It is my pleasure to review the manuscript "The incorporation of an organic soil layer in the Noah-MP Land Surface Model and its evaluation over a Boreal Aspen Forest" by Chen et al. The Noah-MP land surface model is used to investigate the impact of incorporating a pure organic soil layer on simulating surface energy and water budgets for a Boreal Aspen Forest. Although the incorporation of an organic layer into Noah-MP is new, the author was not able to achieve a consistently better performance year-round in comparison to the default model physics. Besides, there are a couple of significant flaws or misleading expressions. According to this, I suggest to rejecting this paper, but the authors are encouraged to substantially revise the manuscript and re-submit it.

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 more clear on how the snow process affecting the evaluation.

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.

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.

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.

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 are able to show consistent improvement can be achieved.

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 10^{o}C. 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.

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.

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.

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

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.

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

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

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.

h. Line 401: From the manuscript I did not see the nighttime simulation, why the author mentioned in the conclusion? I lacks context.

The minor concerns are as follows:

1) Line 225: the text here did not reflect the figures correctly.

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.

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

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

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.