Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2017-41-RC1, 2017

Geoscientific
Model Development
Discussions
Open Access

Interactive comment on "MicroHH 1.0: a computational fluid dynamics code for direct numerical simulation and large-eddy simulation of atmospheric boundary layer flows" *by* Chiel C. van Heerwaarden et al.

E. Bou-Zeid (Referee)

eliebz@gmail.com

Received and published: 9 May 2017

The paper documents the development of a CFD code that can be used in DNS and LES mode and that is made available to the community. The authors have a broad range of expertise in the physical and numerical aspects of such codes and this new code is very well designed. It will provide a great tool for researchers and I expect it to be widely used.

Some comments:

1. Eq 11: It would be useful to explain what Q represents physically (phase change,

radiative divergence, . . .). Also it should be included in 13 since the authors also use it to represent sources of heat unrelated to evaporation/condensation.

2. Page 5, first few paragraphs of the section "Gird" and many other places in the text. The authors use too many paragraphs. Some should be consolidated. E.g. the first 2 paragraphs of this section should be joined.

3. Eq 28: So I presume here the authors use j as the vertical index. That should be specified. Also maybe at some point the authors should point out that only the bottom and top boundary conditions (is it detailed sufficiently?) need a special treatment like this since the other are periodic.

4. Eq 28 again: At some point later in the paper I thought the authors mention that with 4th order accurate scheme 2 ghost cells are needed. If that is so, why is there a need for a biased formulation in 28 that would only use one ghost cell below the surface.

5. Eq 36 and other places: it would be useful if for each of these options (2nd versus 4th order for example), the flag that controls it in the code input file is listed. This will make it easy for the user to see how to control these options.

6. Eq 41: tilde is later used for filtering. Maybe denote the intermediate velocity with something else like an asterisk.

7. The fact that the code is mainly periodic in the horizontal direction should be underlined earlier in the paper than it is now. Maybe in the abstract.

8. After Eq 47: please provide a reference to the "Thomas algorithm"

9. LES equations 63 and so on are only for very high RE, i.e. wall modeled LES. Please specify that. Also it would be simple to use the code as a finite Re LES code by keeping the viscous term in 63. Why is this not pursued?

10. "Surface Model" section. The authors only provide the LES surface model. This should be specified. Also better is to add a description of how the DNS wall boundary

condition is treated, presumably through a viscous wall stress. Also the language seems to suggest that the LES is only over rough walls. There is nothing that prevent the code from simulating a smooth surface using the z0 ($\sim \nu$/u*) of a smooth wall. This should be clarified.

11. First line after eq 73: please add “kinematic” to the description of B0.

12. Eq 74: the application of a log law to each velocity component separately is an approximation so the equals here should be replaced by $\approx$. Also this is a LOCAL MOST wall model. This is not a trivial detail and should be specified and discussed briefly with references to papers that discuss the implications in more detail.

13. Eqs 87 and 88: why not use an explicit approach using the fluxes at the previous time step? This is commonly done and since the CFL condition is typically quite < 1 this should be ok? What are the advantages of an explicit approach?

14. Eq 90 is confusing. For example under steady state this almost looks like the pressure gradient is 0. Should the mean RHS <f1> be added? The fact that the pressure gradient force must balance the surface stress force under steady state should be stated.

15. Eq 93: is the momentum balance changed when a subsidence velocity is added to scalars?

16. Page 18 lines 9-11: please provide reference or URLs for these libraries and codes.

17. Figure 1: which of the blue or green is the energy conserving 4th order or the most accurate. Also did the authors describe the 2 methods using these names in the numerics section?

18. ALL figures look like they have problems with some axis labels (some minus signs appear) and so on, please improve quality. If all looks good on the authors computers check that the PDF appears the same on other machines.

19. Why include RK3 in the code release at all given the results?

20. Page 20 line 9, delete “for”

21. Figure 2: slope at smallest dt looks the same for RK3 and RK4, no?

22. Figure 4: symbols not appearing in legend.

23. Section 8.4: give some info about MOSER code for comparison.

24. Page 22 line 6-10: use of word “data” to describe MOSER results is not a good choice here.

25. Figure 6: clearly the spectra of MOSER have some noise or aliasing issues that should be mentioned.

26. Page 24 Line 17: here the authors use the term “potential temperature flux” but previous they used “buoyancy flux”. Pick one since they mean the same thing in dry cases. I would suggest potential T flux since it is a more accurate physics description.

27. Figure 7: maybe use log scale for y.

28. Page 25 line 8: delete “quickly”

29. Figure 9a: area coverage of what? Updrafts? Please clarify.

30. Section 9.3 and in general how is the code initialized? Random perturbations are added to mean profiles? Did the author try alternative approaches to seed turbulence?

31. Section 10: please provide info about the machines in section 10.1 (interconnect speed, processors per node, memory per nodes, ...). These details are needed to understand code scaling.

32. Figure 11: x axis label should be “processors”

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2017-41, 2017.