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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.

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Reply to referee #1

We appreciate referee #1(Dr. Goodfriend) for careful reading our manuscript and for giving useful comments. We would like to reply to referee #1 according his comments as follows.

General comment

"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."

[Reply]

We argue that the dependency of turbulent statistics on the grid configuration is a very important issue for the following reason. As well as the vertical transport of the heat and water vapor, the turbulent statistics that the reviewer pointed out have also important roles in the atmospheric phenomena through the interactions with the ground and the variability in the free atmosphere at the bottom and top of the PBL, respectively.

For instance, the vertical motion at the top of the PBL is sometimes amplified by the phase change of water substance, and it becomes a trigger of the deep convection. In other word, it is a control factor for the timing and location of the heavy precipitation occurrence. Thus, the turbulent aspects such as variance of vertical velocity and skewness have strong impacts on convection development in the free atmosphere. The quantitative evaluation of these turbulence statistics would give fundamental information to convection onset mechanism.

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."

[Relpy]

We appreciate the reviewer's this comment very much. There are several reasons why we choose the scheme for the terms. The direct answer to this comment is associated with the scale-separation of the fast modes (acoustic and fast gravity waves) and slow modes (advection). In the meteorological phenomena, the terms of the pressure gradient in the momentum equations and the divergence in the continuity equation is dominant for the fast modes, while the advection term is dominant for the slow modes. The interaction between the fast and slow modes is not significant generally. If we consider the SGS mixing in a local field such as several grids scale, the fast waves would pass over this field soon before completing the SGS mixing. This means that the fast waves do not participates the local mixing so much, compared with the mixing process itself.

The reviewer criticized that Figure A4 still show the second order. Indeed, the error aspect in Figure A4 does not show the 4th order. However, the error metrics of L2 norm means for the global accuracy in the deterministic test case. If we consider the local mixing mechanism described above, the global accuracy should be separately discussed.

Based on this consideration, we should take much care of the relation between the terms of the eddy viscosity/diffusion and advection, because the eddy viscosity and diffusion terms are originated from the advection terms. The consideration of relationship between these terms is the most important from the viewpoint of truncation error of advection scheme as described in our text; the mixing coefficient in the SGS scheme is proportional to the square of grid spacing, so that the truncation error of advection should be at least higher order than second order.

Additionally, the anonymous referee #2 pointed the necessity of the higher order treat-

ment for the advection terms from the different viewpoint: "The higher order schemes are needed since the advective term is a non-linear convolution and, as such, it requires higher order treatment to resolve additional modes." This statement also supports our treatment of advection term.

Upon the above discussion comprehensively, we can conclude that our approach of mixed order schemes are enough valid for the purpose of this paper. Off course, as pointed out by the reviewer, it is well worth that the investigation of impact of the complete 4th order scheme on the results is an important issue in terms of consideration of fast wave effects on the mixing process. We would like to address it as the future issue. In the next revised, we will add this discussion in the text according to the reviewer.

"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."

[Reply]

We add a discussion of the dependency of the SPE on the vertical grid. Please see the below comment.

"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."

[Reply]

The energy at the energy-containing scales is smaller than the extrapolated one from the -5/3 slope as we can see in Figure 1. The ratio, $E(k)/Ak$-$5/3$ [h3]is small at the large scales as well as at the energy-dissipative range. Note that the inverse, i.e., Ak-$5/3/E$, is larger at these ranges than at the inertial range. In fact, the scale at which the ratio has the maximum value is in the inertial range.

"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."

[Reply]

We add a discussion about this as follows. In all the experiment, the magnitude of the SEP tends to be smaller for the higher vertical resolution. [h4]The tendency is more apparent for the larger aspect ratio. It is possible that amount of the energy dissipation depends on the grid configuration, although it should be identical theoretically. The larger dissipation could results in the smaller SEP in the coarser resolution runs.

"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.

[Reply]

This experiment simulates a development phase of the PBL, and the total energy in the system keeps increasing. This corresponds to the real atmosphere on morning after sun-rising to early afternoon. However, we give a constraint that 1) the same

increasing rates of the energy at a horizontal plane are given every run and 2) the increasing rate become smaller as time goes so that the realistic situation is realized. The former means that the accumulation of energy input is same every run and the latter suppresses too deep PBL. In fact, the change of the 30-min averaged kinetic energy at the 500m in t=3-3.5[hr] to that in t=3.5-4 [hr] is just 3.4

"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."

[Reply]

The statistics are analyzed in the control experiment. We will clarify it and give the statement in the text at the next revised.

"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."

[Reply]

We estimate the total energy for kmax = ∞ from on Eq. 23 in the 10mAR1 run, and it is 104

"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."

[Reply]

The discussion was done in the Sullivan and Patton (2011) themselves. For keeping the consistency and avoiding the misunderstanding, we will change the sentence as follows. "Sullivan and Patton (2011) tried to explain the dependency of the skewness on the horizontal resolution by the SGS moment, but they showed that its effect is quantitatively not sufficient to explain the difference."

"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."

[Reply]

We will add the discussion about the residuals in Fig. 6c not 6d. The regression line is added to the Figure 6c, and the residuals become easily to be understood (Fig. 1 of this comment).

Technical Corrections

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

[Reply]

We will add the scales. The synoptic scale phenomena, whose horizontal scales are O(100)-O(1000) km, ...

"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."

[Reply]

The second term of r.h.s is arisen according to the consistency with TKE $= \tau_{kk}/2$ in the compressive flow (e.g. Moin et al. 1991). We will add the above note and the citation.

"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."

[Reply]

We will add the following statement. "We define the dynamical process as that related to the fluid dynamics; the advection, pressure gradient, and gravitational force terms in the governing equations are treated as the dynamical process in this model. The other processes are the physical process in this model. In this study, the physical processes are only the surface flux for the momentum and the eddy viscosity and diffusion for SGS turbulence. Note that we treat the eddy viscosity and diffusion terms, which is originated from the advection term, as the physical process for the correspondence with the sub-grid turbulence model in RANS mode in this model."

"Page 6035, 6036, Line 18: The slope should be -5/3, not -3/5."

[Reply]

We will correct them.

“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.”

[Reply]

We will change the statement as follows. “The conclusions in this paragraph do not change for a range of the threshold qualitatively.”

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

[Reply]

We will specify the height. The variance from the highest and lowest resolution runs (10mAR1 and 30mAR5) are 1.75 and 1.4 m2/s2 at 500 m height, respectively.

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

[Reply]

We place this paper as the reference paper of this model.

“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.”

[Reply]

We will replace the larger fort and 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.”

[Reply]

We will add the statement in the caption.

“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.”

[Reply]

We increase font size and add a label for the y-axes.

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

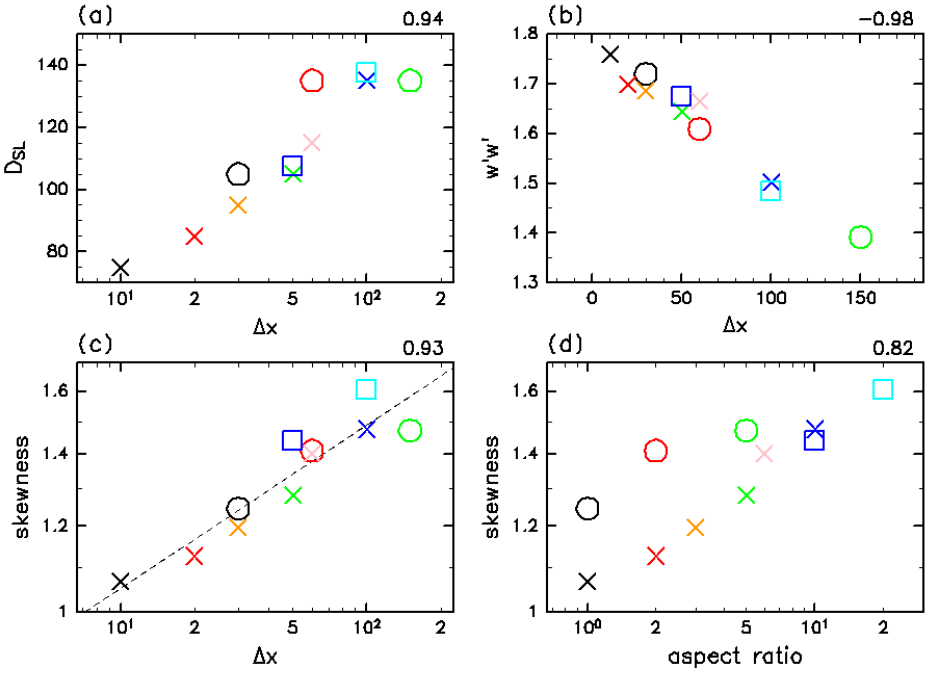

Fig. 1. Figure 6 in the text but the regression line is added in (c).