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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.

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We appreciate the anonymous referee #2 for careful reading our manuscript and for giving useful comments. We would like to reply to referee #2 according his comments as follows.

Major Concerns

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

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abstract is “In order to distinguish them as much as possible: : :” this is poor writing.

[Reply]

We would like to keep on making the text better. We also note that the manuscript was edited by an English editing service; we attache the certificate of English editing.

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.

[Reply]

We think that the reason why such flatten grid configuration has been used in the meteorological LES is strongly related to the historical backgrounds of the meteorological simulations. The explanation of such backgrounds would help readers to understand importance of the investigation of the grid aspect ratio. In order to clarify the purpose of the part, we will revise this part in the introduction, focusing on readability and association with our purpose as much as possible.

The referee #1 said about "non-cubic" grid. We grasped the meaning of his term "non-cubic" as anisotropic grid. However it may mean a kind of non-regular mesh used in the finite element method. If so, we answer to this comment as follows; Before the use of non-regular grid, the detail investigation of the aspect ratio in the regular grid would be prior as a first step.

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.

[Reply]

The sentence was confusing, and we change as the following.
From the 1970s, LES has started being used for meteorological dynamical simulations (e.g., Deardorff, 1980; Moeng and Wyngaard, 1988; Sulivan et al., 1994), and it is recently used for realistic meteorological simulations with the rapid development of computers (e.g., vanZanten et al., 2011).

Also, the appropriate benchmark Deardorff paper to cite is: Deardorff, 1970: J. Fluid Mech. 41, 453âĂŤ480.

[Reply]

As the referee #1 commented, Deardorff (1970) is the key work and the first paper which performed a LES. However, the simulation of the channel flow studied in Deardorff (1970) is kind of a fluid dynamical simulation and it seems not to be an appropriate reference as a meteorological simulation.

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).

[Reply]

It was a little oversimplified, and we change as follows.
The theoretical base of most sub-grid scale (SGS) models in LES is on dynamics of three-dimensional isotropic turbulence.

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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.

[Reply]

We add the mention about a computational domain as follows.
The computational domain for the meteorological LES has often wider range in the horizontal than in the vertical as well as the previous meteorological simulations: e.g., the domain size is 3 km^2 horizontal region and 1 km height in Sulivan et al. (1994); 12.8 km^2 horizontal region and 4 km height in vanZanten et al. (2011).

I also recommend that the authors provide an equation for their definition of aspect ratio.

[Reply]

The equation is added as follows.
In meteorological simulations, the grid aspect ratio a of the horizontal Δx to vertical grid spacing Δz is generally much larger than in other fluid dynamics fields. Here we define the aspect ratio as $a = \Delta x / \Delta z$.

The first line on p. 6026 when defining the domain: x is the Euclidian vector and should be bold.

[Reply]

x is not the Euclidean vector but an element of the set $\{v, l, s\}$; v, l, and s represent vapor, liquid, and solid, respectively.

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

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term is a non-linear convolution and, as such, it requires higher order treatment to resolve additional modes.

[Reply]

We appreciate this reviewer's comment very much. It is a important point that the scheme of advection terms should be higher order, and we would like to add the statement.

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.

[Reply]

Actually, we employ the finite volume method (FVM) for the spatial discretization. We do not use FFTs in the simulation. Although the numerical method of simulation is based on FVM, the analysis of energy is done by the spectral method (FFT). We here distinguish the numerical method from analytic method. We believe this is so reasonable.

Clarification: p. 6029: The derived values are 0.16 (Pope, "Turbulent Flows").

[Reply]

Here we discuss about a SGS constant C_k, which is not the Smagorinsky constant C_s. If our description leads misunderstanding, we will describes this part clearly.

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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.

[Reply]

The vertical transport of heat and water vapor by the turbulence is the most important effect of the turbulence on the atmospheric circulations. The transport is most dominant in the convective regime, in which energy is injected from the surface into the atmosphere. So we focus on the regime in this paper. As the referee pointed out, the effect of the grid aspect ratio on turbulence in the stable and neutral regimes is also important topic, and it is a scope of future works.

Plotting of variables: z(m) instead of z/H or, more importantly when considering resolution effects in LES, z/Delta

[Reply]

The y-axis of Figure 5 is z(m). In our experiment, the domain height H is almost identical, so the vertical profile looks the same if z/H is used as the y-axis instead of z(m).

The z/Delta is useful for a discussion about the profiles near the surface. We add a figure to discuss the profile in the surface layer (see the below comment).

-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).

[Reply]

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The influence of the grid configurations on the turbulence with landscape heterogeneities is a really interesting topic. However, we think that basic knowledge in the case of simpler homogeneous topography should precede an advanced study of the heterogeneous case.

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.

[Reply]

We add the mention about dynamic SGS models as follows.
There are procedures which derive the constant dynamically, called dynamic SGS models (e.g., Germano, 1991). They have advantage in the case where the assumption of the isotropic turbulence is not justified, e.g., very close to a boundary. However, in our opinion, there are some practical problems, e.g., numerical stability, and more importantly, they seem to be sort of a mathematical procedure and seems not to be on physical basis.

p. 6033 says there is "background flow of 5 m/s." Is this the streamwise component of the geostrophic 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.

[Reply]

The term "background flow" was confusing. It is not the geostrophic wind. We change it to "constant flow".

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.

[Reply]

It is quite important point. We increase the font size.

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.

[Reply]

We show the filter-length Δ in Figure 1 (See Fig. 1 in this comment). The grid-width is not shown, because a lot of lines seem to be little bit messy and it is relatively easy to recognized.

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).

[Reply]

The variance is compared in Figures 5 (c) and 6 (b), and we have the discussion of the difference against resolution.

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

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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.

[Reply]

The reduced variance at the energy-dissipative range results in lower mixing by the filtered motion. However it can be compensated by the SGS mixing, so it cannot result in total lower mixing. We introduce the index SEP to represent the magnitude of the spurious energy piling, because the energy piling is artificial, which cannot be compensated by any SGS motions.

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.

[Reply]

A lot of lines in narrow range is quite messy, so we show the range. In order to focus on the change with filter/grid width, we show their dependency in the Figure 6.

p. 6038: The sentence "This tendency can be reasonably understood: : :" I think is not accurate. Instead, it is that increasing the resolution increases (tildeuËĘprime)ËĘ2 and therefore greater mixing.

[Reply]

Figure 2 in this comment shows the profile of θ, $(w')^2$, and $(u')^2+(v')^2$ against z/Δ. The filtered (resolved) variance $(w')^2$ and $(u')^2+(v')^2$ is smaller in the 5mAR20 run than in the 10mAR1 run, then the mixing due to filtered motion is smaller in the 5mAR20 run.

Therefore the deeper surface layer is because of the larger SGS mixing. Note that u and v represent the filtered variable in our paper.

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?

[Reply]

The vertical heat flux by the filtered motion is differ between the runs, however the total heat flux is almost identical. Additionally, the vertical profile of the potential temperature is also almost identical. This means that the temporal accumulated heating rate, which is vertical divergence of the heat flux, is identical. From these results, we conclude that the total vertical heat flux (filtered + SGS) is reproduced in all the runs.

I think the authors should present a plot showing profiles of (tildeuËĘprime)ËĘ2 against z/Delta – in order to make a strong comparison on the role of resolution and, more importantly, grid ratio.

[Reply]

We appreciate the referee’s this comment very much. Figure 2 in this comment shows that the potential temperature and the variance of the horizontal velocity have similar profile against z/Δ in terms of the vertical gradient. This suggests that the vertical gradient of the profile in the surface layer is determined by the filter length.

Minor Concerns

p. 6029, “where Ck is an SGS: : :” should be “where Ck is a SGS: : :”

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p. 6029, “Wynggard” should be “Wyngaard”

p. 6035, “The spectra shows an spurious energy: : :”

p. 6036: “Since the energy spectrum is not perfectly logarithmically linear: : :” should be simply “Since the energy spectrum is not a power law: : :”

[Reply]

We correct the text according to all the comments.

Figure captions

Due to the limit of the number of characters for figure captions in this system, the caption is truncated. So the captions are shown here.

Fig. 1. Same as Figure 1 in the text, but the filter-width is represented by the vertical lines.

Fig. 2. The vertical profiles against z/Δ of (a) the potential temperature, (b) variance of the vertical velocity, and (c) variance of the horizontal velocity in the 10mAR1 (black solid line), 10mAR2 (red solid), 10mAR5 (green solid), 10mAR10 (blue solid), and 30mAR1 (black broken) runs.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C2174/2015/gmdd-8-C2174-2015-supplement.pdf

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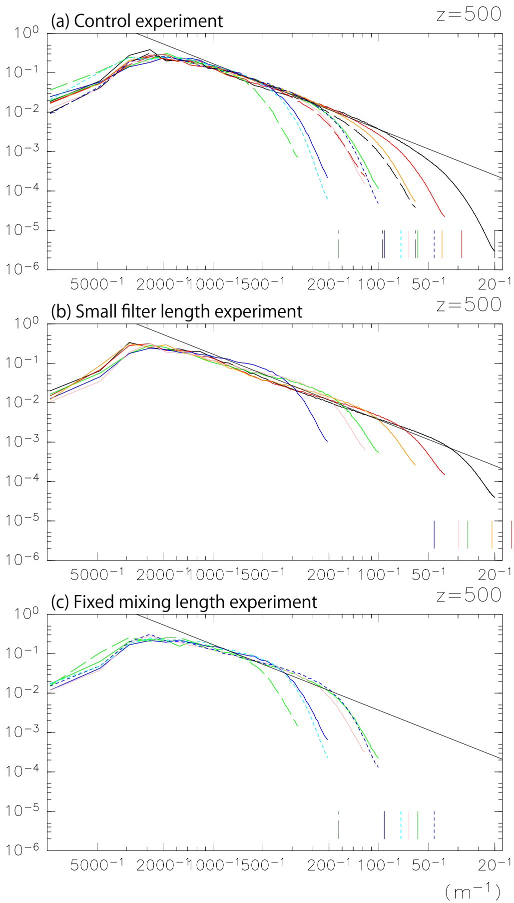

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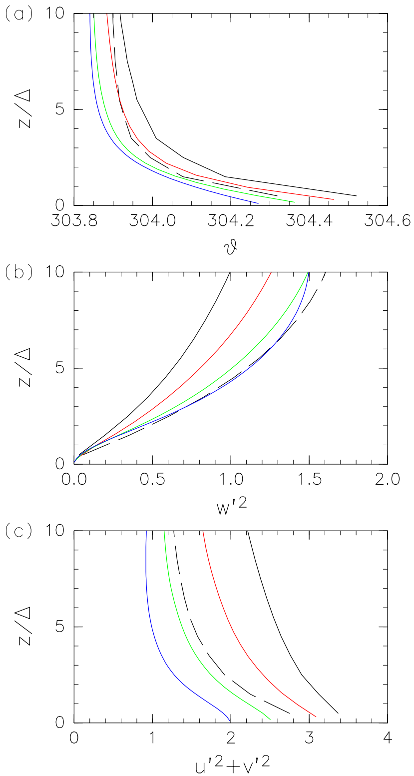

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