Rebuttal to review of Dr. Bou-Zeid

The authors thank the reviewer for his kind words in his opening paragraph. We will address his comments point-by-point.

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.*
   Q can be any source or sink of heat. Phase changes are excluded from Q, as the dry dynamics do not support those, and the moist dynamics are based on the liquid water potential temperature that is constant under phase changes. The reviewer is correct that sources and sinks need to be included in Eq. 13 as well, and we will do so in the revised manuscript.

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.*
   We will carefully go through the text and merge paragraphs at the suggested location and wherever appropriate.

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.*
   The reviewer is correct that j is the vertical index. We will explicitly mention this in the revised manuscript. We will also include an explicit reference to the fact that only the vertical dimension needs a special treatment.

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.*
   Many operations involve a sequential application of two operators. For instance, in the 4^{th}-order diffusion, we compute the laplacian as the divergence of a gradient. In this operation, only the gradient can make use of both ghost cells, but the divergence cannot, and therefore relies on a biased operator at the wall.

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.*
   MicroHH comes with a document that lists all the available options. We have failed to mention this in the text and will add it to the revised manuscript. We will explain as well in the revised manuscript that the model defaults to the order of generated grid.

6. *Eq 41: tilde is later used for filtering. Maybe denote the intermediate velocity with something else like an asterisk.*
   We will follow the suggestion of the reviewer to avoid confusion between filtered

variables and the intermediate velocity.

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.*
   We agree with the reviewer that an earlier notification is necessary, because it clarifies both the grid description and the pressure solver. We will introduce it in the introduction of the revised manuscript.

8. *After Eq 47: please provide a reference to the "Thomas algorithm"*
   We will include a reference in the revised manuscript.

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?*
   We will follow the reviewer's suggestion and mention that our LES is developed for very high Re. Extending our code to a finite Re LES code would be trivial, but has not been pursued yet. The reason is that most MicroHH users that run the model in LES-mode run atmospheric cases.

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 (~ v/u*) of a smooth wall. This should be clarified.*
    The description of the DNS boundary conditions is contained in 3.7, but we failed to make this clear to the reviewer. We will improve both Section 3.7 and 4.2 to clarify our implementation. The code could indeed specify the z0 of a smooth wall, but also here, it has not been implemented yet.

11. *First line after eq 73: please add "kinematic" to the description of B0.*
    Correct. We will add this.

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 ≈. 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.*
    We will introduce the approximation symbol. Furthermore, we will discuss the results with respect to existing literature, such as the reviewer's paper in Physics of Fluids (2005).

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?*
    Using the fluxes of the previous time step is often a good solution, but can lead to inaccuracies under, for instance, free convection, where fluxes and wind speeds can change fast at the surface, or under conditions of changing stability. Our methods

have a 100% convergence guarantee under all conditions. Furthermore, it is based on a lookup table that starts searching from the value at the previous time step, which makes it a very fast procedure.

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.*
    With Eq. 90, we aimed to show that the forcing is just part of the total tendency (note the $F_{p;ls}$ suffix). It is a definition rather than an equality. As we have failed to explain it properly, we will clarify this in the improved manuscript.

15. *Eq 93: is the momentum balance changed when a subsidence velocity is added to scalars?*
    It is not. Solving the momentum balance in a doubly-periodic domain under subsidence conditions is a non-trivial exercise that deserves its own study. We follow the simplified treatment that is used in other codes such as DALES and UCLALES. We will add an additional explanation to the paper.

16. *Page 18 lines 9-11: please provide reference or URLs for these libraries and codes.*
    We will add URLs to the referenced libraries and tools.

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?*
    The green line is the energy-conserving discretization, whereas the blue line is the accurate one. Surprisingly, the energy-conserving discretization is in the Taylor-Green-vortex test case also the most accurate one, but this does not apply to all test cases. We forgot to explain the abbreviations in the legend of Figure 1, and will do so in the figure caption of the revised manuscript. Furthermore, we will improve the color scheme to ensure that all cases can be easily distinguished.

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.*
    Something apparently went wrong in the process of adding the GMD logos to the manuscript. In the current online version, as well as in the revised manuscript, all labels are in order.

19. *Why include RK3 in the code release at all given the results?*
    We will keep the RK3 case for testing purposes and for potential extension with implicit-in-time diffusion in the future. The reviewer is correct that our tests show that the RK4 scheme is beneficial under all conditions.

20. *Page 20 line 9, delete "for"*
    We will fix the sentence.

21. *Figure 2: slope at smallest dt looks the same for RK3 and RK4, no?*
    It appears so. The lines are bumpy and the exact slopes are hard to extract. We hope nonetheless, that the reviewer is convinced about the difference in convergence and accuracy between the two methods.

22. *Figure 4: symbols not appearing in legend.*
    We believe this is related to the previous problem (point 18) we had with the figure axes. In our current version, all symbols are visible.

23. *Section 8.4: give some info about MOSER code for comparison.*
    The code of MOSER is spectral with Chebychev polynomials in the non-periodic dimension. We will explain this in the revised manuscript.

24. *Page 22 line 6-10: use of word "data" to describe MOSER results is not a good choice here.*
    We will refer to MOSER's result as "model output data", rather than "data" in the revised manuscript.

25. *Figure 6: clearly the spectra of MOSER have some noise or aliasing issues that should be mentioned.*
    The spectra of MOSER display aliasing in the pressure data, most likely related to the velocity multiplications in the Poisson equation that solves for the pressure. We will make this clear in the text.

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.*
    We distinguish between the two. The dry dynamics have potential temperature as the governing variable, therefore the bottom BC is a potential temperature flux. Our simplified thermodynamics use buoyancy as the governing variable, and therefore a kinematic buoyancy flux as the bottom BC. We will clarify the text.

27. *Figure 7: maybe use log scale for y.*
    We will remake the figure with a log scale and introduce it into the revised paper.

28. *Page 25 line 8: delete "quickly"*
    We will remove the word "quickly".

29. *Figure 9a: area coverage of what? Updrafts? Please clarify.*
    We were referring to the area coverage of cloud and cloud-core that are contained in the legend. We will make this explicitly clear in the figure caption in the revised manuscript.

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?*
    The code is initialized with random perturbations over the mean profiles, which is

sufficient for convective cases. We have also the options of introducing large vortices that are more efficient in generating turbulence under neutral or stable conditions. We will explain these options in the revised manuscript.

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.*
    We will introduce references to the machine specifications and introduce a brief description of each of them in the revised manuscript.

32. *Figure 11: x axis label should be "processors"*
    We will fix this in the revised manuscript and use the word "cores".