Atmos. Chem. Phys. Discuss., 15, C12756–C12765, 2016
www.atmos-chem-phys-discuss.net/15/C12756/2016/

Interactive comment on "The incorporation of an organic soil layer in the Noah-MP Land Surface Model and its evaluation over a Boreal Aspen Forest" *by* L. Chen et al.

L. Chen et al.

yanping.li@usask.ca

Received and published: 26 February 2016

Thank you for your careful reading and thoughtful comments, which help to improve the presentation and scientific content of the manuscript. We have carefully taken them into account when revising the manuscript, and our responses are below is in italics.

My major concerns are as follows: 1) In the reply to my previous comments, the author also recognized that below-canopy turbulence and radiation transfer are critical for the winter land-atmosphere interactions. Since the authors also showed that the incorporation of organic layer mainly improved the turbulent heat flux simulations during

spring time. I suggest the author should check the work published by "Clark, M. P., et al. (2015), A unified approach for process-based hydrologic modeling: 1. Modeling concept, Water Resour. Res., 51, 2498–2514, doi:10.1002/2015WR017198" and "Zheng, D., et al. (2015), Under-canopy turbulence and root water uptake of a Tibetan meadow ecosystem modeled by Noah-MP, Water Resour. Res., 51, doi:10.1002/2015WR017115", and try to include the new parameterization mentioned in the two papers to check whether the turbulent heat fluxes can be improved. In my opinion, I think the author should first address the existing simulating errors by default Noah-MP, and then do the sensitivity test to investigate the impact of adding an organic layer. Besides, it's better for the author to present the comparison for snow and snow-free period, which will make the reader clearer on how the snow process affecting the evaluation.

Thanks for mentioning these new publications that discuss the issue related to under-canopy turbulence, which are now cited in the manuscript. We recognize that the parameterization schemes of those physical processes need to be improved and Noah-MP has weaknesses in other sub-process parameterizations. Nevertheless, the main objective of this paper is to explore the impact of incorporating organic soil on surface energy and water budgets, rather than comprehensively addressing errors in existing Noah-MP parameterization schemes. It is a good suggestion to separately evaluate snow and snow-free periods. We calculated the winter (Table 1 below) and summer (Table 2 below) statistics compared between model results and observation data. In general, both CTL and OGN perform better in winter, and the differences between CTL and OGN is small. During the spring snow-melting season, the OGN results are much better than the CTL (Figs 6 and 7). We modified the Introduction and Section 4.3 to reflect these explanations.

2) In the reply to my previous comments, the author mentioned they carried out sensitive test to investigate the different parameter values proposed by Lawrence and Slater (2008) and Letts et al. (2000). I think the authors should include the results of the

sensitive test in the manuscript, and to show clearly how the different parameter values will affect the simulated water and energy budgets.

Good point. In Section 3.1, we performed parameter sensitivity tests and the results are shown in the two figures below (not shown in the manuscript), but we added the following sentences to address the raised issue:

To investigate impacts of uncertainties of those parameters on simulations, we also conducted sensitive tests for key parameters such as saturated hydraulic conductivity, porosity, suction, and Clapp and Hornberger B parameter. Those parameters were perturbed within 5-20% range (except for hydraulic conductivity that is changed over 4 times below and above the default value) following the work of Letts et al. (2000). Results showed that the simulated soil moisture is not sensitive to these parameters perturbations, and the simulated soil moisture and temperature results are not sensitive to the changes in porosity, saturated suction, hydraulic conductivity. This implies that the model results are not sensitive to uncertainty in each specific soil parameter, but more sensitive to differences in physical properties between CTL and OGN. Therefore, we decided to use Lawrence and Slater (2008) and Letts et al. (2000) recommended values instead, which produced soil moisture and soil temperature close to observations (see Table 2).

3) In the reply to my previous comments, the author argues that Noah and Noah-MP have been tested in many literatures with reasonable results. I remind the author to check in which case the Noah and Noah-MP were used. The diffusive form of Richards equation is generally used in Noah or Noah-MP for two conditions: one is the assumption of homogeneous soil column, and the other is for large scale simulation that the soil moisture is rarely saturated in the soil column in large grids. However, this study tried to introduce the organic soil layer (i.e. heterogeneous soil column) and shallow groundwater dynamic (the groundwater level is around 1-5 m), which thus is not suitable to keep using the diffusive form of Richards equation. I think the author should replace the diffusive form of Richards equation with the mixed form of Richards

equation and to check how this will affect the simulation.

This is a good idea for future investigation. Again, a comprehensive assessment of other Noah-MP parameterization schemes (e.g., Richards equation) is beyond the scope of the current study. Noah-MP has been verified over many river basins and some of these basins have a shallow water table (see Niu et al. 2011 and Yang et al. 2011).

4) For the model spin-up, the author set 10 years based on the default Noah-MP model run without groundwater scheme. Then the author included the groundwater scheme in the control experiment. According the work by Cai et al. (2014) also cited in the manuscript, the time needed for the groundwater level is around 55 years. So I wonder whether the groundwater level reached its equilibrium or not. I think the author should select the spin-up time with the groundwater scheme included.

For Cai's paper, the spin-up time takes a long time in extreme drought areas, and the water depth is deeper than that in our site where the water table depth is shallower (less than 2.5 m). So it takes $\sim$7 years for water table depth to reach equilibrium. Our spin-up results showed a slower spin up with the freezing/thawing processes, and we set 10 years for the spin-up time for all the experiments discussed here. Text in Section 4.1 was modified to reflect this point.

5) The author showed that the inclusion of organic layer slightly improved the simulation of sensible heat flux during spring time (Figures 4 and 9) as well as improved the simulation of soil temperature (Figure 6). However, the authors also showed that the inclusion of organic matter degraded the simulation of surface soil moisture (Figure 7a) as well as turbulent heat flux during summer period (Figures 8 and 9). The author concluded in the abstract as well as in the manuscript that "the OGN show significantly improved performance of the model in surface energy fluxes and hydrology", which is obviously wrong due to the contents presented in the manuscript. If the inclusion of organic matter significantly degraded the simulation of soil moisture and turbulent heat

flux during summer period, which may imply that it should be careful to include the organic matter scheme for the current and future study, unless the author is able to show consistent improvement can be achieved.

The text, abstract, and conclusions are modified to explain the improvements and degradation of using the organic parameterization in Noah-MP for soil moisture, soil temperature, and surface heat fluxes. Interpretation of high bias in summer sensible heat fluxes in OGN is presented in Section 4.4.

6) The author argued that the soil moisture measurement may be unreliable for winter time, and it's difficult to justify which simulation is better between the CTL and OGN for the surface soil moisture during frozen period (Figure 7a). Actually, form Figure 7a we can find that the simulated liquid soil moisture approaching zero with OGN model run, which is however inconsistent with previous finding that (e.g. "Guo-Yue Niu and Zong-Liang Yang, 2006: Effects of Frozen Soil on Snowmelt Runoff and Soil Water Storage at a Continental Scale. J. Hydrometeor, 7, 937–952.") there is still liquid water below minus 10oC. Since the improvement of sensible heat flux during spring time and soil temperature is associated with the surface soil moisture simulation (see Lines 297-299), the conclusion in this manuscript is not robustness if the author cannot justify whether the soil moisture simulation is improved or degraded. I think the author should carry out more analysis to justify the inclusion of OGN can improve the simulation of soil moisture year-round.

The relationship alluded to in Niu and Yang (2006) defines the maximum amount of liquid water that can be present at a given temperature and soil type (based on saturated matric potential and C-H b parameter). Using the mineral parameters in Table 2, at -10C the maximum liquid content is 25% of the porosity while for the organic soil the maximum liquid content is only 1% of the porosity (due to both lower b parameter and lower potential) so very little liquid is predicted in the organic soil in winter.

7) There are several misleading or incomplete expressions in the manuscript, and I

think the author should add more careful expression to the results they presented. For instance: a. Line 246: I think the thinner snowpack provides less insulation causing the increase of evaporation, not the less precipitation/snow.

The original sentence is replaced by "when the thinner snowpack provides less insulation, leading to higher evaporation, which reduces soil moisture."

b. Line 247: the OGN produce lower soil moisture during winter time but higher soil moisture during summer time, the seasonal difference should be mentioned.

The text was revised to reflect it and now reads "With an organic soil horizon, the OGN produces lower (higher) liquid soil water content during winter (summer) in the topsoil layer (Figure. 5). Lower (higher) soil moisture reduces (increases) thermal heat conductivity, and results in higher (lower) winter (summer) soil temperature in OGN as compared to CTL."

c. Line 323: I think the increase of runoff is due to the increase of base flow that more water is available in the deep soil layer, the author should present this more logistically.

Correct. Revised the sentence to read "In OGN simulation, the water moves faster into deep layers than in CTL simulation, leading to more infiltrated water in the deep soil and hence higher base low. Consequently, the total runoff is increased."

d. Line 361: I think the OGN increase surface runoff due to the more production of ice content, which will however reduce the infiltration of water into the soil column and thus reducing the subsurface flow. The reason for the increase of subsurface flow is due to the OGN produce wetter soil profile. The author should present this more logistically.

Revised "because of the higher surface layer soil ice content, the increase of subsurface flow is due to the OGN producing a wetter soil profile"

e. Line 367: More soil-ice content dose not necessary lead to wetter water content, the presentation should be more logistically.

Replaced “OGN produces more soil-ice content and higher soil porosity, and leads to higher deep-soil-layer soil water content than CTL simulations.” By “OGN produces more soil-ice content and higher soil porosity, and leads to higher total soil water content than CTL simulations as the higher ice content severely restricts movement of water out of the soil column.”

f. Line 383: From the content, the OGN does not significantly improve the performance.

This entire paragraph in “Summary” is revised to explain the specific improvements and degradation in the OGN simulation compared to the CTL simulation.

g. Line 390: I think the simulated liquid soil moisture produced by OGN should be related to the hydraulic parameters like porosity, saturated air potential and b parameter. Yes, I agree.

h. Line 401: From the manuscript I did not see the nighttime simulation, why the author mentioned in the conclusion? I lack context. The mentioning of nighttime results is deleted from conclusion, although the qualitative comparison of the diurnal cycle of heat fluxes between model and observation are shown in Figs. 8 and 9, and nighttime OGN results are fairly close to CTL results.

The minor concerns are as follows: Line 225: the text here did not reflect the figures correctly.

This section has been deleted in the revised manuscript.

2) It’s better to add explanation to the legend of color bar, and it’s also suggested to add RMSE and IOA results in the figures.

Added color bar explanation in the caption and RMSE and IOA statistics in Figure 7.

3) Line 279: It sounds strange to mention figure 12 before figures 8-11, can the authors present this in a more logistic way?

Delete the sentence “Simulated summer evaporation from the ground is smaller for

OGN than CTL (Figure 12).”

4) For the paragraph between Lines 278-299, can the author reorganize this paragraph? It’s difficult to follow the logistics.

This section is removed and a more concise explanation about Noah-MP option selection is in Section 3.1.

5) Since the OGN affect both daytime and nighttime simulations, I cannot understand the author only presented the daytime results in Table 4. Maybe it’s better to show the comparisons for daytime and nighttime separately in two tables.

The quality of nighttime flux-tower data is questionable (e.g., Chen et al. 2015), especially for the OAS located at boreal forest. Therefore, we focused our quantitative evaluation of daytime heat fluxes. However, the qualitative comparison of diurnal cycle of heat fluxes between model and observation are shown in Figs. 8 and 9, and OGN nighttime results are fairly close to the CTL results. .

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C12756/2016/acpd-15-C12756-2016-supplement.pdf

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 29265, 2015.

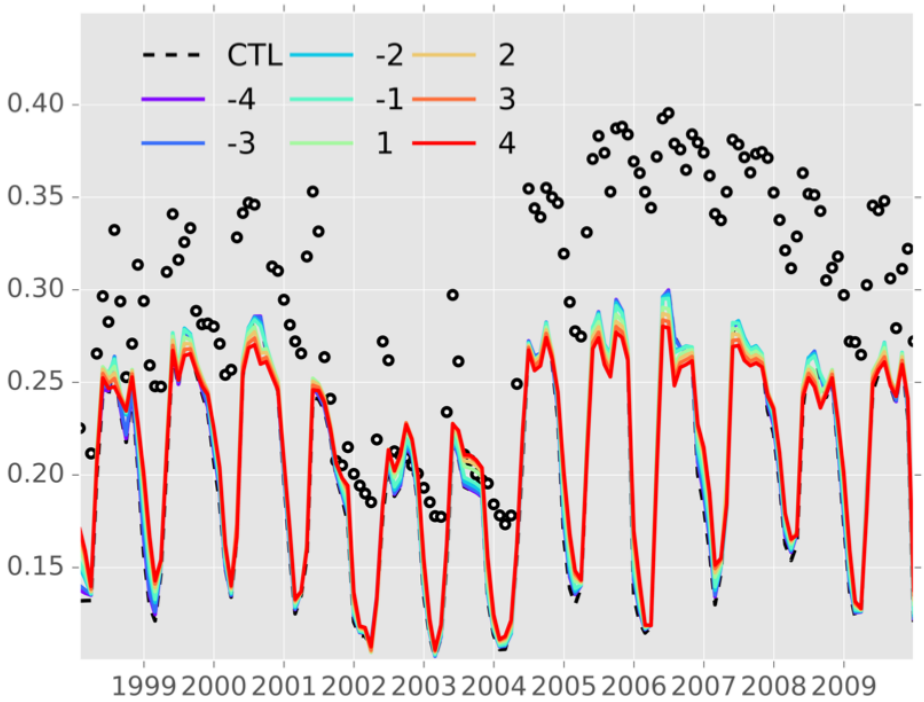

Fig. 1. Sensitivity of total soil column liquid water content to varying hydraulic conductivity.

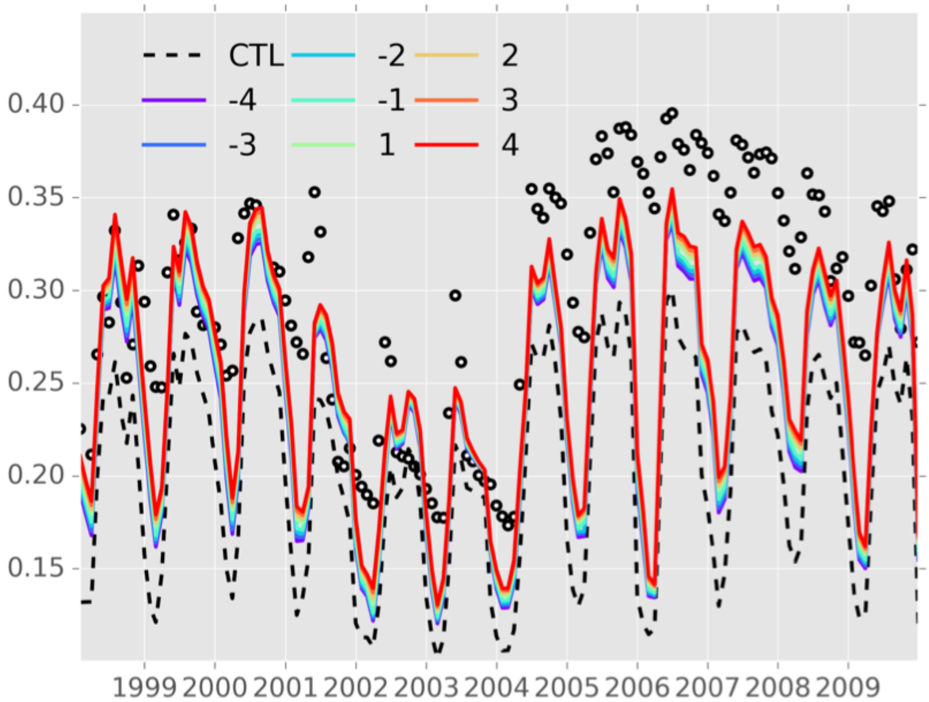

Fig. 2. Sensitivity of total soil column liquid water content to varying porosity.