

Interactive
Comment

Interactive comment on “Long-range transport of terrain-induced turbulence from high-resolution numerical simulations” by M. Katurji et al.

Anonymous Referee #1

Received and published: 13 July 2011

General Remarks

While the authors state that "The simulations carried out in this work are 2-D turbulence resolving simulations. 2-D and 3-D turbulence resolving simulations are very different in nature (physically and numerically)," however, in my opinion, they do not attempt to comment on and assess the limitations of their 2-D simulation study. Limitations of two-dimensional numerical studies and their applicability to real atmospheric flows are clearly indicated in the abstract of Doyle and Durran, 2007, (JAS 64, p. 4202-4221), paper cited in the manuscript: "The strength and evolution of the subrotors and the internal structure of the main large-scale rotor are substantially different in 2D and 3D simulations. In 2D, the subrotors are less intense and are ultimately entrained into the larger-scale rotor circulation, where they dissipate and contribute their vorticity toward

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the maintenance of the main rotor. In 3D, even for flow over a uniform infinitely long barrier, the subrotors are more intense, and primarily are simply swept downstream past the main rotor along the interface between that rotor and the surrounding lee wave. The average vorticity within the interior of the main rotor is much weaker and the flow is more chaotic."

Specific Comments

Page 9805 - Lines 20 to 26 - The explanation given for choosing one-minute averages does not make sense. In order to obtain statistically good estimate of the average the averaging period should be several eddy turnover times. The authors state that spectral analysis indicated that eddy turnover time is between 7 and 11 minutes (which is reasonable) then averaging time should be at least 30 minutes. For example, in Figures 4, 5, 6, 7. 8, 9, and 12 30-min averages are presented, so it is not clear why the authors did not compute use these averages to compute TKE.

Page 9805 - Lines 26 to 28 continuing on Page 9806 - The estimate of subgrid TKE is given for the whole domain and as such could be considered meaningless. Subgrid TKE is dominant near the surface or near any strong gradients (eg. inversion) and subgrid TKE should be always included in the TKE estimate.

Page 9808 - Line 16 - The authors present most of the results in terms of comparing base simulations to an ensemble of simulations, however, no explanation is given about the meaning of an ensemble over different slopes. In my opinion the analysis should be focused on the effect of the slope - variation and scaling of the results with the steepness of the slope. Ensemble averaging prevents such analysis.

Technical Comments

Page 9800 - Line 23 - Instead of "arenot" it should be "are not"

Page 9801 - Line 26 - Instead of "resolutionsimulationcan" it should be "resolution simulation can"

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Page 9807 - Line 4 - The sentence "It is worth noting that the formation of the above 5 mentioned rotors do not represent a steady and periodic one." is not clear, it is not clear what is "steady and periodic one."

Page 9810 - Line 9 - It should be "2 H" instead of "20 H".

Page 9810 - Line 24 - Instead of "whichonly" it should be "which only."

Page 9820 - Figure 3c - It is not clear what is the square in the figure representing.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 9797, 2011.

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