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 authors incorporated an organic soil layer into Noah-MP and evaluated its performance over a Boreal Aspen Forest site. The method is straightforward, however part of the results and conclusions are questionable due to the unrealistic simulations of liquid water and ice during the cold season. Besides, there are a couple of flaws or misleading expressions. According to this, a major revision is suggested, and the authors are encouraged to substantially revise the manuscript and re-submit it.

My major concerns are as follows:

1) In Figure 5, the liquid water in the top soil layer is much underestimated or unrealistically simulated comparing to the observation after incorporating the organic soil layer. In Figures 8&9 as well as Sections 4.3&4.4, the authors show that the inclusion of organic soil layer produces comparable or worse simulations of turbulent heat flux during summer, autumn and winter compared to the default model setting, and only improvement is seen in spring. Since the water and heat is strongly coupled during cold season including spring, the underestimation of liquid water or overestimation of ice in the top soil layer during cold season will inevitably affect the simulations of turbulent heat flux. Hence, if the underestimation of liquid water in the top soil layer during cold season cannot be resolved appropriately, the conclusion drawn upon this is questionable.

2) The underestimation of liquid water in the top soil layer is due to the introduction of much lower value of b parameter for the organic soil (see Table 2). The values suggested by Lawrence and Slater (2008) are directly adopted in this study for the organic soil layer. The authors also did a sensitivity test and showed that the total water contents are not sensitive to the specific soil parameters. However, the chosen of parameter ranges, 4 times for hydraulic conductivity and 5-20% for other parameters, is not rigorous, since Letts et al. (2000) showed that the value for b parameter of organic soil ranges between 2.7 and 12, and for hydraulic conductivity ranges between 0.1 and 280×10^{-6} m/s. Besides, it's better to show how the parameters affect the soil moisture simulation of each layer.

3) Since a pure organic soil layer is assumed, the parameterization of organic soil in this paper is not exactly the same as the parameterization proposed by Lawrence and Slater (2008) as well as the Equations (1) and (2) shown in section 3.1. It's suggested to remove the equations and rewrite the method. The method adopted here is straightforward, and it's suggested to collect the soil samples and measure the hydraulic and thermal properties of organic soil directly, which will largely overcome the parameter uncertainties.

4) Another uncertainty with the introduction of organic soil layer is that it will cause the discontinuity of soil moisture between the first and second soil layers. Specifically, the soil water potential between the interface of first and second soil layers is identical or continuity. However, due to the different soil parameters assigned for the first and second soil layers, which will cause different soil moisture for layer interface with the identical soil water potential. The authors are suggested to address

this problem appropriately and to show how will this affect the soil moisture simulation results.

5) In section 4.4, Line 328-337, the authors attribute that the overestimation of sensible heat flux during summer time is due to the energy imbalance in observations. If it's the case, the authors are suggested to address the energy closure problem appropriately, and then compare the model simulation with the correct observations, which may subsequently change the results presented in Table 3, Figures 6-12 and the corresponding text.

The minor concerns are as follows:

1) Line 228-230, "Lower (higher)…to CTL". This expression here is nor rigorous, since more ice is produced during winter, which will increase the thermal heat conductivity.

2) Line 310-320, "OGN has…sensible heat flux". This part is lack of context with the previous presentation and also there are not figures or tables to support the text. Since this section is focused on the diurnal cycle, maybe it is better to remove this part or move it to section 4.5.

3) Line 331, the term "GFX" is not defined before.

4) Line 340, change "last" to "previous".

5) As shown in table 1, the authors choose the zero heat flux as the soil temp lower boundary. Since the soil column is 2m, which maybe too shallow to configure with the zero heat flux to correctly simulate the multi-year soil temperature dynamic over the frozen soil/permafrost area. Can the authors comment on this.

6) The authors describe in Line 410-411 that they plan to apply the parameterization proposed in this paper to other region, the question here is that is it the parameters adopted in this paper also applicable to other region, or what's the challenge to transfer the parameter or address the parameter uncertainty?