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Geoscientific Model Development Discussions

Interactive comment on "Adaptation of the meteorological model Meso-NH to laboratory experiments: implementations and validation" *by* Jeanne Colin et al.

Anonymous Referee #2

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General comments

This paper presents an adaptation of the model Meso-NH to run Direct Numerical Simulations (DNS). The authors have introduced two modifications to the code: the addition of the viscous term and the non-slip boundary condition. The code is then tested against exact solutions of the Navier-Stokes equations and against laboratory experiments.

I think that the main idea of the paper is really good: to use a meteorological model in DNS mode in order to, for example, test subgrid parameterizations can be very useful. These experiments can be really helpful as resolution keep growing, and parameter-

ized processes, as turbulence or convection, are getting more resolved

However, I also think that the paper needs substantial improvement before being considered for publication: the test cases are either too simple or too complicated for testing the code, and the paper is often not rigorous enough. Besides, if the authors intend to use the model for complementing experiments in laboratories, they should provide convergence studies in order to be able to compare the results with other DNS studies. For these reasons I recommend major revisions.

Specific comments

1) In section 3 the authors show that Meso-SH recovers the analytic solutions of pure laminar flows. These tests show that the equations are properly coded, but they do not provide any extra information. I think that showing one single example is sufficient. It would be more useful to show convergence studies, in order to know which resolution is necessary with this numerical implementation. This would allow the comparison with other DNS codes. Also, the diffusion of a 2D-Gaussian field is a more common and challenging test for this kind of studies.

2) While I understand that the authors want to compare their simulations with the results of experiments of the same research center in section 4, this is not a good choice for validating the code. My reasons are the following. First, the DNS cannot simulate some features of the real flume, like the rough wall or the top open-boundary. Second, the data in the experiment are insufficient to validate the code close to the near-wall region, which is the most challenging part for boundary layer flows. Also, entrance effects in the flume seem to be very strong when compared with other experiments in wind tunnels, which makes difficult the comparison (therefore the different definitions of \delta). Third, simulating large Reynolds numbers like in the flume installation is quite challenging, and I do not think that the authors have the resources for that. In wall units, the vertical grid spacing can be estimated as delta z+ = 10, which is probably insufficient for boundary layer flows. All these factors might explain the differences

between experiment and simulations, but the validation of the code is not satisfactory. I would suggest the authors to compare their results with other DNS codes in channel or boundary layer flows, and/or with other experiments in smaller facilities. The classical references for boundary layer and channel flows, Spalart (1988) and Kim et al. (1987), are still good but probably more modern DNS/experiments could also be included.

3) The approximation of using a high viscosity in DNS to compare with meteorological flows in section 2.2 should be better discussed. My current experience is that many meteorologists still distrust DNS, and one should be very careful with this kind of comments. The high-viscosity approximation is only valid if Reynolds-number independency, and therefore enough scale separation, is achieved (see Pope). This is the case with turbulent mixing when Re_\lambda \sim 50 (Dimotakis 2005) for neutral stratification. However, even for very high Re_\lambda, the high-viscosity approximation can fail in regions where small eddies or viscous effects are important. These are the cases of the flow close to the wall, or with a strong stratification. Reynolds-number independency should be always checked with simulations with different Reynolds numbers, and not taken by granted. This is also the case for your boundary-layer simulation in section 4.

4) The authors should use non-dimensional numbers when describing the experiments in section 3, as it is standard in the fluid-dynamics literature. This makes easier the comparison with other experiments. For example, when writing the problem with non-dimensional numbers the three cases in section 3.2.2 are the same; they only differ in the time stepping. This comment is not against providing some reference length and time scales, which can be useful to some readers.

5) In section 4 the authors state which non-dimensional numbers are relevant for the experiment, but they do not discuss them. Please use the non-dimensional numbers to discuss which effects are relevant. The Froude number should be used to discuss if the free surface of the flume (which is not possible to simulate with the current code) is relevant. The aspect ratio d2 should be be used to discuss the width-effects. The length

of the flume could be use to discuss finite length effects. Discuss also the Reynolds number in the code and in the simulations.

6) Define the Komogorov length scale from the dissipation rate as it is commonly done in fluid dynamics literature. There is not point in providing only an approximation when you have all the data from the DNS.

7) Use wall units when presenting results in figures 14. Also it is more usual to fit \kappa and not ustar, which can be taken from measurements/simulations. You should also show the velocities close to the wall as they approach zero, and compare them to some boundary-layer reference (see above).

8) The roughness-length model cannot be applied to the simulations with non-slip boundary conditions, as done in page 15. The non-slip condition creates a viscous layer close to the wall, which is equivalent to the roughness layer but it is not the same. Using the proper viscosity and the right resolution is critical to get the right flow in the viscous layer. The flow close to the wall determines the surface drag u*, and therefore also influences the mean flow. It also determines the constant $B \sim 5.4$ from the Prandtl log law (see Pope page 274), which is independent from the roughness length. It is difficult to determine if the flow in the experimental facility close to the wall is dominated by roughness (there is no data of the roughness length), which makes difficult to compare with simulations.

Typos and small comments

1) Large grid in abstract is vague, use proper dimensions. 2) Page 2, line 23. I would use “complement the experimental data” instead of complete. 3) Page 4, line 10. Gas constant R 4) Equation (3). \rho_ref is missing from the momentum term. 5) Page 4, line 10. In LES the dissipation of energy is often done by the turbulence subgrid scheme. 6) Page 7, line 28. Bracket missing. 7) Section 3. A reference from a textbook would be useful when discussing simple flows. 8) Page 10, line 20. Why resolution does not really matter? 9) What do you mean by false floor in the experimental flume?

10) Page 15, line 12: shear velocity. 11) Figures 8 and 9. State which field is plotted.

References

Dimotakis, P. E., 2005: Turbulent mixing in stratified fluids. Annu. Rev. Fluid Mech., 37, 329–356, doi:10.1146/ annurev.fluid.36.050802.122015.

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