Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2017-41-RC2, 2017

Geoscientific Model Development Discussions

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.

Anonymous Referee #2

Received and published: 23 May 2017

(Based on gmd-2017-41-manuscript-version2.pdf)

The manuscript describes an anelastic atmospheric finite difference model, called MicroHH 1.0, with extensions to include buoyancy-driven turbulence. The code is open source, built on a C++ library, and uses MPI and CUDA. Some validation results are provided for doubly-periodic domains, with and without the moist turbulence model. Numerical convergence demonstrates 2nd-4th-order accuracy for the dry model, and results match idealized convective turbulence tests. Overall the manuscript is clearly written, reasonably comprehensive, and establishes a capability that can be used for

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Discussion paper

understanding turbulent convection with the addition of more realistic physics parameterizations.

High-level suggested revisions: - In sections 1-2, switching between anelastic and Boussinesq should be made clearer, and with what approximations. In the rest of the paper, it should be clear what "mode" each test is run in. - Claims of conservation should state the caveat that the simplified equations are in flux-conservation form, but that they are not fully mass- or energy-conservative (for example, looking at total mass, \rho_0 + \rho', in equation 10, is not conservative). p7 l18 how is it "fully energy conserving"? - A little more discussion of why this discretization was chosen, and what its benefits/limitations are. A little extra information would be a good way to flesh out the conclusion and provide more context for the reader.

Detailed minor revisions: - p1 abstract: "code reaches speedups of more than ... conventional code" running on what processors? Generally best to express it as a % of peak FLOPS and specific to the two architectures you compare in results. - p1 "approach the synoptic scales" remove the? to clarify, maybe add LES resolution (< 1km?) at "scales of 1000km or more"?

- p2 l3, "order codes" ? Older codes? - p2 last intro paragraph ... it is worth mentioning Sec 5 (output), and 7 (instructions to reproduce), to encourage others to do the same, maybe mention w/ sec 13 or even move those sections to the end? - p2 l18 "constant with height z" maybe restate \rho_0(z) only to support eq. (2)?

- p3 Derivation of eq(4) should be either referenced or add an extra step ... eq (5) should come first, for example, to introduce the potential temperature EOS that's substituted into eq(4). - p3 l20 perturbational pressure form - not conservative / does not match eq (2)?

- p4 l16, introduced N without an equation/definition?

- p6-7 I appreciate the compactness of the notation and clarity in presenting it.

- p7 eq (40), why not use a similarly compact 4th-order 5-pt wide stencil, instead of the larger 7-pt wide one?

- p9 DFT solver eq (45) is not clear . . . assuming periodic bc's or cosine transform for Neumann bc's on pressure? Is there a reference for this approach? - p9 "hat" DFT notation conflicts with "average" notation on p6. - p9 eq(46-47) could mention "corresponding to eq(39-40) respectively" around l17-18? - p9 l24, Ah! That's a big assumption, periodic lateral boundaries. Should be moved up and stated prominantly, along with motivation/limitations. Now I understand why p4 l15 "periodic with slopes" was introduced

- p11 l12-16, is the model-top pressure constant in time or modified every time step? what value is used?

- p12 l5, is filtering actually applied in your algorithm, and if so, at what resolution? Do you do anything to prevent discrete aliasing of unresolved wavelengths? - p12 tilde variables conflict with tilde "intermediate velocity" in eq (41) - p12 eq (67) S_{ij} subscript? and what's the definition of Sˆ2? - p12 l21, Nˆ2 definition here different than above p4 l16?

- p13-15 sec 4.2 . . . is this a new atm turbulence model? The reference Wyngaard (2010) is an entire book, and it is not clear which tests warrant which boundary conditions, etc. p15 l5 is particularly confusing . . . might be worth describing Obukhov length and its use as a stability/mixing parameter, and why a look-up table is needed.

- p15 l11, why would you not just include a background U_f and define a perturbational velocity from that? That would be compatible with periodic bc's, guarantee mass conservation, etc. - p15 bottom "adveciton" should be "advection"?

- p17 l19. "precompiler statements"? Meaning #define of GPU CUDA code? Any thoughts or statements on maintaining the different code bases in your C++ framework?

- p18 top, MPI-IO should have a reference? - p18 l9, change netCDF footnote to reference? - p18, maybe sections 5-7 should be moved/merged with 13 or all in an appendix? - p18 l25, love the post-processing mode based on restart files!

- p19 eq (98) should "4 \pi y" be z? - p19 figure 1 / p20 l1 discussion . . . L1 error in 2D should asymptote to hˆ4, even with 3rd-order boundary errors (O(N) pts * O(hˆ3) boundary error vs. O(Nˆ2) pts O(hˆ4) interior error). Please explain? Also adding a 2nd set of dotted lines for 3rd- and 4th-order on the bottom set if u,v 4M fields will better show the break.

- p20 l8, "diffusion off" you mean viscosity, no source terms, etc. so that total energy should be conserved? What's your equation for "energy" in this test? - p20 l10, "its energy conservation." you mean improved? It doesn't conserve energy exactly. - p20 figure 2, maybe put top figure on log |\DeltaE| scale as well to distinguish the results better?

- p21, line 4. Isn't there a difference in maximum CFL for each as well? - p22, l6, "perfect match" . . . so perfect it's hard to see any difference at all. What do you attribute that too, since you have completely different discretizations, etc. How were the Moser 1999 results so similar? Could you quantify the differences, plot them, and explain them? - p24, l12 . . . ditto for "nearly perfect match" here. Fig 6 also shows a "kink" in E_pp at higher \kappa . Is it worth explaining?

- p26 l8, Fig 9a,d - why are the vertical velocities diverging with resolution?

- p29 bottom p30. By putting these on a single GPU, you are avoiding communication overheads for the GPU. Did you run 1 MPI rank on the GPU? Did you run "n" MPI ranks on the CPU? For the B512 run you are getting very good (90%?) strong scaling for 1-4 CPU nodes.

- p30 l4, "a parameterizations . . . has been" singular? - p30 section 12 . . . could add a more comprehensive summary, call out any limitations or tradeoffs.

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