Impact of model resolution and turbulence scheme on the representation of mountain waves and turbulence (egusphere-2025-4308)

Roshny Siri Jagan and Juerg Schmidli

Institute for Atmospheric and Environmental Research, Goethe University Frankfurt, Frankfurt, Germany

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Authors response to reviewers

We thank both reviewers for their thoughtful and constructive comments on our manuscript. We appreciate the time and care invested in evaluating our work and providing valuable feedback. We hope that all their concerns have been duly addressed in the revised version of this paper.
Comments by the reviewers are in black, followed by our replies in blue font. As described in detail in response to Reviewer #1, we identified an error in the original configuration of the simulations without the SSO source term in the TKE equation (I10-TKE-HS and I10-TKE-none) during the revision process. This error has now been corrected, leading to changes in the resolved flow field in Fig. 8.

REVIEWER#1

General Comment:

This study investigates mountain waves in ICOsahedral Nonhydrostatic (ICON) model simulations, using two different turbulence schemes and various horizontal resolutions. Results are compared to DEEP-WAVE campaign observations and to ICON-LES simulations.

The first turbulence scheme is the operational turbulent kinetic energy (TKE) scheme, which is comprised of several components, including optional horizontal shear (HS) and subgrid-scale orography (SSO) terms. The second scheme is the newly developed two-energy turbulence scheme (TE). Results of those simulations are partially compared to a flight performed during the DEEPWAVE campaign on the 12 of July 2014.

The manuscript is well written and includes many references. The researched topic is very current and important as high resolution simulations are increasingly common, and mountain waves play a crucial part in atmospheric dynamics. However, none of the two goals mentioned in the abstract, comparison of the different turbulence schemes and comparison to the flight measurements, seems to be sufficiently reached. For this reason, I recommend the manuscript for major revisions, with specific comments below.

Response:

We sincerely appreciate and acknowledge the valuable suggestions of the reviewer. In the revised manuscript, we have addressed all the points / queries raised by the reviewer. Below, we present the point-to-point response to the comments raised by reviewer:

Major Comments

1. Although the different simulations are very well formulated to achieve a good comparison between the two turbulence schemes and various resolutions, they are very limited by looking at a one specific timestep. This was chosen as to compare to the DEEPWAVE flight done at this specific time. However, looking at the one timestep is not enough to make conclusions about the differences between the two turbulence schemes, especially considering there are other times of flights available. If the primary focus of the paper is to compare the schemes and resolutions, I would recommend looking at more timesteps and at longer time averages, especially when it comes to variables such as momentum flux, that show significant differences between the various resolutions and schemes as well. This way the results would be way more robust.

   Response:
   We agree that for a comprehensive comparison of the two turbulence schemes more and different cases should be analyzed. However, this would require much additional work beyond the scope of the current project. To address the reviewer's concerns we have included several time steps in our analysis (see revised Figure 3 and 10), showing that the results for this case are robust, and clarify that our goal is not a comprehensive comparison over many different cases. We think that the current case study is already very instructive and valuable regarding the differences between the two schemes, in particular regarding the impact of the extra terms in the TKE scheme. A more comprehensive statistical evaluation of the two schemes is planned in future work.

2. There is a little comparison done between the observations and the simulations as such. The only figure that compares them is Fig. 4, which does not include the TKE simulations. Once again, to make the comparison more robust, it would be useful to include another flight, e.g. F10 (ref. Fritts

et co. 2015), and another simulation time, or to include some quantitative measurements such as momentum flux calculated from the observations (ref. Smith and co. 2016).

Response:
We have revised Figure 4 and now include also the TKE simulation. It can be seen that in terms of the resolved flow, the two turbulence schemes yield similar results. We have clarified the introduction regarding the aim of our study – a sensitivty analysis, not a comprehensive statistical evaluation of the two schemes. As mentioned above, analyzing different cases is beyond the scope of the current project.

3. It would be useful to include more theoretical and technical details regarding the different turbulence schemes and their components. This would help with the conclusions as they are not quite clear. For example, you show (Fig. 8, Fig. 9 and 10) that the I10-TKE simulation, which includes both HS and SSO components, has more similar wave field to I10-TKE-SSO than to I10-TKE-HS. However, the outputted SGS-TKE from the schemes is more similar between the I10-TKE and I10-TKE-HS[Neznámý a1]. The outputted TKE suggests that HS component of the scheme is more influential, but that is not consistent with the wave and wind field. Then, in Fig. 9 a) you can see that vertical velocity variance of the 0.5 km TE resolution simulation is closest to the LES simulation, but that's not the case for the momentum flux in Fig 10 a), where those two simulations are the most dissimilar. Also, in the text on the line 287, you say that 0.5 km simulation converges to the LES one, which is not apparent from Fig. 10. More technical details regarding the schemes could help in explaining those differences, as they are not clearly explained now.

   Response:
   We agree with the reviewer's comment. Additional theoretical and technical details details of both the 2TE and TKE turbulence schemes, including key equations and explanations, have been added in the new Appendix A. The new material highlights the differences between the schemes and provides the necessary context for understanding the behavior shown in Figs. 8-10. During the process of revising the manuscript, we identified an error in the original configuration of the simulations without the SSO source term (I10-TKE-HS and I10-TKE-none). In the previous setup, the entire SSO scheme had been inadvertently deactivated, rather than only the additional SSO source term in the TKE equation. Consequently, the differences in the resolved wave field in the earlier version of Fig. 8 resulted from the absence of the SSO blocking component, not from the modified TKE source terms. In the corrected setup used in the revised manuscript, only the TKE source terms (horizontal shear and SSO contributions) are switched on or off, while the SSO blocking scheme remains active in all simulations. With this corrected configuration, the various TKE-scheme settings influence primarily the TKE field, whereas the resolved flow remains nearly identical across all 1 km simulations (both 2TE and TKE). We have updated the discussion of Figs. 8–10 accordingly. The revised text now also explains the differences between the I05-2TE simulation and the LES, noting in particular why the LES cannot always be assumed to be the most realistic reference for all diagnostics.

Minor Comments

1. On line 169 you define overbars as horizontal mean, is it taken over the whole domain area?

   Response:
   No, the horizontal mean is taken over the area indicated by the purple box in Figure 1. This is mentioned in section 2.4. We have clarified the captions in Figure 9 and 10, accordingly.

2. Could you rephrase the sentence starting on the line 197? The meaning is not quite clear.

   Response:
   The sentence has been reformulated.

3. In the section 3.2 (line 215) you refer to Fig. 6 and after that you refer to Fig. 5 (line 228). Also, a paragraph starting on the line 235 refers to Fig. 4 and includes lot of information that was already said in a paragraph starting on line 201. This makes the text hard to follow in this section.

   Response:
   We thank the reviewer for pointing out the unclear figure ordering and resulting redundancies. In the revised manuscript, the order of Figs. 5 and 6 has been adjusted to ensure a more natural progression of the discussion. Sections 3.1 and 3.2 have been revised for improved clarity and flow. In particular, the discussion of Fig. 4 has been streamlined to avoid repetition. Section 3.1 focuses exclusively on the reference simulation (I10-2TE), while Section 3.2 discusses the sensitivity to horizontal resolution using the other simulations.

Technical Comments

1. In the title of subsection 3.4.1 you introduce new acronym – SGS-TKE – which is not explained previously.

   Response:
   We have revised the whole manuscript to consistently use TKE. The focus of the analysis is the parameterizations schemes and the TKE simulated by those (which is be definition sub-grid scale). Thus there is no need to distinguish between resolved-scale and subgrid-scale TKE.

2. Figure 9 has incorrect description – subfigures c) and d) are not referred to and a), b) are referred to erroneously.

   Response:
   The figure captions have been corrected.

3. Choice of colours for the lines in all figures in Fig. 4, Fig. 9 and Fig. 10 makes them very hard to read and to differentiate between the simulations.

   Response:
   All figures have been thoroughly revised for clarity and the color schemes have been adapted.

References

1. Smith, R. B., and Coauthors, 2016: Stratospheric Gravity Wave Fluxes and Scales during DEEP-WAVE. J. Atmos. Sci., 73, 2851–2869, https://doi.org/10.1175/JAS-D-15-0324.1.

2. Fritts, David & Smith, Ronald & Taylor, Michael & Doyle, James & Eckermann, Stephen & Dörnbrack, Andreas & Rapp, Markus & Williams, Bifford & Pautet, P.-Dominique & Bossert, Katrina & Criddle, Neal & Reynolds, Carolyn & Reinecke, P. & Uddstrom, Michael & Revell, Michael & Turner, Richard & Kaifler, Bernd & Wagner, Johannes & Mixa, Tyler & Ma, Jun. (2015). The Deep Propagating Gravity Wave Experiment (DEEPWAVE): An Airborne and Ground-Based Exploration of Gravity Wave Propagation and Effects from Their Sources throughout the Lower and Middle Atmosphere. Bulletin of the American Meteorological Society. 97. 150709110621006. 10.1175/BAMS-D-14-00269.1.

3. Lachnitt, H.-C.; Hoor, P.; Kunkel, D.; Bramberger, M.; Dörnbrack, A.; Müller, S.; Reutter, P.; Giez, A.; Kaluza, T.; Rapp, M. Gravity-Wave-Induced Cross-Isentropic Mixing: A DEEPWAVE Case Study. Atmos. Chem. Phys. 2023, 23 (1), 355–373. https://doi.org/10.5194/acp-23-355-2023.

REVIEWER#2

General Comment:

This research study focuses on simulating mountain waves within the ICON model. The focus is on mountain-wave dynamics and turbulence in the numerical model and its dependence on turbulence paramterization and resolution. The operational turbulent kinetic energy scheme (TKE) and the new two-energy turbulence scheme (2TE) are applied in this study. For the first scheme, the impact of the individual components (horizontal shear (HO) and subgrid-scale orography (SSO)) are investigated. The outcomes are then compared against aircraft observations from the DEEPWAVE field campaign, as well as high-resolution ICON-LES model results.

Overall, the paper is clearly presented and well-supported by literature. The topic is highly relevant given the growing use of fine-scale atmospheric simulations also in the upper troposphere and lower stratosphere region. However, I have one main point - which is the lack of information and investigation considering the 2TE scheme - which is missing in the manuscript. Consequently, I suggest major revisions to address it.

Response:

We sincerely appreciate and acknowledge the valuable suggestions of the reviewer. In the revised manuscript, we have addressed all the points / queries raised by the reviewer. Below, we present the point-to-point response to the comments raised by reviewer:

Major Comments

1. I am missing a discussion on why is the 2TE scheme is more appropriate. What physical aspects are responsible for this behaviour? You switched on and off the HS and SSO in the TKE scheme, can you do a similar analysis with the 2TE scheme?

   Response:
   We address this point by adding a detailed description of both turbulence schemes in the new Appendix A, including the expected advantages of the 2TE scheme. These potential benefits are largely theoretical at this stage and will require comprehensive evaluation across a wider range of cases in future work. For the present study, we were encouraged to see that the 2TE scheme performs comparably to the well-tuned operational TKE scheme in terms of the resolved flow. At the same time, our analysis highlights specific shortcomings of the TKE scheme in its operational configuration, particularly the use of additional empirical source terms that lead to unrealistic TKE production at kilometer-scale resolution. As demonstrated in the manuscript, these extra terms are not well justified at such resolutions.

2. Further, no equation is offered here for the 2TE scheme (which should be included, at least in the Appendix). It is required to make some statement about what is it, which is leading to the better performance of the 2TE scheme.

   Response:
   As noted above, we now provide the key governing equations and a concise theoretical description of the 2TE scheme in Appendix A. We would like to emphasize that the goal of this study is not to demonstrate that the 2TE scheme performs better than the operational TKE scheme (without its additional empirical source terms). A rigorous performance comparison would require a comprehensive statistical evaluation across many cases with suitable turbulence observations, which lies

beyond the scope of the present work. Nevertheless, we believe that the focused analysis of this well-documented DEEPWAVE case provides valuable insight into how the two schemes behave at kilometer-scale resolution that are of interest to the community.

3. Concluding, after reading the paper I know why the TKE scheme fails going to fine resolution but I don't know what exactly is the difference in the parameterization of the 2TE scheme resulting in a better performance. This would be very interesting and might also be the main part to see what is missing in some models or how can they be improved.

   Response:
   In order to elucidate the differences in the simulations using the two schemes, we have included theoretical details about the two schemes in Appendix A. This gives strength to our conclusion that for kilometer-scale resolutions, model setups should consider disabling or tuning the horizontal shear and SSO terms in the TKE scheme to avoid spurious TKE production. This makes schemes like the 2TE turbulence scheme helpful as it avoids such additional terms in the analysis.

Minor Comments

1. Think about referring to Fig. 1 earlier in chapter 2.1.

   Response:
   Done.

2. Line 215: At 2 km - where? The plot shows the vertical wind w - it is not clear why now a horizontal wave length is discussed.

   Response:
   The text passage has been clarified.

3. Think about adding Two Thumb Range and Mount Cook as names on the x-axis. It would help the reader.

   Response:
   Done.

4. Fig. 7 and corresponding discussion: It is not clear if it is the resolved TKE, the SGS TKE or the sum of both? → SGS TKE comes in Chapter 3.4.1, but the first time mentioned here it is not clear.

   Response:
   As we do not discuss resolved TKE, but only the parameterized TKE from the two turbulence schemes, we now use the term TKE throughout the manuscript to denote the parameterized (subgrid-scale) TKE. For clarity, we have removed all references to SGS TKE.

5. Fig. 8: Think about, maybe difference plots between the corresponding simulations in Fig 8 and the reference one are a way to be more quantitative? - It would help chapter 3.3.2 and might be a way to include percental differences or something more quantitative in the text.

   Response:
   We prefer to keep Fig. 8 in its current form. As noted in our response to Reviewer 1, we identified and corrected an error in the original configuration of the simulations without the SSO source term (I10-TKE-HS and I10-TKE-none). With the corrected setup, the differences between the configurations are now much clearer and in fact minimal for the resolved flow. Including additional difference plots would add complexity without providing substantial additional insight. A more

quantitative comparison of the area-averaged TKE is already presented in Fig. 9, which complements the qualitative assessment shown in Fig. 8.

6. Fig. 9: did the difference between I05-2TE and I01-LES-S matter in between 7-10km? Please try to be more quantitative instead of stating ”in good agreement”.

   Response:
   The sentence has been revised to improve clarity.

7. Line 287: In the 2TE simulations, the 0.5 km run converges toward the LES, while the 2 km run shows reduced fluxes, especially in the lower troposphere. I am confused, is this sentence correct? Does it state that the dark blue curve converges towards the gray one? Aren't the differences in the lowest 4km important and should be interpreted and mentioned?

   Response:
   Thank you for pointing out this inconsistency. The original sentence was not correct. While the resolved vertical-velocity variance dows converge toward the LES with increasing resolution (see Figure 9), this behavior does not carry over to the momentum flux. The 0.5 km 2TE simulation does not converge toward the LES flux profile in the lower troposphere, and the differences in the lowest 4 km are indeed substantial. We have revised the discussion of Figs. 9 and 10 to clarify this point and to highlight the differences between the I05-2TE simulation and the LES.

8. 292: Overall, the results highlight that area-averaged momentum flux converges more rapidly with resolution than local turbulence diagnostics, making momentum flux a more robust diagnostic for evaluating mountain-wave simulations. Do you mean at lower heights? This is not clearly enough written for the reader to understand what you wanted to say and should be formulated in more detail and in a more quantitative way.

   Response:
   What we intended to convey is that area-averaged quantities, such as the area-averaged momentum flux provides, generally exhibit a more robust and monotonic behavior with increasing resolution than local quantities such as vertical velocity or local TKE fields. Local fields are strongly influenced by phase shifts, small-scale variability, and non-stationarity, making convergence assessments more difficult and less meaningful. In contrast, spatial averaging reduces this sensitivity and therefore provides a clearer indication of resolution effects. Our revised discussion of Fig. 10 now explains this more explicitly and avoids the ambiguity in the original formulation.

Technical Comments

1. 190: Further

   Response:
   The requested change has been made. “Farther” has been replaced with “Further”.

2. Fig 3: Think about making the dashed lines thicker. Especially the red ones overlying with the isentropes.

   Response:
   The dashed lines, including the red ones overlapping the isentropes, have been thickened. Similar updates have been applied to Figures 6–8 for consistency.

3. observations not in the legend in Fig. 4

   Response:

We have added the legend label for observations in Figure 4. Kindly note that we have also changed the line styles for the same figure.

4. reference to Fig. 6 before reference to Fig. 5

   Response:
   Done.

5. Fig. 5: labels and numbers are small especially in comparison to the other plots

   Response:
   The font size of the labels and numbers have been increased for better readability.

6. 222: 100 km got some issue with a $\sim$

   Response:
   The issue with "$\sim$100 km" in line 222 has been corrected.

7. 250: Figs. 7a, c $\rightarrow$ spacings are missed, also Fig. A2 in the same line

   Response:
   The missing spacings have been corrected; the same line in Fig. A2 has also been updated.

8. 251 Figs. 7b, d

   Response:
   The missing spacings have been corrected.