Atmospheric
Chemistry
and Physics

Open Access

Discussions

EGU

ACPD

Interactive
comment

Interactive comment on "**A new downscaling method for sub-grid turbulence modeling**" *by* **L. Rottner et al.**

Anonymous Referee #2

Received and published: 25 March 2016

In the manuscript "A new downscaling method for sub-grid turbulence modeling" present a proposal for a subgrid turbulence model based on a Lagrangian particle model. In this study the Lagrangian particle model is forced by resolved fields obtained from a large-eddy simulation (LES) with Meso-NH model. The focus is on analysis of small scale turbulence unresolved by Meso-NH model. Considering that the proposed subgrid model does not provide a feedback to the Meso-NH model this study can be considered as a-priori analysis of subgrid model.

General Remarks While Lagrangian particle models have been extensively used to simulate turbulent dispersion their application to turbulent flow modeling has been mainly in the area of combustion. The analysis presented in the manuscript can be considered as a priori analysis of subgrid turbulence without feedback to the resolved filed. The outline of the approach to turbulence modeling using new Lagrangian particle

model is lacking:

- the motivation for using Lagrangian particle model is not clearly stated,

- the focus is on assessment is on the subgrid turbulent kinetic energy (and spectra), however, for a model to be a viable turbulence model it needs to represent (subgrid) turbulent stresses and fluxes, but these were not evaluated,

- the analysis is limited to a relatively small part of the Meso-NH computational domain (2x2x2 coars grid cells, or 8x8x4 grid cells) and a short time period (15 minutes) and therefore the results may not be robust,

- the limitations of Lagrangian particle models have not been clearly stated and computational cost compared to more common turbulence closures has not been addresses,

- fundamental question that is not addressed in the manuscript is what scales of turbulence is Lagrangian particle model representing, this essential question needs to be addressed in the manuscript, and

- the conclusions that Lagrangian particle model follows Kolmogorov law is not supported by the results (i.e., the spectrum does not follow -5/3 scaling).

Considering that there may not be any previous examples of use of Lagrangian particle model as a closure for subgrid turbulence in LES (none are cited in the manuscript) the authors should have provided better background and motivation for their approach. Also, the Lagrangian particle model output is compared to higher-resolution LES, however, this is likely not a fair comparison considering that spatial and temporal resolution with Lagrangian particle model may be significantly higher than high-resolution LES. The authors do not address the issue of scales. Furthermore, the manuscript is not organized well: as indicated earlier, the motivation is not clearly stated, the model development is not concisely outlined, e.g, use of Meso-NH simulations to force the model is discussed before the model is presented, and the analysis does not follow logical order, e.g. turbulence spectra are analyzed before TKE is analyzed. Finally, the

manuscript in many instances does not follow proper English idiom as indicated below under "Specific Remarks." Taking all the above into account I do not recommend the manuscript for publication in the Journal Atmospheric Chemistry and Physics in the present form. The manuscript could be considered for publication after questions and suggestions for major revisions are addressed.

Specific Remarks

- Line 146, instead of "fine grid-point simulation" better would be "high-resolution simulation."

- Line 147 "The experience holds in three. . ." it should be "The process consists of three steps. . ."

- Line 151, same as above.

- Line 243, since the eddy dissipation rate is used in the model it should be stated how is it computed in Meso-NH.

- Line 254, instead of "close results" it should be "similar results."

- Line 258, instead of "experience" it should be "case," "study," or "experiment."

- Line 263, instead of "particle models" the terminology should be more precise: "Lagrangian particle models."

- Line 275, since the reader cannot know about authors' experience instead of "our experience" it should be "our study" or "our simulations."

- Line 323, number of particles used (75 per grid cell) is quite large, increasing grid resolution for the same number of grid cells per coarse grid cell would result in almost an order of magnitude higher resolution.

- Line 360, since dissipation of TKE (or more precisely energy transfer) is one of the critical roles of a subgrid model it would be important to describe how is energy dissi-

pation rate modeled.

- Line 388, instead of a reference to Shannon (1949) original work by Nyquist should be referenced (This is actually Nyquist frequency, c.f. Certain factors affecting telegraph speed (1924) and Certain topics in Telegraph Transmission Theory (1928)).

- Line 405, "the aim of this work" should have been stated in the introduction.

- Line 427, the rationale for different treatment of horizontal and vertical velocities should be provided.

- Line 451, instead of "... we remind first how the TKE is computed..." better would be "...we first review how TKE is computed..."

- Figure 5, it is not clear what is the purpose and value of the comparison shown in this figure.

- Lines 509-514, the statements are based solely on a limited qualitative analysis (comparison of plots) and as such they are of little value.

- Line 523, the statement "more turbulent" is qualitative, that needs to be qualified. The question is what should be the level of turbulence at the scales that are resolved. Another question is what scales are particles representing?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-1015, 2016.