Review on article with title "On the suitability of general-purpose finite-volume-based solvers for the simulation of atmospheric-boundary-layer flow"

The paper is a thorough characterization of OpenFOAM for large-eddy simulations of half-channel flows with reference grid sizes 64^3. In particular, the authors consider a Smagorinsky subgrid-scale model with wall modeling. OpenFOAM is based on a co-located finite volume formulation and the authors compare the results with those obtained from a pseudo-spectral–finite-difference method. The authors study the sensitivity to grid size and grid aspect ratio.

OpenFOAM is an open-source, versatile software tool used for engineering and environmental applications both in industry and academia, including wind-energy applications. The characterization of OpenFOAM when applied to simulate the ABL might therefore be of interest. Nonetheless, I found that some aspects of the manuscript should be strengthen or clarified before publication:

Major points

1. This paper is about OpenFOAM rather than a generic finite-volume model. This is stated by the authors in line 195 *"The results herein proposed are hence representative of the OpenFOAM solver with the standard wall-layer treatment – the set-up that is most commonly adopted when using this code"*.

   This information first comes in line 87. The relevance of the paper would be clarified and improved if this information is clearly stated earlier in the manuscript, in the abstract and maybe the title. The abstract should also indicate that the analysis is restricted to the Smagorinsky subgrid-scale model because this is important to interpret the results.

2. The paper considers an "open-channel-flow set-up", and not an atmospheric boundary layer, and this information only comes in line 150. This information should also be in the abstract and the introduction.

3. The sensitivities that the authors study greatly depend on the resolution, measured in terms of the number of grid points (or an effective Reynolds number [Sullivan and Patton, 2011], or the ratio of the filter size to the integral length scale). Therefore, the grid size that the authors consider in the analysis should also be in the abstract, the introduction, and the conclusions. This information is important to interpret the results.

   The sensitivity to grid size is particularly large for the resolutions that the authors consider, which are lower than in common ABL studies. In line 165, the authors write *"The chosen grid resolutions are in line with those typically used in studies of ABL flows (see, e.g., Salesky et al., 2017)."*, but Salesky et al. 2017 uses 160^3 or 256^3, which is a substantial difference to 64^3. Resolution studies consider even larger grid sizes [Sullivan and Patton, 2011].

4. The statements regarding the dependence of the results on resolution are too general. For instance, the authors write

   - in line 5, *"It is found that first- and second-order velocity statistics are sensitive to the grid resolution and to the details of the near-wall numerical treatment, and a general improvement is observed with horizontal grid refinement. Higher-order statistics, spectra and autocorrelations of the streamwise velocity, on the contrary, are consistently mispredicted, regardless of the grid resolution."*
   - in line 20, *"Although mean flow and second-order statistics become acceptable provided sufficient grid resolution, the use of said solvers might prove problematic for studies requiring accurate higher-order statistics, velocity spectra and turbulence topology."*
   - in line 70, *"the excessive damping of resolved-scale energy at high wavenumber is likely to compromise their predictive capabilities for high-Reynolds ABL-flow applications."*
   - in line 222, *"Grid refinement in the horizontal directions leads to an improved matching between the FV and the PSFD solver, both in terms of shape and magnitude."*
   - in line 233, *"Grid refinement in the horizontal directions improves the matching between the FV-based and the PSFD-based [...]"*

   It might be more useful to say how much this dependence on resolution is, i.e., how much one particular property change when changing resolution around a particular value. In the end, as the grid is refined, we would reproduce better and better more and more properties. The important question is what grid size (or effective Reynolds number, or ratio between the filter size and the integral length scale) we need to obtain certain statistics with a given accuracy, in this case, when using OpenFOAM with a Smagorinsky subgrid-scale model in wall-bounded shear flows.

   For instance, for direct numerical simulations, we know that second-order methods typically need twice the resolution of spectral methods to similarly represent the variances [Moin and Mahesh, 1998]. What would be the equivalent for OpenFOAM in the model configuration considered in this study?

   This comment relates to what the authors write in line 83: *"Note that the studies conducted with FV-based solvers are mainly focused on first- and second-order flow statistics, which are themselves not sufficient to fully characterize turbulence– and related transport– in the ABL."*. What do the authors mean by "fully characterize"? For some applications, correctly representing the first- and second-order moments might be sufficient, whereas for other applications (atmospheric chemistry, wild fires) representing the spectra and LSMs might be insufficient.

5. The introduction reads too much as a review, the focus on OpenFOAM appearing first and unexpectedly in lines 85-90. It might be useful to focus more the introduction around OpenFOAM, the half-channel configuration, and the kind of grid sizes that are considered in this analysis. This might help setting the right expectations earlier in the paper.

   In a similar line, the review on LSM between lines 260 to 275 might be shortened.

6. In line 187, the authors indicate that the log-layer mismatch observed in this study is a well-known problem of wall models.

   In line 218, the authors indicate that rms-deviations observed in this study is a well-known problem in FV-based WMLES.

What is then new in this manuscript? The particularization to OpenFOAM at this particular resolution? I guess this comment relates to point 1.

Minor points

1. In line 137, I am not sure I understand where $u_\tau = \sqrt{\tau_{\alpha 2,w}|\mathbf{u}|/u_\alpha}$ comes from.

   do not understand equations 5 to 6. Related to it "no-slip applies at the lower surface" in line 153 is strange...

2. In line 154, the authors write *"The kinematic viscosity is set to* 10^{-7} m^2/s *in the bulk of the flow, resulting in* $Re_\tau = 10^7$*"*. I think the information about Re_τ is meaningless because the effective Reynolds number introduced by the subgrid-scale diffusivity is much smaller. As the authors later say, one can neglect the molecular viscosity against the subgrid-scale viscosity. The value of the viscosity is also a bit strange for an ABL context.

3. Adding colors in the figures might help the reader to distinguish the various cases more easily.

4. In line 227, the authors refer to the results of del Alamo et al 2006 regarding skewness, flatness and correlation coefficient. It migtht be useful to add this data to figure 3.

5. In table 3, why taking the tangent point to $\kappa^{-5/3}$ to distinguish between inertial and large-scale and not some integral length scale [Pope, 2000]? Moreover, 32 points seem too few to distinguish an inertial subrange.

References

P. Moin and K. Mahesh. Direct numerical simulation: A tool in turbulence research. *Annu. Rev. Fluid Mech.*, 30:539–578, 1998.

S. B. Pope. *Turbulent Flows.* Cambridge University Press, 2000.

P. P. Sullivan and E. G. Patton. The effect of mesh resolution on convective boundary layer statistics and structures generated by large-eddy simulations. *J. Atmos. Sci.*, 68:2395–2415, 2011.