Comments on “Large-Eddy Simulation of Turbulent Flux Patterns over Oasis Surface”

The manuscript “Large-Eddy Simulation of Turbulent Flux Patterns over Oasis Surface” by Cao et al. submitted to the Atmospheric Chemistry and Physics proposes a method to improve the simulation of surface fluxes for large-eddy simulations (LESs). The premise of the study is the long-standing challenge for wall-modeled LESs to parameterize surface fluxes. Current LESs mostly rely on Monin-Obukhov similarity theory (MOST) for surface fluxes, while it is known that MOST applies to the ensemble averaged rather than instantaneous fluxes. The authors therefore apply MOST to the horizontally averaged fluxes, and use that to scale the fluxes predicted by the LES closure at the individual grid points to achieve agreement with MO at the LES domain scale. While the proposed method is of practical value, I am afraid that I must reject the manuscript in its current form. Please see my major comments for details. Meanwhile, please improve the English writing. I gave up looking for typos and grammatic errors at line 61, so please, make sure all listed authors proofread the manuscript carefully.

Major Comments

1. I found the physics behind the proposed method questionable. For that, I must draw the authors’ attention to the two-part eddy viscosity model proposed by Sullivan et al. (1994). I think the authors are essentially pursing the same idea, to obtain agreement with MOST on the scale at which the theory is developed for. Achieving this through linear scaling of the LES fluxes with Eq. 17 gets you the right result (i.e., agreement with MO for the domain-averaged fluxes), but is not fully based on the physics of turbulence closures. I strongly encourage the authors to read Sullivan et al. (1994) carefully, and find that the LES closures are developed for isotropic turbulence, and near the wall, you essentially need a RANS model that encompasses all turbulence scales. Simply scaling up/down the LES closure to produce the RANS results are not physically-founded. For the purpose of achieving MOST for the domain averaged fluxes, the two-part eddy viscosity model is more physically meaningful, as it acknowledges the shift between LES to RANS-modeling as the surface is approached.

2. The method is not well-described with many key details missing, assumptions unaccounted for, and possible errors in the equations. For example,
   - Eqs 3 and 4, it is best to formulate the turbulent fluxes with turbulent perturbations.
   - Eqs. 5 and 6, these are steady state budgets. I don't think LSM assume steady state energy balance, they predict changes of surface temperature and humidity.
   - Please double check Eq. 13, I think it should be z1/K(z1)*ln(z1/z0).
   - Reynold mean simulation (RMS) and k-l model, I think they are most often referred to as RANS simulation and TKE-1.5 models.
   - At which vertical levels (or up to what height) do the authors apply the proposed MO-correction (i.e., Eq. 17), as this is clearly a surface layer correction.
   - I don't think it is necessary to introduce Eqs. 18 and 19, applying correction to the fluxes as in Eq. 17 should be sufficient.
   - I think the proposed method for forcing MOST for the horizontally averaged fluxes may not apply to heterogenous surfaces despite the claims of the authors. For that to apply, the authors may need to go into the inertial sublayer (which may very well lie above the first few grid points above the wall). If this is the case, how would the authors deal with those grid points that lie within the viscous sublayer where surface heterogeneity matters.

3. The purposes of the tests to determine α is not clear. It seems like the authors are simply testing the validity of MOST, or how good a specific set of MO similarity functions are in predicting the surface fluxes at the measurement site. They use fluxes obtained from eddy correlation measurements as benchmark to test the performance of the MOST-parameterized fluxes. For this purpose, I don't think the authors need LES, a single column model would do.

4. The real case evolution is not targeted at the improvements the method could potential lead to. I don't think mean profiles and time series of sensible and latent heat fluxes are useful for the purpose of evaluation. If the authors ready force MOST at the domain scale, the domain averaged fluxes would be in good agreement. You do not even need LES for that, a single column model would give you similar results. The authors should focus on turbulence characteristics instead of mean profiles. The spectra might be a good place to start. The horizontal distribution of fluxes in Fig. 9 offers some insight, but it is hard to tell

the differences (also please make sure all subplots have the same sizes for ease of comparison).

Minor Comments

1. Line 11, often violate the …
2. Line 12, better turbulence closure than model closure
3. Line 13-14. … LES models. It computes …
4. Line 14, here and elsewhere, “closure” rather than “closure scheme”
5. Line 20, how would a constraint increase with resolution? Please rephrase for clarity
6. Line 22, exchange
7. Line 26, “to be” rather than “is”
8. Line 41, here and elsewhere, Deardorff’s model is commonly referred to as the 1.5-order TKE scheme, rather than $k - l$ scheme. The $k - l$ scheme usually refers to two equation models, which include a separate prognostic equation for the length scale l (specifically kl), see for Holt and Raman (1988) for a review.
9. Line 43, better prescribed then pre-specified.
10. Lines 52-53, “Second, how to estimate surface fluxes in LES models is an unsolved problem”. Not sure what you are trying to say here, you might need to be more specific. How is this related to “scale differences”?
11. Line 56, based on
12. Lines 58-60, please refer to the work of Redelsperger et al. (2001) that addresses exactly point 3.
13. Line 61, nighttime, one word, as for daytime.

Reference:

Holt, T., and S. Raman. 1988. “A Review and Comparative Evaluation of Multilevel Boundary Layer Parameterizations for First-Order and Turbulent Kinetic Energy Closure Schemes.” *Reviews of Geophysics* 26 (4): 761–80. https://doi.org/10.1029/RG026i004p00761.

Redelsperger, J. L., F. Mahe, and P. Carlotti. 2001. “A Simple and General Subgrid Model Suitable Both for Surface Layer and Free-Stream Turbulence.” *Boundary-Layer Meteorology* 101 (3): 375–408. https://doi.org/10.1023/A:1019206001292.

Sullivan, P., J. C. Mcwilliams, and C. Moeng. 1994. “A Subgrid-Scale Model for Large-Eddy Simulation of Planetary Boundary-Layer Flows.” *Boundary-Layer Meteorology* 71 (3): 247–76. https://doi.org/10.1007/BF00713741.