

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
August 10, 2026

Authors' response to reviewers (second revision)

We thank both reviewers for their continued careful evaluation of the manuscript. We are grateful to Reviewer #2 for recommending the paper for publication, and to Reviewer #1 for comments that led us to sharpen the central message of the study. In revising the manuscript we recognised that our previous framing was misleading: we do *not* claim that the two-energy (2TE) scheme is intrinsically superior to the operational TKE scheme. Our claim is more specific: the empirical horizontal-shear (HS) source term of the operational TKE-scheme configuration produces spurious TKE at km-scale resolution, as shown by the controlled source-term experiments and by the scaling of the term. We have reframed the abstract, Sect. 3.3.2, and the conclusions accordingly, and have strengthened the treatment of the non-stationarity of the case and of the observation–simulation comparison.

Comments by the reviewers are in black, followed by our replies in blue. References are to the revised manuscript. Note that a new figure of the horizontal wind (Fig. 5) has been added; this shifts the numbers of all subsequent figures by one relative to the previous version (the source-term experiments are now Fig. 9, the variance profiles Fig. 10, and the momentum-flux profiles Fig. 11).

REVIEWER #1

General comment

Although numerous changes have been made to the manuscript that helped the readability and altogether clarity, which I thank the authors for, nevertheless there are still several significant inconsistencies that became even more obvious after the revision. Some of the answers were not satisfactory. For this reason, I suggest more major revisions before accepting this manuscript.

Response: We thank the reviewer for pressing these points; addressing them has improved the paper. As noted above, we have removed any language suggesting a general superiority of one scheme over the other and now state, and support with controlled experiments, our actual claim. The point-by-point replies below identify the corresponding changes in the revised manuscript.

Major comments

1. *Even when using three different hours, the results are not robust. The simulation still covers only one flight ... the wave field is mostly stationary, however the parameterized TKE from the TE parameterization changes more significantly. How are any following comparisons reliable then? ... this raises concerns regarding the significance of this manuscript.*

Response: We have clarified the scope and strengthened the temporal analysis. The study is a

process-oriented case study built on *controlled* sensitivity experiments, in resolution and in the individual TKE source terms. Its aim is to establish the resolution at which the wave field and the area-averaged momentum flux approximately converge, and to diagnose the behaviour of the turbulence parameterizations, not to provide a statistical evaluation over many cases (Introduction; Sect. 3.1). Single, well-observed cases are standard for this type of study (e.g. Bramberger et al., 2020).

We no longer describe the field as stationary. The primary wave in vertical velocity over Mount Cook is quasi-stationary in the model, but the ambient flow is not: in the reference simulation the wind direction on the lower leg changes by about 10° per hour across the three output times (Sect. 3.1). Because only a single observation is available on each leg, we compare it with the simulation at the three output times and find the closest agreement at the output time nearest the actual crossing time (Sect. 3.1; new Fig. 5). This shows that the flow is non-stationary and that the timing of the comparison matters; it is also consistent with genuine non-stationarity of the observed field, which, with the two legs sampled about an hour apart, cannot be assumed to represent the same instantaneous state. The variability of the parameterized TKE that the reviewer noted is genuine, confined to lower levels, and has little effect on the wave maxima near 6 km or the flight-leg altitudes (Sect. 3.1). The two central findings (the resolution-convergence requirements and the HS-term artifact) are independent of the particular meteorology: the artifact is demonstrated by switching the source terms on and off within a single simulation (Fig. 9) and is explained by the scaling of the term (Appendix A).

2. *There are strong differences in the upper leg ... You could provide other variables from both observations and simulations such as u and v ... to distinguish if the different wave characteristics in the upper leg are connected more to different winds or to the turbulence parameterization. If the simulations do not agree with the observations, there should be more explanation as to why.*

Response: We have added the requested comparison of the horizontal wind. The new Fig. 5 shows wind speed and direction along both legs, for the observations and the reference simulation at three times (Sect. 3.1). Three points follow.

First, the upper-level wind-speed bias (up to about 10 m s^{-1}) is common to all resolutions and to both turbulence schemes, and the wind direction is comparable across the simulations. The upper-leg differences therefore reflect the represented flow and the resolved wave field, not the turbulence parameterization, consistent with the nearly identical resolved variances of the two schemes (Sect. 3.4; Fig. 10).

Second, part of the observed wind fluctuation is the wave itself (wind-speed perturbations of about 5 m s^{-1} , wind-direction perturbations of $10\text{--}15^\circ$ on the upper leg). Because the field is non-stationary and the two legs are sampled about one hour apart, they cannot be assumed to sample the same instantaneous wave field; the inference that the amplitude grows aloft implicitly assumes such vertical continuity between the legs (Sect. 3.1).

Third, the underestimation of the simulated amplitude aloft is most likely a matter of wave propagation, not turbulent dissipation: the model's Scorer profile implies partial trapping of the short ($\sim 8 \text{ km}$) primary wave, whereas the observed wave keeps a nearly constant wavelength and a strong signal on the upper leg (Sect. 3.2). This propagation difference may in part reflect a bias in the represented upstream wind and stability profile, which sets the Scorer parameter; in either case it points to the represented flow rather than the turbulence scheme. We also note that the orography is three-dimensional and multi-scale (Introduction; Sect. 3.2), so exact point-by-point agreement is not expected, in particular downstream, where waves from several ridges interfere.

3. Throughout the manuscript you state that the TE scheme is better, with more realistic outcomes. I don't understand how you show that ... From Fig. 9 it seems there is close to zero TKE produced by the TE scheme at higher altitudes ... It would be useful to calculate the gravity-wave drag from the momentum flux to diagnose whether the heightened TKE in the TKE scheme agrees with the deposition of the drag.

Response: We have reframed this argument and no longer claim that the 2TE scheme is more accurate at representing turbulence. The controlled experiment of Fig. 9 switches the individual source terms on and off *within the same scheme* and shows that the excess TKE appears only when the HS term is active, in patches that do not coincide with regions of strong wave activity. We have added the explicit form and scaling of the HS source term to Appendix A, and a mechanism paragraph in Sect. 3.3.2: the term is a two-dimensional Smagorinsky-type closure that assumes deformation near the grid scale reflects a net downscale turbulent cascade; at km-scale the grid-scale deformation is instead dominated by resolved, largely reversible gravity-wave motion, which the closure cannot distinguish from an irreversible cascade and therefore converts into TKE.

The reviewer is right that some TKE should be present in the UTLS. We now distinguish two opposite deficiencies (Sect. 3.3.2): the HS term *over-produces* TKE where the observations show no turbulence, whereas genuine small-scale turbulence, for example downstream of the Two Thumb Range and at the sub-km scales documented by Lachnitt et al. (2023), is *under-resolved* and missed by all configurations, including the LES. The appropriate conclusion is therefore not that low TKE is realistic everywhere, but that the HS term adds TKE in the wrong places while genuine turbulence is under-resolved.

On the suggestion to diagnose the gravity-wave drag: the area-averaged momentum flux, and hence its vertical divergence, is essentially insensitive to the HS term (Sect. 3.4.2; Fig. 11), even though the HS term strongly inflates the TKE. The additional TKE is thus not accompanied by any momentum deposition, which we take as further evidence that it is spurious. This argument has been added to the discussion of Fig. 11.

Minor comments

1. In the appendix, could you add a definition of what the subscripts 1 and 2 mean in the equations A2–A4?

Response: Done. We now state, below Eq. (A2), that the subscripts 1 and 2 denote the two horizontal wind components (U_1, U_2).

2. If the TKE values from the TKE scheme are overestimated, you could use equation A1 to calculate the TKE from the LES ... it could help validating the TKE from the other simulations near the surface.

Response: We agree that, near the surface, an estimate of the resolved-plus-subgrid TKE from the LES could serve as a reference. A like-for-like estimate would, however, require reprocessing the LES output with a careful treatment of the resolved/subgrid split, and even near the surface the LES TKE is only partially resolved. We consider this beyond the scope of the present study and did not pursue it here.

REVIEWER #2

General comment

The paper has been revised and is now ready for publication. The inclusion of A1 and A2 is very helpful in understanding the causes of the results. I have noticed only two minor points in A2. The subscripts k and s in A3 and A4 are not explicitly named.

Response: We thank the reviewer for the positive assessment and for recommending the manuscript for publication. We have addressed the remaining points as detailed below.

Minor points

1. *Two minor points in Eq. (A2).*

Response: We have corrected Eq. (A2): the first subgrid-scale-orography term now reads U_1 (a missing subscript has been fixed), and the temporary colour highlighting of the additional source terms has been removed. In addition, the subscripts 1 and 2 are now defined below the equation as the two horizontal wind components (see also Reviewer #1, minor comment 1).

2. *The subscripts k and s in A3 and A4 are not explicitly named.*

Response: We now name these subscripts where the 2TE equations are introduced: e_k is the turbulent kinetic energy (TKE) and e_s the total turbulence energy (TTE), with τ_k , τ_s and K_{e_k} , K_{e_s} the corresponding dissipation time scales and diffusion coefficients (below Eq. A4).