**Review of the** *Geoscientific Model Development* **manuscript**

**Formulation of and numerical studies with the Dutch Atmospheric Large-Eddy Simulation (DALES)**

by

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**Recommendation:** needs considerable revisions

The reviewed paper is aimed at a comprehensive description of the Dutch Atmospheric Large-Eddy Simulation (DALES) code. Writing such a paper is not an easy task because it implies finding a subtle balance between making the description of the code as informative as possible, but at the same time keeping the manuscript concise enough to meet standards of a scientific journal article. Generally, the authors were up to this task, but in the process of bringing together individual contributions by co-authors, the technical integrity and clarity were somehow lost. The resulting manuscript contains numerous descriptive and technical flaws that severely complicate digestion of the material by the reader and leave many features and capabilities of the code rather unclear.

I marked up the manuscript while reading and provided an annotated copy to Thijs Heus, who will now have an opportunity to involve his co-authors in the joint work on revisions. I should emphasize that the required corrections and modifications are mostly technical in nature, but the extent of these technical revisions is such that they will need a considerable joint effort by the authors.

Below I summarize my suggestions on the improvement of the manuscript by referring to its individual sections and focusing only on the major presentational issues and points of confusion. Minor corrections could be implemented by following the annotated copy that was given to Tijs.

*Title*

I would suggest rewriting it as **Formulation of the Dutch atmospheric large eddy simulation code DALES and overview of its applications**.

*Abstract*

Using words “simulation” and “model” together is confusing and redundant at the same time, depending on the context. It would be nice if the authors find a way to distinguish between modeling and simulation throughout the paper.

*Introduction (section 1)*

“Cascade of tools” would require a definition. Why not “spectrum” or “set”?

Generally speaking, direct numerical simulation (DNS) is also an atmospheric modeling/simulation tool (even more “detailed” than LES).

“Over coarser model”. Which models are meant here? Could a model be “coarser” or “finer”, or we speak about resolution here?

Too many undefined acronyms (check other sections of the text for the acronyms as well).

As far as I know, it is inappropriate to start phrases and sentences with acronyms or mathematical symbols (such cases should also be checked throughout the entire text).

Primes that denote the subfilter-scale fluctuations are not used consistently throughout the paper. They appear in the text for the first time much later that they are announced. Also, in terms of definitions, the difference between, e.g., $\widetilde{u_1 u_3} - \tilde{u}_1 \tilde{u}_3$ and $\widetilde{u_1' u_3'}$, is not addressed.

References to the papers presenting examples of DALES code performance evaluated through other models or reference datasets should be given.

*Section 2*

Procedure of deriving the Poisson equation from the original momentum and mass balance equations should be briefly explained.

Notation should be cleaned up in this section and elsewhere within the text (to follow the authors promise that even for a non-unique symbols it will always be clear which physical quantity it represents in each particular case).

The second portion of the description of the SFS-TKE model is confusing. The reduced production-dissipation equilibrium in the TKE balance contradicts the original SFS-TKE balance equation (14). Spectral considerations involved appear to be irrelevant (and probably erroneous). The adopted SFS-TKE model is basically the same model that was originally proposed in the study of Deardorff (1980), which is not mentioned in the Introduction, by the way). Why not to follow the model description from the *op. cit.*?

Subgrid Prandtl number is first defined as filter-size dependent and then declared to be constant.

Usage of the Boussinesq approximation should be clarified and reference-state parameter values should be distinguished from the constant temperature and density values that appear in denominators of the buoyancy acceleration and TKE production terms.

Description of boundary conditions is confusing. Some of them are presented in the local formulation and some – in the unclear (sort of ensemble-filtered) formulation (compare Eqs. 32 and 37). It should be clearly stated how the Monin-Obukhov theory is applied for the formulation of surface boundary conditions and how velocity and temperature/humidity fields at the surface are related through these conditions.

Smagorinsky's SFS model is not sufficiently described. It is much more that just "some algebra" in its derivation. See Wyngaard (*JAS*, 2004; *Terra Incognita* paper) for details of the derivation and interpretation of the underlying assumptions).

Description of four options for the surface flux calculation should be completely revised. In the present form, it leaves more questions than gives answers. Particularly, the implementation of the shear-free option (with friction velocity equal zero) is confusing. How are gradients and fluxes of scalar related in this case? Also, even in the shear-fee convection regime, there is always non-zero local horizontal wind and plane-average friction velocity would be never exactly equal to zero.

Numerical scheme subsection does not contain a description of the pressure calculation method (it is not clear in this connection how the filtered continuity equation is employed numerically) and a formulation of boundary conditions used in the pressure calculation.

In the description of parameterization of slope effects, some expressions are wrong (e.g., Eq. 101) and some contain confusing parameters (like reference-state potential temperature and the constant parameter potential temperature denoted in the same way in Eqs. 98 and 98). Whether the subfilter closure is modified for the slope-flow applications (as it should be) is not clear.

Definition and physical meaning of the dissipation term in Eq. 107 should be checked and corrected. How is this term evaluated from the LES output?

*Section 3*

Shear does not increase “boundary layer height” (after all, a layer cannot have height, but can have depth, for instance), it rather increases boundary layer growth rate (but not in all cases, as shown in Conzemius and Fedorovich, *JAS*, 2006).

Discussion of the turbulence energy transformations in a stable layer should be either presented consistently, in terms of balances of turbulence kinetic energy and turbulence potential energy, or omitted. In the plots, some DALES data ate shown with resolution 3.125 m, which contradicts description in the text.

In the common meteorological sense, the low-level jet (its boundary-layer version) is understood as the wind maximum close to the ground that develops as a result of interplay between inertial oscillation and reduction of turbulent mixing in the boundary layer, typically, during night. Note that the jet-like formation in the profiles from Beare (2006), which are reproduced in the reviewed paper, is not called the low-level jet in *op. cit.*, but rather super-geostrophic jet, apparently because its nature is not the same as that of the conventional low-level jet.

In the case of heterogeneous underlying surface, how does application of the surface boundary conditions change?