This manuscript contributes to model improvement in snow cover fraction parameterization. The manuscript is suitable for the journal. The topic is interesting and the potential achievements are expected to have prospects for numerical simulation and forecasting over the Tibetan Plateau region or even the globe. However, there is still large space to improve the manuscript for final publication, including descriptions and verification of technical methods, investigations and discussions. Therefore, a major revision is suggested. My comments are listed in the following.

1. Section 2.4, Please clarify which data is used for these selections.
2. Please add the resolution/size of sub regions in Figure 2, A, B, C, D and A1, A2....., other wise may confuse the readers, confused with grid resolution/size.
3. Add units in proper places, such as the tables, introduction text of variables in each equation.
4. Section 3.2, Please clarify which snow depth and SCF data is used for optimization.
5. Line 135, Better to introduce W_snow and W_max, and how to derive the two.
6. Line 166: 'Through judging the smallest RMSE between observed SCF and the fitted value': 'using the least square fitting method' maybe better.
7. Is K_accum and N_melt depends more on σ_topo or SAI for grass land? I suggest to do some analysis. For example, calculate the coefficients of K_accum/N_melt between the two.
8. Is the optimized scheme resolution dependent? I suggest to added some analysis is better, or at least some associate discussions.
9. How the form of each equation for K_accum and F is chosen. For example, Eq.5, Bare land the form of eq. Is: a*X**b, while for grass land is: a*X1*X2+b. Why the two are different? I suggest to add some explanations.
10. Why not using all TP region for the optimization but only using 4 small sub regions? Normally, from a statistical perspective, the more samples, the results are more robust. I.e. if another 4 sub regions is selected randomly from the TP for bare land and grass land, are the same equation can be achieved? Qualitatively, by how much (personally, uncertainties within 10% is acceptable, but within more than 50% maybe too large) the fitted coefficients is reliable needs to be answered.
11. Line 317-318, when you state that 'CLM5 still shows cold biases', it is better to show biases rather than spatial pattern of the CLM5 and observation.
12. Line 266-268. It is hard to ' identify which process contributes most to the improvement,' based on current experimental design. Additional experiment (only using optimized SCF during snow accumulation) may required when accurately investigating the roles of the two optimization (snow accumulation (eq.6) and snow melting (eq.10)). Because snow accumulating and melting could happen each day, and the effects of the two would compensate each other when both are used.

13. Table 4, Why using the 8 sub regions for evaluation? If the new method is developed based on these 8 sub regions, the evaluation is not independent.
14. Figure 9, I suggest to show the annual cycles. Then the roles of both optimizations can be clearly seen.
15. Too small and inconsistent font sizes for figures.
16. I suggest to calculate the error metrics for evaluations and statistical metrics for comparisons.
17. Section 4.3, if you want to draw conclusions on surface energy budget, then the energy variables should be evaluated or investigated, including the short wave, long wave and heat fluxes. They are all influenced by SCF. Albedo is only one variable that directly influences the surface short wave budget. Further, the evaluation seems inadequate, authors may consider more comprehensive investigations.
18. For areas without observations, CMFD forcing is also less reliable. Consequently, the cold biases may be inherited, line 317-318.
19. Line 318-319, spatial pattern could be quantified by correlation coefficient. When a conclusion is drawn, better to have a quantitative support. Please check the rest of the manuscript.
20. The optimizations seems not very effective. The atmosphere forcing restricts the energy input to the land surface, a coupled atmosphere-land simulations may achieve more effective results by enhancing the snowcover-albedo-radiation energy feedbacks. The authors may consider a set of coupled simulations, or at least add some discussions for outlook.