Review of "Representing effects of surface heterogeneity in a multi-plume eddy diffusivity mass flux boundary layer parameterization" by Nathan Arnold

Summary

The author presents a modeling approach that allows communication between subgrid surface heterogeneity and the overlying atmosphere by accounting for the updrafts assigned to individual buoyant surface tiles. This approach is incorporated in a multiple plume EDMF boundary layer scheme and configured in the single column mode of the GEOS model to assess the impact on the boundary layer states and shallow clouds. Sensitivities of major parameters in the proposed approach are also examined to understand the uncertainty that may be introduced. Results show that the new approach can more effectively reflect the surface heterogeneity compared to the original treatment in GEOS through the inter-updraft variation of thermodynamic quantities, though it has a pretty modest impact on the mean states and cloud properties in the boundary layer. Limitations and pathways toward future implementation in ESMs are also discussed. This work is useful in the land and atmosphere modeling communities and within the scope of GMD. The manuscript is well written and organized. I recommend publication provided that the below minor comments are addressed.

Minor comments

1. L118: "singly to the most buoyant tiles in descending order" It is a bit confusing. Do you mean the remainder of the updrafts are assigned to those least buoyant tiles?
2. L164: "We use 137 levels" Is 137 a typical vertical level number for 0.5-deg resolution GEOS? It seems too many for a coarse-resolution Earth system model. Does the author think the vertical resolution will influence the DMF performance (or the propagation of the surface heterogeneity upward the atmospheric boundary layer)?
3. L176: "heterogeneous"->"modified" to distinguish from the heterogeneous surface case just being mentioned before.
4. Figure 5: (1) please add a legend for Fig. 5 (2) using an error bar to denote the data range may be more appropriate here
5. Figure 8: Could the author elaborate more about the cloud fraction changes due to the applied DMF approach? Is it a robust signal related to the heterogeneous treatment of the updrafts? Or is it arising from a couple of profiles with large cloud fractions? Considering the simulation period is only three months with a mean cloud fraction of ~2%, a small number of overcast profiles might determine the statistics. If it shows consistently increased cloud fraction in DMF, could the author discuss more about the underlying mechanisms? The author stated in L255 that "the DMF approach can impact the mean state by altering the updraft vertical fluxes, and by modifying the higher order moments used as inputs to the ADG

PDF. This in turn can affect cloud properties, buoyancy flux and the generation of TKE.", which apparently is not demonstrated in Fig. 8. It might be helpful to add profiles of vertical fluxes, higher order moments, buoyancy flux, etc., for clarifying the impact of DMS in model simulations.