

The manuscript “Optimization of Snow-Related Parameters in Noah Land Surface Model (v3.4.1) Using Micro-Genetic Algorithm (v1.7a)” by Lim et al. This is my second time review. Authors have addressed some of the concerns, however I think the manuscript needs serious improvements before it is accepted for publication. The following are the comments, which may improve the manuscript. Still, the results are not promising.

1) Table 4: In terms of correlation, all improvements are in second decimal. For example for FSC  $R^2=0.219$  ( $r\sim 0.467$ ) and 16.4% improvements in OPT\_6 will be  $R^2=0.255$  ( $r\sim 0.50$ ), which is the highest improvements. Is change of  $r$  by about 0.037 significant? I am not sure. Unfortunately authors have tried to mislead their study by not bringing out meaningful results.

2) Similarly in Figure 6, one would be interested to see the difference between observations and model simulation, and not only the difference between control and improved version of model. This makes the improvements further questionable.

3) How the evolution of snow/land parameters (e.g. snow depth) looks like in model and observations. As shown in Figure 6, how the mean state varies with time (daily/monthly average) in model and observations. I can see/guess existence of a seasonal cycle of snow depth in Figure 6a, however authors in their reply have argued that “Snow parameters do not have the observations; thus, it is impossible to compare the snow-related parameters between model and observations.”

4) Finally, authors need to show some improvements/results to be worth publication. I do not agree that offline Noah LSM takes so much time that one cannot do grid-point simulation over SK region. However, I am not insisting on this, but pointing out one of the possibilities to bring out some positive impact of this method/study. At this stage the results are not encouraging.