The Cryosphere Discuss., 9, C475–C484, 2015
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The Cryosphere
Discussions
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TCD
9, C475–C484, 2015

Interactive comment on "21st century changes in snow water equivalent over Northern Hemisphere landmasses due to increasing temperature, projected with the CMIP5 models" *by* H. X. Shi and C. H. Wang

Anonymous Referee #1

Received and published: 19 April 2015

GENERAL COMMENTS

This study documents the changes in annually and seasonally averaged snow water equivalent (SWE) over the Northern Hemisphere continents, as simulated by the CMIP5 models during the 21st century under RCP2.6, RCP4.5 and RCP8.5 scenarios. To my knowledge, studies on this specific topic using CMIP5 data have not been published before. However, there are similar studies for the CMIP3 data set (Räisänen (2008), Climate Dynamics), as well as studies on snowfall in the CMIP5 models (Krasting et al. (2013), Journal of Climate). As a result, the new information available in the current analysis is somewhat limited. This alone is no strict hinder for publication; however, there are other issues related to this research and the presentation of the results that make me recommend rejection of at this point, with an eventual option for resubmission following thorough improvements.

1. The text fails to adequately refer to and use existing knowledge on characteristics of simulated climate change in general and dynamics of SWE change in particular. There are (at least) three key findings that are basically known from earlier research and therefore would not require much discussion in this paper:

A. The dependence of global mean warming on the RCP emission pathway is small for the next few decades but increases rapidly towards the end of this century (IPCC WG1 2013, Chapter 12).

B. Other aspects of anthropogenic climate change scale more or less directly with the global mean warming (Frieler et al. 2012, Journal of Climate; IPCC WG1 2013, Chapter 12). Therefore, it is logical to assume that the dependence of SWE changes on the RCP scenarios follows the pattern indicated in 1.

C. SWE is governed by three factors: (i) total precipitation, (ii) the fraction of precipitation that falls as snow, and (iii) melting of snow in mild periods. Given that climate models project both an increase in winter precipitation in mid-to-high-latitude Northern Hemisphere (as documented in IPCC WG1 2013, Chapter 12) and an increase in temperature, it is obvious that (i) tends to increase SWE whereas (ii) and (iii) tend to reduce it. In Räisänen (2008, Climate Dynamics), a diagnostic method was presented that helps to quantify the contributions of these factors and thus the precipitation and temperature change effects. Such a diagnostic decomposition would confirm the importance of temperature changes much more directly than the correlation analysis presented in the current manuscript.

2. The results are almost exclusively presented as absolute SWE changes. This makes comparison between different areas and seasons difficult to interpret, because the

change is necessarily constrained by the baseline SWE. If, for example, area A has baseline SWE of 50 mm and loses all of it, whereas area B has baseline SWE of 200 mm and loses half of it, is it meaningful to say that the change is smaller in A than B? To help the interpretation, relative (per cent) changes should be shown along with (or instead of) absolute changes. Furthermore, if the authors maintain that absolute changes are more important than relative changes, they should motivate this focus. While local absolute changes in SWE might indeed be important from a hydrological point of view, what (if anything) do the Northern Hemisphere mean changes tell?

3. The finding that the rate of decrease in the Northern Hemisphere mean SWE tends to slow down with increasing warming may actually not be correct (detailed comment 37 below). Even if it is correct, it would require physical interpretation and a regionally more detailed analysis. Obviously, as climate warms, some areas will lose their snow cover and will show no further decreases in SWE after that. On the other hand, some cold areas that first exhibit an increase in SWE with warming might begin to experience decreases in SWE after a threshold temperature is passed. Thus, the behaviour in this respect is very unlikely to be uniform over all of the Northern Hemisphere.

4. Parts of the analysis seem physically meaningless. In particular, what is the point in analysing the spatial maximum of the annual mean SWE in Figure 4? These results basically reflect model behaviour in a few single grid boxes and have no wider relevance.

5. The documentation of the methods is insufficient. For example, what do the correlation and regression coefficients between SWE and temperature represent in Tables 2 and 3? Were they calculated from interannual variations of Northern Hemisphere land mean temperature and SWE, or from local interannual variations of temperature and SWE with appropriate averaging afterwards, or from something else? I assume that the first alternative was used but this is not documented in the text.

6. The technical presentation of the results is not well-thought. The single most out-

standing example are Tables 2-3, which include a huge set of highly variable numeric values. The implications of these numbers, if any, will be very hard to judge for a reader of the article – a classical "don't see the forest from the trees" problem.

7. There are substantial problems in the English language of the manuscript. Suggesting any detailed improvements to these goes beyond my resources. It seems that the only solution in this respect is to let a professional language editor to correct the text, or to find a skilful colleague who is able and voluntary to do this.

8. It seems that the manuscript was submitted very hastily without even properly checking the list of references, which is not consistent with the references cited in the text.

In summary, I feel that the manuscript is currently far from the level that would make publication in the Cryosphere justified. The authors might consider a resubmission of their paper to the Cryosphere or another journal after thoroughly considering the major issues discussed above, as well as the more specific comments listed below. However, in my opinion, the required improvements go beyond what is usually considered as major revisions.

SPECIFIC COMMENTS

1. P2136, L13 "the reduction is SWE there is related to rising temperature". You don't need to calculate correlation coefficients to make this conclusion which is obvious from physical reasoning alone.

2. P2136, L14-15. "temperature may reach a threshold value". Regionally, "threshold values" of temperature will be reached when the climate becomes essentially snow-free. However, this will not happen everywhere in the Northern Hemisphere at the same time!

3. P2136, L26 – P2137, L9. Please focus on the most up-to-date information that is available in AR5. The earlier IPCC assessments do not add anything important to that.

4. P2137, L15-16. Just like SWE, snow depth is affected by both temperature and

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precipitation.

5. P2137, L25-27. How is the first part of the sentence “Following comparison with observational data” related to the second part “global climate models consistently project”? The model projections are not dependent of observations.

6. P2138, L4-6. Can you give the original reference of this regional climate simulation? It is very unlikely to be the IPCC AR4 chapter by Christensen et al. (2007).

7. P2138, L25-27. Whether this is true or not depends on the baseline climate.

8. P2138, L28-29. Rather “since climate change is dependent. . .” because this is true for all aspects of climate change.

9. P2139, L1-5. This sentence gives a strange impression. Is it not physically obvious that both temperature and precipitation play a role?

10. P2140, L15. Be specific: spatial correlations?

11. P2140, L16-17. It does not make much sense to report that the correlation between the model simulations and observations is statistically significant (because this is highly unsurprising). In this case, the magnitude of the correlations is more interesting than their significance.

12. P2140, L18-20. Time series of what? The Northern Hemisphere mean SWE or local SWE?

13. P2141, L18-19. (This pattern suggests. . .). Delete. There is no point in repeating well-known facts that hold for all aspects of anthropogenic climate change.

14. P2141, L19-20. (Meanwhile. . .). Also delete this, for the same reason.

15. P2142, L1-2. Is this also true when the changes are expressed in relative (i.e. per cent) units?

16. P2142, L12-15 (Viewed in greater detail . . .). Delete. This is well known from a

large number of earlier studies.

17. P2142, L19-20. (And the magnitude of the decrease...). This is the case just because there is more snow in higher than in lower latitudes. The relative decrease in SWE (which would be more informative for many practical purposes) is larger at lower than in higher latitudes.

18. P2142, L23-24. (The relative changes in SWE are similar...). While the absolute (not relative!) changes are smaller at lower latitudes, the scenario-dependence of the changes shows exactly the same pattern south and north of 60°N in Fig. 4e.

19. P2142, L26-27. (Moreover ...). Delete. Nothing new.

20. P2142, L27-29. There is a large body of research on the causes of the polar amplification of greenhouse-gas induced warming (see e.g. box 5.1 in IPCC AR5). Reduced snow cover is just part of the story.

21. P2143, L1-L28. As pointed out in the general comments, the analysis of ZMSWE is meaningless. Delete.

22. P2143, L16-18. Brutel-Vuilmet et al. (2013, not 2008!) study the change in the average annual maximum SWE, not the change in maximum of annual mean SWE. These are different things!

23. P2143, L28 – P2144, L1. This text is repetitive. Delete.

24. P2144, L3-4. How were the slopes and correlations in Table 2 calculated? Are they based on interannual or spatial variability of temperature and snowfall?

25. P2144, L3-8. I would be very careful to draw any conclusions from Table 2. First, it is not self-evident that the long-term climate change relationship between temperature and SWE is quantitatively the same than the interannual relationship. Second, the large and irregular variability of the numbers in Table 2 suggests that they are substantially affected by internal variability, even when they are statistically significantly different

from zero.

26. P2144, L20-21. Why not include the observations in Fig. 5?

27. P2144, L27. How can seasonal SWE changes be contrary to monthly changes? Or do you mean that the decrease in SWE is smallest in summer when the baseline SWE is also smallest?

28. P2145, L1-6. Shorten this text. It is well known from earlier research that larger forcing leads both to larger multi-model mean changes and larger inter-model differences in the response to the forcing.

29. P2145, L7-9. This pattern would be reversed if you considered relative rather than absolute SWE changes.

30. P2145, L10-26. It is not very meaningful to relate SWE change to changes in the Northern Hemisphere land mean temperature and precipitation, as much of the hemisphere is snow-free particularly in summer. It would be better to calculate the temperature and precipitation changes over the area where snow does occur during the baseline period. Even better would be to explicitly diagnose the effects of precipitation and temperature changes as in Räisänen (2008).

31. P2145, L25-26. In mid-to-high latitudes (which are obviously most important for the SWE change) the largest precipitation increases tend to occur in winter (e.g. IPCC AR5, Fig. 12.22).

32. P2146, L13-14. (The sensitivity of SWE . . .). This conclusion would most likely be reversed if you considered relative rather than absolute SWE changes.

33. P2146, L25. (This pattern shows. . .). Delete the sentence.

34. P2146, L21 – P2147, L21. Maps for the annual mean SWE change were already shown in Fig. 3. The only piece of substantially new information in Fig. 7 is the seasonal distribution. Thus, there is no need for a lengthy discussion of the annual

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mean trends.

35. P2148, L1-2. This suggests a shift in the seasonality of the SWE, with the maximum occurring earlier in a warmer climate. This is physically expected, because the maximum of SWE in spring occurs close to the time when the mean temperature rises above zero, and the zero-crossing time becomes earlier when the climate warms.

36. P2148, L11-15. This behaviour follows the rate of global temperature change reported in IPCC AR5 (Chapter 12).

37. P2148, L18-20. Instead of calculating the regression coefficients within the 20-year periods, one could infer the temperature-dependence of SWE simply by comparing the different periods and RCP scenarios with each other. This would allow more reliable conclusions, because a larger range of temperatures is covered and long-term climate change is not confounded with internal variability. Looking at Figure 9 (particularly 9c) from this perspective, it seems that SWE decreases more or less linearly with temperature for T > 5.5°C. On the other hand, there is an abrupt jump at 5.5°C (i.e., between the historical simulations and the RCP simulations) which suggests a problem in the model data or processing of these data. Moreover, for each of RCP2.6, 4.5 and 8.5, the regression coefficients calculated from the interannual variability are larger in MP and LP than EP. In all, this suggests that the purported decrease in the rate of SWE decrease with increasing temperature is questionable.

38. P2148, L23-25. Yes, but (i) the slopes for each of the three RCP scenarios are smaller in EP than MP and LP, and (ii) the apparent discontinuity in the mean value between the RP and the later periods raises the question whether the regression coefficients are actually comparable.

39. P2149, L6-7. This claim contradicts the regression coefficients in Fig. 9 (-1.49 for EP, -1.68 for MP, -1.81 for LP).

40. P2149, L20-22. This precisely follows the scenario and time dependence of tem-

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perature changes.

41. P2149, L24-26. A physically more convincing argument: as winter precipitation in snow-covered areas is simulated to increase, the increase in temperature is the only factor that can lead to a decrease in SWE.

42. P2150, L1. In mid-to-high-latitude areas, the largest increase in precipitation (at least in per cent terms) is projected for winter.

43. P2150, L3-5. Use of relative rather than absolute SWE changes would reverse this latitude dependence.

44. P2150, L7-9. As noted above: Figure 9 does not seem to support this conclusion for the Northern Hemisphere as whole. In any case, such threshold behaviour would be more meaningfully studied in a regional than a Northern Hemisphere mean sense.

45. Tables 2 and 3.The number of numeric values in these tables is excessive. Condensation is needed. Either only show the results for RP and LP, or RP and only RCP8.5 in the three periods. Nothing more is needed, because the temperature-dependence of the correlations and regression coefficients (if there is any) should be captured by these cases. In addition to this, consider displaying the numbers in figures, instead of tables.

46. Figure 3. Please also show the changes in per cent units at least for the last period. Alternatively, show per cent changes for all three periods and absolute changes only for 2080-2099.

47. Figure 4. Delete the first column, because of the reason discussed in General comment 4.

48. Figure 5. Why are the numeric values much smaller than those shown in Fig 2? Were the averages calculated over a different area?

49. Figure 6. Please define the area over which the changes were averaged

50. Figure 7. Show the trends in per cent units instead of / in addition to the absolute units.

TECHNICAL COMMENTS

1. The following studies are cited in the text but are not included in the list of references: Brown and Mote (2009), Christensen et al. (2007), Lemke et al. (2007), Maloney et al. (2012)

2. P2138, L27. Apparently, Räisänen (2011) should be either Räisänen (2008) or Räisänen and Eklund (2011)

3. P2140, L24. "stimulation" should be "simulation"

4. P2141, L1. "the most individual model" should be "most of the individual models"

5. P2141, L15. Apparently, "relative-error ratio" (RE) means relative change.

6. P2159, caption of Figure 2. "stimulated" should be "simulated"

Interactive comment on The Cryosphere Discuss., 9, 2135, 2015.