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Interactive Comment

Interactive comment on "Influence of grid aspect ratio on planetary boundary layer turbulence in large-eddy simulations" *by* S. Nishizawa et al.

Anonymous Referee #2

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A review of GMD Discussion, Article # gmd-2015-97:

Influence of grid aspect ratio on planetary boundary layer turbulence in large-eddy simulations

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Summary: This manuscript presents results of a parametric LES study of convectively stratified ABL flow. The authors have identified an important topic – resolution and filter width in the rectangular prismatic computational mesh arrangements typical of ABL simulations are indeed common and this effect is important. However, in its present form, I am concerned about this paper. Since the publication format for this journal differs from what I am accustomed to, I cannot recommend that this manuscript be declined for publication (it appears to have already been published). I do not, however, consider this work to be complete and I cannot endorse its scientific veracity. My review is composed of Major Concerns and Minor Concerns.

Major Concerns -Writing: I recommend that the authors print a hard copy of the text and review it – line by line – for grammatical errors. I found only a few spelling errors, but the grammatical errors make the test extremely difficult to read. For example, the second sentence of the abstract is “In order to distinguish them as much as possible. . .” this is poor writing. -Introduction: To me, it seems that a large portion of the Introduction was not relevant to the manuscript. In fact, it seems that a large portion of the manuscript spanning Section 1, p. 6022 to 6023, could be removed and instead the authors could go directly to the challenge of non-cubic computational meshes in ABL simulations. The authors discuss phenomena spanning the spatial ranges of Earth’s atmosphere when, in reality, only matters relating to LES of the PBL are pertinent. -Introduction: p. 6024: “With the rapid development of computers LES has recently. . .” what do the authors mean by recently? LES is now nearly 50 years old. Also, the appropriate benchmark Deardorff paper to cite is: Deardorff, 1970: J. Fluid Mech. 41, 453—480. Similarly, the authors state that “The theory of LES is based on dynamics of three-dimensional isotropic turbulence”. The theory of LES is simply that spatial filtering of flow quantities at high Reynolds number results in Reynolds decomposition of the flow quantities into resolved and unresolved scales. One can of course assume isotropic, homogeneous turbulence and use the turbulence kinetic energy transport equation to derive that C_s = 0.16 (for example, please see Professor Pope’s “Turbulent Flows” book). -Introduction: I think that a simple sketch of a ‘typical’ ABL computational domain illustrating how the notion of aspect ratio not equal to one would assist with the introductory remarks. I also recommend that the authors provide an equation for their definition of aspect ratio. -2.1 Dynamics: first line on p. 6026 when defining the domain: x is the Euclidian vector and should be bold. -2.1 Dynamics: The authors state “The reason the advection terms are. . .diffusion terms representing effect of SGS turbulence.” The higher order schemes are needed since the advective term is a nonlinear convolution and, as such, it requires higher order treatment to resolve additional modes. Here, I have another concern: the ABL generally exhibits two spatially homogeneous dimensions (see Professor Wyngaard's "Turbulence in the Atmosphere") and this enables us to compute fluctuations of the resolved quantities based on deviation from the plane-average. It also allows the opportunity to perform spectral discretization for the advective term, thus attaining spectral accuracy with O(NlogN) operations. In fact, the authors say on p. 6033 that ffts are used. This matter needs to be clarified. -Clarification: p. 6029: The derived values are 0.16 (Pope, "Turbulent Flows"). -Stratification regimes: why convective? Why not a channel (neutral) or stable? It seems to me that unstable stratification removes clarity from the study by adding another parameter instead of addressing the concerns as described in the abstract. -Plotting of variables: z(m) instead of z/H or, more importantly when considering resolution effects in LES, z/Delta -Landscapes: In truth, PBL flows over homogeneous topography are rather trite, scientifically. I recognize that filter width and grid aspect ratio are the topic of this study, I encourage the authors to explore the role of resolution in capturing important dynamics due to the presence of rudimentary landscape heterogeneities (i.e. heterogeneity in heat flux or aerodynamic surface stress). -Section 2.2.2: p. 6030. Beyond empirical models, the onset of dynamic SGS models profoundly influenced LES. I know the authors are using the constant Smagorinsky model, but a brief mention of the dynamic modeling procedures seems relevant to me. -Section 3: p. 6033 says there is "background flow of 5 m/s." Is this the streamwise component of the geostropic wind, Ug? If so, this should be related to the streamwise pressure gradient forcing (which I suspect is actually used to force the flow) and then to the friction velocity, u_*, and the friction velocity should be offered. -Section 4: I have a major concern that, in many places, the axis numbers are so small that they cannot be read. I also could not discern the axis labeling. This is a serious "small mistake" that should be corrected. -Section 4.1: On Figure 1, I recommend that the authors add vertical lines representing the grid- and filter-widths. This will help readers relate wavelengths with excessive dissipation to details of the SGS modeling procedure. Also, since the variance is the square root of the integral of the spectrum, the authors could compare variance for the difference cases against resolution which would be helpful (see, for example, Fig. 5 of Bou-Zeid et al., 2005: Physics of Fluids 17, 025105). -Section 4.1: Following the above comment, I propose a different function for comparing deviation from the idealized spectrum:

SEP = int_Delta^L |E(k) – Ak^(-5/3)| dk

since this would compute the variance between the idealized and LES spectrum and provide a better number of how failure to resolve additional scales manifests with reduced variance and therefore lower mixing. -Section 4.2: To me, I do not like the gray shaded region illustrating the range of possible values since trends are more important. I recommend the results be plotted in a different way so as to show some kind of monotonic variation with changing filter/grid width. -Section 4.2: p. 6038: The sentence “This tendency can be reasonably understood. . .” I think is not accurate. Instead, it is that increasing the resolution increases (tilde{u}^prime)^2 and therefore greater mixing. Also: the authors say “We can conclude that the total vertical heat flux is reasonably reproduced regardless of grid configuration.” What is physically responsible for this? -Section 4.2: I think the authors should present a plot showing profiles of (tilde{u}^prime)^2 against z/Delta – in order to make a strong comparison on the role of resolution and, more importantly, grid ratio.

Minor Concerns -Spelling: p. 6029, “where Ck is an SGS. . .” should be “where Ck is a SGS. . .” -Spelling: p. 6029, “Wynggard” should be “Wyngaard” -Spelling: p. 6035, “The spectra shows an spurious energy. . .” -Grammer: p. 6036: “Since the energy spectrum is not perfectly logarithmically linear. . .” should be simply “Since the energy spectrum is not a power law. . .”

Interactive comment on Geosci. Model Dev. Discuss., 8, 6021, 2015.