Reply to the comments of Reviewer #1 on paper CLIDY-D-09-00146: “Scale-decomposed atmospheric water budget over North America as simulated by the Canadian Regional Climate Model for current and future climates” submitted by Raphaël Bresson and René Laprise.

General comments:

This manuscript investigates the present and future atmospheric water budget over North America thanks to a dedicated scale decomposition methodology. The method used is original and has been tested recently for the current climate by Bielli and Laprise (2006, 2007). Here the authors are using simulations that were forced by CGCM3 output (instead of NCEP-NCAR in Bielli and Laprise) for both the present and future climate over a slightly larger domain.

This paper brings an interesting contribution but it is too long (it contains a large number of figures – 19 -) and the focus should be put more on the changes between the present and the future climate which is the new and most interesting point of this paper. Indeed, section 3, which discusses the scale decomposed water budget for the current climate, has already been published.

We thank this reviewer for his thorough review and his several encouraging and constructive comments. In the revised version, the paper has been shortened by 5 pages and the number of figures reduced to 15. In particular, the section on current climate has been cut down by 4 pages. In the revised manuscript we emphasized that the analysis of the scale-decomposed water budget for the current climate differs from the work of Bielli and Laprise (2007) in the version of CRCM used (with important land-surface processes changes), in the use of CGCM for driving the simulations rather than reanalyses, and in the domain size (larger for us).

The section 3 is too long and contains too much figures: this part must be shortened. Most of the results presented in this section are already known. The most interesting point of this section is that under perturbed conditions (CGCM3 forcing instead of NCEP-NCAR reanalysis), results are comparable to Bielli and Laprise 2007. This is especially true for the winter season; you should hence remove Figs 4 and 5, and also reduce the number of figures (which is 4) for the summer season. In summary, this section should focus on the differences between your results and those obtained by Bielli and Laprise. Likewise, the 2nd and 3rd paragraph of the Summary and Conclusion section, which deals with the results from section 3, should be shortened as there is nothing really new in this part. Finally the abstract (last paragraph) should also reflect this point and focus more on the projected climate.

Section 3 has been shortened for the analysis of both the winter and summer seasons. The interannual variability is not dealt with anymore and the discussion has been reduced to the most important remarks. The summary and conclusion section and the abstract have also been reworded in consequence.

The number of figure has also been reduced, from 4 to 3 for each season. Some panels have been withdrawn (for each season: 4 panels concerning the interannual variability, as well as 5 panels showing the intraseasonnal variances of total water budget variables).

However we believe that the results for the current climate are worth being described again, since the configuration of the simulation used here is significantly different from the configuration of the simulation used by Bielli and Laprise (2007). We also believe that examining the results for the current climate helps the reader to better understand the future climate results. Therefore we kept figures displaying scale-decomposition results for the current climate.

The purpose of the paper was not to investigate specifically the differences between our results and those obtained by Bielli and Laprise in a different experimental configuration. In the revised paper, we emphasize the fact that the driving data are different (NCEP-NCAR for them and CGCM3 for us). The CRCM version is also different. In particular, our version includes an up-to-date surface scheme, whereas their version used a simple bucket model. The domain size is also larger here than in their study.

In section 4 presenting the future climate, I presume that some model outputs displayed as difference fields could be subjected to tests of significance. You should test and check the local significance level of the differences between present and future climate fields.

We thank this reviewer for this excellent suggestion. In the revised manuscript, the local statistical significance level of climate change projections has been tested, using two tests. They have been performed at each grid point, in a two-sided configuration and with a 5 % rejection level. Firstly a bootstrapping test has been carried out for both the difference of the means (future minus current climates) and the difference of intraseasonnal variances. In the climate change section, only statistically significant changes are now displayed and discussed, which allows us to focus on most important results. A t-test has also been carried out; it gave results very similar to those of the bootstrapping technique. We therefore are very confident in the correctness of the statistical significance test that has been performed.

Finally, the quality of figures should be improved. Some of them appear blurred (e.g. Fig. 4, 8 and 18), and it is hard to distinguish the different shades of red which sometimes render the following of the discussion difficult. A different color scale for the variance should hence be used.

The colour palettes of almost all figures have been changed, to make figures easier and more pleasant to look at. Some scales have also been changed to get more consistence between figures (For example, the scale of Fig. 3 is the square of the scale of Fig. 2), or to make fields easier to examine.

Other comments:
1) Introduction*.*
a) p3. 2nd para*: You may want to add a paragraph on the conclusion of the AR4 on regional projection over North America compared to global projections.*

As this reviewer will recall, the regional climate projections for North America that were discussed in Chap. 11 of AR4 were mainly based on global model projections, rather than regional projections. The reason was the unavailability of an ensemble of RCM projections for North America, unlike the situation for Europe where the results of an ensemble of RCM from the PRUDENCE project gave some valuable RCM information in addition to the CGCM ensemble. Hopefully the NARCCAP project that is underway will change this situation for AR5, but this data is not yet available.

2) Section 2
a) 2.1 *You should briefly define here the surface scheme used as you are discussing later (p18) the differences arising from the use of the CLASS scheme instead of a bucket scheme.*

A description of the CLASS scheme has been added in the CRCM description (section 2): “In particular CRCM uses the Canadian Land Surface Scheme (CLASS; Verseghy 1991; Verseghy et al. 1993). It is a three-layer soil model, which also includes a snow layer where applicable, and a vegetative canopy treatment. Its prognostic variables are frozen and liquid soil moisture content as well as temperature, which evolve following energy and moisture fluxes at the top and bottom of each layer. Fluxes are computed according to Darcy's equation. Soil surface properties are taken to be functions of the soil and vegetation types and soil moisture conditions within a given grid volume.”

b) 2.2 *In the CRCM simulations section, you should clearly state in what respect the simulations you are using differ from those used by Bielli and Laprise: forcing data, model surface scheme, domain size, period, etc…*

In the revised manuscript we have added a short description of the differences in the configuration of the simulations: “It is important to note that only the methodology used here is similar to Bielli and Laprise's (2007) work. The driving data are different (NCEP-NCAR for them and CGCM3 for us). The CRCM version is also different. In particular, our version includes an up-to-date surface scheme, whereas their version used a simple bucket model. The domain size is also different.”

c) 2.3 Choice of wavelength for scale decomposition*: Bielli and Laprise (2006) choose the cut off wavelength between large and small scales by looking at the variance spectra of wind and humidity in both CRCM and NCEP-NCAR reanalyses. The wavelength was chosen such that the large scales were the scales both resolved by NCEP-NCAR and the CRCM. In you study, you acknowledged that the equivalent theoretical resolution of CGCM3 is roughly 1000km according to Laprise (2002), but have you verified that the scales resolved in CGCM3 are the same as the scales resolved in NCEP-NCAR reanalyses by plotting the variance spectra? The effective resolution of CGCM3 could be different from the theoretical one... Please justify.*

As mentioned in section 2: “The CGCM is a spectral model with a 47-wave triangularly truncated spherical harmonic expansion in the horizontal. This corresponds to a minimal equatorial wavelength of 850 km (Laprise 2002).” Thus we expect the effective resolution of the CGCM to reside roughly in the intermediate range from 600 to 1000 km used here for the scale decomposition.

We have not checked the variance spectra of the CGCM3, as this felt outside the scope of our study, but earlier studies by Boer and colleagues have looked at the CGCM spectra (albeit for different version of CGCM), and their results seem to justify our choice.

Boer, G. J., and T. G. Shepherd, 1983: Large-scale two-dimensional turbulence in the atmosphere. *J. Atmos. Sci.*, **40**, 164-184.

Boer, G. J., 1994: Mean and transient spectral energy and enstrophy budgets. *J. Atmos. Sci.*, **51**, 1765-1779.

Koshyk, J. N., and G. J. Boer, 1995: Parameterization of dynamical subgrid-scale processes in a spectral GCM. *J. Atmos. Sci.*, **52**, 965-976.

3) Section 3

a) p13,L13: "*30 winter months"* *: this is confusing, you are using 90 months. Should be "30 winter seasons".*

This has been corrected. Thank you.

b) p17 Last para*: "…* ***large differences*** *…" How large? Please quantify. Are the differences statistically significant?*

A quantitative comparison with the results of Bielli and Laprise (2007) has not been possible. Likewise, no statistical significance test could be carried out for the differences between our and their results. However the overestimation of the hydrological cycle in summer by the version of the CRCM used by Bielli and Laprise (2007) has been documented. See for example

Music B, Caya D (2007) Evaluation of the hydrological cycle over the Mississippi River basin as simulated by the Canadian Regional Climate Model (CRCM). J Hydrom 8:969-988.

This bias has been corrected with the implementation of the surface model CLASS:

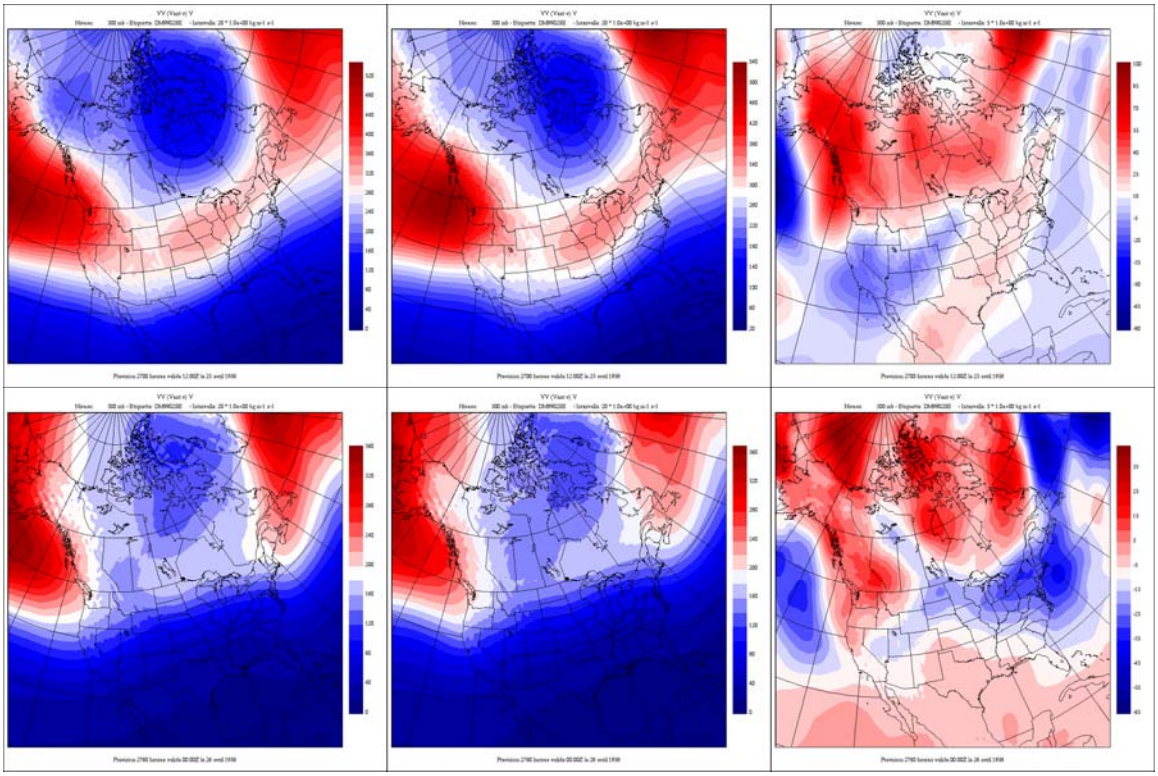
Verseghy DL (1991) CLASS - A Canadian Land Surface Scheme for GCMs. Part I: soil model. Int J Climatol, 11:111-113

Verseghy DL, McFarlane NA, Lazare M (1993) CLASS - A Canadian Land Surface Scheme for GCMs. Part II: vegetation model and coupled runs. Int J Climatol 13:347-370.

This is what we note here when comparing our results to those of Bielli and Laprise (2007).

c) p32 last line "storm tracks"*: It could be instructive to look at the meridional wind at 200 hPA (V200). The region of maximum variance of V200 is a good indicator of the position of the storm tracks.*

As suggested, we have looked at the variability of the meridional wind at 300hPA (the closest level easily available to us). See the following figure. It shows the V300 variability for winter (top row) and summer (bottom row), and for the current climate (1st column), the future climate (2nd column), and the difference future minus current climates (3rd row). This figure supports our conclusion that "Large-scale variability remains larger [than small-scale variability] over a region extending a few thousand km eastward from the Great Lakes, which corresponds to the location of the storm track as revealed by the 300-hPa meridional wind variability." Mentioned in section 3.2).



4) Section 4

a) p22 L11-12 *"This is not surprising since large scales were seen to dominate the winter variability in the current climate" Can you really say that the large scale dominates? Both large and small scales rather show the same amplitude?*

Yes, although it was not explicit on the figure, large scales appear to dominate small scales in the current climate variability of precipitation, evapotranspiration and atmospheric water flux divergence over both oceans and the eastern USA. This can be seen when looking at difference fields between large and small scales. In this paper some scales L_1 are said to dominate scales L_2 when the amplitudes of L_1 exceed 150 % of that of L_2.

b) p22 L21-22 *"The relative changes in small-scale precipitation are larger than for large scales over scattered continental area" Not so clear. From Fig. 3 one can see that the precipitation variance is quite small over the continent, except over the Rocky mountains and on the southwestern part of the US. When you say "continental area", are you talking about these regions? Please detail.*

This remark referred to former Fig. 12 (now Fig. 10) and came from the comparison of large and small scales looking at a difference field. Given that statistically significant small scales are actually very limited for precipitation (see Fig. 10), this remark has been removed in the revised version of the paper.

c) p22 L1-2 from bottom*: "The variability increase over Canada is dominated by U …" This is not so clear from Figure 13, especially over the central part of Canada. You may want to use a different color scale as is it difficult to distinguish the shades of red.*

Although it is not explicit on former Fig. 13 (now Fig. 10), this conclusion came from the inspection of the difference field, which showed that U > 150 % R over Canada and the northern regions.

***d) p23 2nd para**: "Term showing the largest relative increase involve either the very large scales of wind or the small scales of humidity …." You conclude that this could be linked to stronger synoptic mid-latitude systems. I am not convinced since the regions of largest changes are also regions of smallest variance. I would rather show on this figure the absolute change instead of the relative change, with a test to check the significance of the differences. It is sometimes tricky to interpret relative change.*

Our point here was to say that, on the one hand, largest changes occur for terms having the largest variability in the current climate, which has to be expected since no drastic changes occur with climate change. But on the other hand even if they show smaller changes, some terms seem to change more, relatively to their current amplitude. This suggests that these terms would be favoured compared to the others, maybe because they are linked to phenomena changing more than the others. And we can assume that large scales of wind as well as small scales of humidity would be linked to synoptic activity, (unlike small scales of wind which would be more linked to topographic forcing for example).

However, as you suggested, absolute differences are now shown and discussed instead of relative changes. They are undoubtedly less tricky to interpret. But relatives changes are still mentioned in the text.

***e) p24 L2-4 from bottom**: "In summer the patterns of relative change are more similar to the actual change ones than in winter" This is interesting. Could you develop?*

We have noticed that absolute and relative change patterns were really different in winter, with largest absolute changes over oceans and largest relative changes over the northern regions. Conversely in summer, absolute and relative changes display similar patterns with largest absolute and relative changes over the northern regions. We don't really know why.

The fact that largest relative changes occur over the continent instead of oceans in winter is probably due to the fact that variability remains very small over the continent, because synoptic activity is much weaker there than over oceans. The situation is different in summer since continental convection adds to synoptic activity which remains important over oceans. Consequently the location of largest relative changes in summer would be function of the location of largest absolute changes, instead of the location of smallest current climate variability.

***f) p26 L1-2:** Is there a difference between the present and the future climate in V200? (cf comment 3c). Is there an overall displacement towards the north of the variance maximum?*

See the answer to 3c). You will notice that for both winter and summer, the V300 variability field displays a slight northward shift in the future climate, as indicated by the change dipole (3rd column).

***g) p26 2nd para**: Same comment as 4d). I would rather show the absolute difference on Fig.19 rather than the relative change.*

See reply to 4d).

***h) Figs 14 and 19:** The sum of those terms does not give the total variability as the covariance between each 2 terms is not taken into account here. It will thus be helpful to show and discuss the difference between the total variability and the sum of the 9 terms both in winter and summer.*

The covariance term (difference between the total variance and the variance of the 9 terms) has not been investigated in detail. However, we have verified that it remains clearly smaller than both dominant terms in winter, and even more in summer.

***5) Conclusion**: The projected climate of the CRCM is strongly influenced by the CGCM3 projected climate. Hence, it would be interesting to add a discussion on the CGCM3 projected climate with respect to the other models of the IPCC.*

Although this would be interesting, this was not the goal of this paper. We believe that doing that would lead us to far from our main topic, which is the contribution of scales to the water budget climatology and to its evolution accompanying a warmer climate. Moreover the general good agreement between our results and those of Bielli and Laprise (2007) also shows the good agreement between the CGCM3 and NCEP/NCAR reanalyses. A discussion on the CGCM3 projected climate with respect to the other models of the IPCC can be found in the 4th assessment report of the IPCC.

***6) Figures**: Please complete the legends when different scales are used (like in Fig. 12).*

The use of different scales in a same figure has been mentioned where necessary. Thank you.