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Geoscientific Model Development Discussions

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

E. Goodfriend (Referee)

egoodfriend@lbl.gov

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The authors present a useful and well supported study of the effect of the filter length definition on LES results on grids with a range of aspect ratios. The results show that a filter length definition that considers the grid aspect ratio and the effective width of the explicit filter reduces spurious build up of energy at middle wave numbers. These results are novel and important. This study is accompanied by an examination of the effect of the grid aspect ratio on a range of turbulent statistics, which shows that the aspect ratio has an effect on surface layer depth, variance of vertical velocity, and skewness. These results are less important but do not compromise the value of the paper.

On the whole, this paper is interesting and well written. Please see the following sug-

gestions for improving the clarity of presentation.

Specific Comments

Page 6026, Lines 3-16: The discussion of the decision to use fourth-order schemes for advection terms and second-order schemes for the other terms is not clear. The authors argue that, because the eddy viscosity and SGS diffusion terms will be effectively fourth order because they are proportional to the square of the grid spacing, the advection term should also be fourth order. This argument does not discuss the pressure gradient term, which is also second order but is not proportional to the square of the grid spacing. Moreover, the only convergence results presented, in Figure A4 for the potential temperature, show second-order convergence. Since otherwise this study attempts to minimize the effect of the discretization to focus on the grid aspect ratio, it is important to justify this non-traditional approach. Please rewrite this paragraph to clarify the decision to use schemes of different order for different terms.

Page 6035, Lines 5-6: The text notes that only runs with vertical grid spacing of 10 m are shown in the results, but does not discuss whether any dependence of the results on vertical grid spacing were observed. Please include a discussion of the dependence of the filter length results on the vertical grid resolution.

Page 6036, Equation 21: It is not clear why the SEP index uses the maximum of the ratio of the observed to expected energy. As the manuscript states, the index is not equal to one even for the test case used to generate the expected energy, since the energy spectrum is not perfectly linear. It seems that the maximum error would highlight the part of the spectrum that is not expected to have -5/3 slope, i.e. the large, energy-containing scales, unless the SEP is only calculated for length scales within the inertial range. Please clarify why the SEP index is defined as the maximum of the ratio.

Page 6036: In Figure 4, it looks like the SEP is closer to one for tests with a coarser, 30 m vertical resolution. This seems counterintuitive, since the SEP is defined against a finer, 10 m resolution test. Please discuss this result in the main text.

Page 6037, Line 15: The text notes that the turbulence statistics results were averaged over the last half hour of the tests, but the paper does not present any results regarding the convergence of the tests. The paper also does not appear to assert that runs reached a statistical steady state appropriate for drawing conclusions for either set of results. Please include results demonstrating that runs reached steady state before data were analyzed. These results would be appropriate in Section 3.

Page 6037: The manuscript does not state which filter length definition was used for the turbulence statistics results. It is difficult to consider the relevance of these results without knowing their connection to the previous discussion of the importance of the filter length definition. Please specify which filter length definition was used for these results.

Page 6039, Lines 9-12: The significance of the convergence point for the variance of vertical velocity is not clear. The theoretical variance, as calculated with Equation 23, is not noted in Figure 6b, so it is not clear how far the results are from this theoretical point. Furthermore, the statement that "the convergence point is still a debatable issue at higher resolutions" seems to undermine the discussion by stating that the convergence point is not generally accepted. Please rewrite this section to clarify the interpretation of the vertical velocity variance results.

Page 6040, Lines 1-3: The authors state that the explanation given by Sullivan and Patton (2011) regarding the dependency of the skewness on horizontal resolution is not sufficient, but this statement is not supported. Please rewrite to support this statement.

Page 6040, Lines 14-20: The text discusses residuals from a logarithmic linear regression of the skewness, but these results are not clearly presented. The residuals are not obvious from Figure 6d, and are not presented in a table. Please include a new figure or table presenting the residuals, or rewrite the discussion to clarify how the residuals are already shown in the presented results.

Technical Corrections

Page 6023, Line 7: For consistency, please specify the range of horizontal length scales associated with synoptic scale phenomena.

Page 6027, Equation 1: This formulation of the Smagorinsky-Lilly eddy viscosity SGS turbulence closure is not the standard one. Please check the equation, and provide a citation if the equation is correct.

Page 6034, Lines 1-2: The manuscript does not specify which processes are dynamical and which are physical, nor does it provide a reference to the section of the appendices that discuss this difference. For clarity, it would be better to briefly outline here, in the main body, which processes are dynamical versus physical, and provide a reference to the details in the appendix.

Page 6035, Line 18: The slope should be -5/3, not -3/5.

Page 6036, Line 14: The slope should be -5/3, not -3/5.

Page 6037, footnote: The footnote states that the results are qualitatively robust, but does not support that statement. Please justify the statement that the results are qualitatively robust, for example by stating that the results were calculated for a range of thresholds with similar conclusions.

Page 6038, Lines 17-18: Please clarify at which height the variance in vertical velocity was 1.75 and 1.4 m2/s2.

Page 6043, Line 9: It would be useful to include references to previously published work using this code.

Page 6085-6086, Figures 1 and 2: These figures are difficult to read. Please increase the font size in the figures (axis labels, titles, etc) and increase the thickness of the lines.

Page 6086, Figure 2: Please state in the caption which experiment generated the results in these figures: control, small filter length, or fixed mixing length.

Page 6088, Figure 4: These figures are hard to read, please increase the font size on the axis labels. Also, please include a label for the y-axes.

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