

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

Reply: Ok. This section will be modified in order to make the reading easier.

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

Reply: We call this "apparent sensitivity" because the real physical variable influencing the snow melt is not the global mean temperature, but the local spring temperature. We will state this more clearly. We will also define what we exactly mean by "SCE sensitivity to temperature" in the methods section.

15. Reviewer comment: Page 3333 line 10: suggest you change "wrong" to "inadequate".

Reply: OK.

16. Reviewer comment: 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.

Reply: Yes. There are even more reasons for this. The land area per degree latitude decreases towards the pole. Therefore, if the warming is strong, SCE will decrease very strongly initially, but vanish sooner or later also for this reason. Even for the end of RCP8.5, deviations from the linear relationship become apparent. We will write that this linear relationship holds only in a limited global mean temperature

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/6/C2154/2012/tcd-6-C2154-2012-supplement.pdf

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