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Interactive comment on "Adaptation of the meteorological model Meso-NH to laboratory experiments: implementations and validation" *by* Jeanne Colin et al.

Anonymous Referee #3

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The authors modify the existing large-scale model Meso-NH by inclusion of an explicit diffusion term and a no-slip boundary condition. The modified model is tested against two analytical solutions of laminar flows and experimental results for the Blasius boundary layer.

The title and abstract of this paper fit the scope of Geoscientific model development; a problem with the manuscript is, however, that it does not stand up to the promises given in the beginning due to a number of methodical problems. While the paper is well-written and the authors convey their messages clearly, the work is insufficiently embedded into the ongoing scientific discussion on direct numerical simulation which has achieved considerable progress over the past decade or so. Key references are missing and the numerical testing is not up to the standards that should be put up for such simulation. It remains unclear which equations are actually solved by the so-called DNS and how these equations relate to the actual physical problems the reviewers claim to investigate.

In the reviewer's opinion, the paper is definitely not publishable in its present state, and I recommend to the editor to **reject the manuscript in its present form**.

1 Reasons for Rejection

There is two particular reasons for which I cannot recommend the publication of this paper. Beyond this, there are also a large number of major issues (see next section) which altogether, justify rejection of this work in the reviewer's opinion.

I) The authors do not demonstrate the convergence properties and accuracy of their numerical algorithm; the approaches presented are insufficient in a number of ways (cf. Rogallo and Moin (1984); Moin and Mahesh (1998); Coleman and Sandberg (2010)):

- First, a vague reproduction of analytical solution with errors on the order of 10^{-2} does neither serve as a demonstration of consistency nor does it prove convergence. Hence, the title and corresponding statements in introduction and conclusion about a validation of the numerical method cannot withstand a rigorous scientific assessment.

- Second, if the authors wish to establish their code as a DNS code, the code must also withstand rigorous testing of a truly three-dimensional turbulent case, and not only be validate approximately against noisy experimental data from a Blasius boundary layer. One such example is the turbulent Ekman layer (where a vast literature exists including very exact predictions for bulk turbulent properties such as the surface veering angle and wall friction as a function of the Reynolds number and the geostrophic wind; cf. Coleman et al. (1990); Deusebio et al. (2014); Mellado and Ansorge (2012)), but tests with open or closed channel flows are also possible (cf. Moser et al. (1999)). I wish to point out that these tests, which can be carried out on grids of the order of 128^3 would only require a fraction of the numerical resources used in the present work.

- In section 3.2, there is not only an error, but there seems to be a bias (the numerical solution of the model gives a consistently higher boundary layer than the analytical one). This should be explained. Further, it must be demonstrated that this bias approaches zero when the resolution in time and space is increased. Else, the model may be solving an equation different from the actual physical problem that the authors claim it solves.

II) The authors claim to establish Meso-NH as a tool for direct numerical simulation. This is not the case. The exact version of the governing equations which are solved is unclear, so I would like to pose the question DNS *of what?* The reason is that the authors lament about height-dependent outer-layer wind profiles (p. 9, l. 29) for cases where, from a physical perspective, there is clearly no reason for a height-dependency looking at the governing equations of the physical problems the authors claim to solve. There must hence be a discrepancy between the governing equations of the physical problem and the equations actually solved by the model.

2 Major Issues

M1 While I am not familiar with the details of the measurement procedures presented here, I would like to point out that such noisy measurements are insufficient to calibrate a DNS tool and much better test cases are available in the literature. In principle do agree that the comparison of DNS results to observational or laboratory data is not only very illustrative to demonstrate the benefit of the method but also has substantial methodical benefits over the comparison with analytical results for turbulent flow, for the present work, I would suggest to focus on an improved demonstration of the actual convergence properties of the adapted numerical method before resorting to comparison with experimental data. Moreover, the mere availability of a dataset from an on-site laboratory should step back behind the availability of data at higher quality; and there definitely are much better-controlled data available for a Blasius-type boundary layer.

M3 While, as mentioned above, there are fundamental issues and technical problems associated with the use such a complex model in the mode that the authors refer to as 'DNS', the present manuscript does not mention methodological, technical or computational advantages of doing so in comparison to the established approach to DNS where light-weight but highly optimized implementations of PDE solvers are used to obtain the respective solutions.

M4 On page 5 (lines 9-10), the authors state that *meteorological models do not take into account viscous dissipation*. This is not true in general – effects of viscous dissipation are implicitly taken into account by either assuming that they are small or as part of the physical parameterizations. Eventually, the turbulent mixing, which is definitely taken into account, is a consequence of viscous adhesion.

M5 The explanation of the concept of Reynolds-number independence (p. 5, l.10-20) must be motivated and introduced better.

- *at the end of the spectrum* – which end do you refer to?
- *dissipation of energy [. . .]is then ensured by a numerical diffusion scheme* – this is in general not true. Numerical diffusion is an artifact of the numerical scheme chosen and can be minimized by choice of a proper numerical

scheme. Turbulence diffusion is an explicit representation of the turbulent mixing as a function of resolved variables. There is only a small class of LES which use the numerical diffusion as part of the turbulent mixing.

M6 The order-of-magnitude estimates on p. 6, l3-14 are inconsistent in accuracy and thus cannot be reproduced (I obtain $\eta = 0.24$ from your values and using single-digit accuracy, this corresponds to $\eta = 0.2$ instead of $\eta = 0.1$; to resort to . Further, I consider this textbook-knowledge which I think does not need to be reproduced such broadly in a scientific paper.

M7 *From this validation, we conclude that our implementation of the viscous term is correct.* This is not possible. First, you must check for convergence and accuracy (and not consistence only) to claim this. Second, a check with a flow in the other direction is also needed to claim this.

3 Further comments

p.8,l.25 it is not possible to describe a whole boundary layer analytically; for certain aspects of it, this claim may be hold up.

p.10,l3 *Above, the resolution ranges from 2 m to 10 m and it is equal to 50m in the upper levels.* Does that mean, there is a factor-five jump in vertical resolution?

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