Review of "*Adaptation of the Metreological model Meso-NH to laboratory experiments: Implementations and validation.* "

by J. Colin, C. Lac, V. Masson and A. Paci.

The manuscript presents an update to the well known Meso-NH model than enables it to run DNS. This is an interesting development and the details and testing deserve to published. Not only for the Meso-NH user base, but also others whom may wish to attempt a similar approach. First, the authors motivate their work by expressing their wish to explicitly resolve the turbulent motions in lab(-sized) experiments. Second, the two changes that were made to the code are presented: (1.) Including a viscous diffusion tendency term and (2.) the implementation of a Dirichlet-type bottom boundary condition for the momentum components. Third, 1D and 2D testcases are run to check the implementations of the aforementioned steps and finally, a comparison of the model results is made with the measurements obtained from a (3D turbulent) water tank experiment.

I agree with the authors that the development of atmospheric models can benefit from lab-experimental results. This concept is what attracted me to this study.

However, much of the content, arguments, and analysis presented in this manuscript would require very substantial revisions before I would consider the manuscript suitable for publication. Here is why:

Major issues:

The Motivation
What is de added value of the presented efforts compared to existing DNS codes? Is it the easy inclusion of the atmospheric physics modules that are already present in Meso-NH?

The Implementation
Eventough an entire section is devoted to it, the manuscript remains unclear on how the viscous diffusion term is actually implemented. I would like to see a more detailed description so that future readers will be able to reproduce the steps taken by the authors. (formulation with stencils etc). E.g. The fact that only one layer of ghost cells is defined suggest that the authors have opted for a second-order-accurate formulation. I would like to see this more explicit. Instead, section 2.2 reports on some general aspects of DNS and the authors' personal interpretation of that. Maybe the manuscript could do without the narrative?

The Tests
For all test cases, the setup seems rather arbitrary in terms of the chosen scales etc. A non-dimensional formulation of the problems would greatly help with the interpretation of the scales and results. I do not feel that bringing it to a "atmospheric scale" (that is apparently 4750m?) is very helpful as the tests bare no resemblance with the real atmosphere anyway.

The desired accuracy that the authors assume to be a low enough threshold is highly debatable and not motivated in the text. Often the authors even resort to statements as "Very close" to describe the comparison between the numerical results and the theoretical solutions. I find this is not very satisfactory.

Sect. 3.1.
The validation of the implementation of the diffusion-tendency term rather unconvincing. First, the test results are probably (I checked that this is true for atleast my personal favorite diffusion solver)

very sensitive to the way the boundary conditions are implemented and what they were chosen to be. This seems in contrast with the fact that this case was presented to be a test for the diffusion tendencies only. Furthermore, in order to upgrade this test to more of a validation-type analysis I would urge the authors to study the **spatial convergence properties** of their implementation for a **3D diffusion problem** (e.g. Gaussian pulse or a 3D extension of their periodic function on a triply periodic domain). Note that it is not obvious for me that a given order of spatial accuracy for a 1D problem is (naively) inherited by higher dimensional simulations.

Sect. 3.2 and 3.3.
I have similar objections to the current validation of the implementation of the new-boundary conditions with the first Stokes problem. It is clear that the analytical and numerically obtained solutions are rescaled versions of each other. Please use proper scaling (non-dimensional) for the analysis of the results. Provided that the test cases are suitable, a conclusion on the order of the spatial convergence rate is the only reasonable way to validate the implementations.

Finally, I think the quality of the work would greatly improve if the authors show that they are able to reproduce the results of the turbulent channel flow of Moser et al. (1999), these results have proven to provide excellent DNS benchmark results.

Ref: http://turbulence.ices.utexas.edu/MKM_1999.html

However, I can also understand that validation with the waterflume data could be a more attractive option. But that would require the quality of that part of the manuscript to be raised considerably.

The lab experiment part
As elegant as the Pi-Buckingham theorem may seem, it warrants careful consideration of the selection of parameters that define the flow. Here an issue arrises for me: It is not obvious to me why the gravity acceleration (g) is chosen to be part of the list of parameters that define the problem. The turbulence in the flume is neutrally stratified right? I can only imagine it has an effect on the surface water-air interracial waves. These are not resolved in the simulation, correct? Therefore I find the introduction of the Froude number confusing.

It not clear how the Reynolds number (25000) is computed (Lambda = ?). This also leaves open many question with regards to if the Kolmogorov scale is reasonably estimated.

The authors mention that the simulation is performed without any parameterization (L14p13). This is not true, the effect of the lateral walls, top wall (roof) the inflow and the outflow are (crude) parameterizations of the real experimental setup. Also the authors never address the actual implementations nor the effect of these parameterization choices.

L5-10 P14: The analysis where the iso surfaces of |U| are said to indicate isotropic turbulence and therefore prove that the flow must be accurately resolved over the whole spectrum is debatable on many levels.
- I have no clue what features of the graphs would be clear indicators of isotropy. I can only identify the presence of chaotic/erratic/irregular features by eye.
- What type of isotropy am I actually looking for? The most obvious feature I can spot is that |U| increases with height (i.e. an anisotropic feature).
- Assuming that there are hints of isotropy in the graphs, How do I know if they are indeed correct?
- Given the presence of a correct (isotropic) inertial subrange. How can I know if the smallest viscous lengthscales, that are also part of the spectrum, are resolved correctly?

A quantitative spectral analysis would be of great value here. I'd love to see the -5/3 scaling of the E(k) spectrum and **a consistent viscous range**. It is only conjecture, but at this moment I have strong doubts that the chosen grid spacing would be low enough for this purpose. I think it is required that I am proven wrong here.

L3p14, I have seen no results that indicate a fully resolved DNS. Please be more critical to the chosen benchmarks and the results. None of them prove that the inclusion of the diffusion term actually did something relevant for the flow evolution or has influenced the presented statistics. The only presented result that could be indicative of the of viscosity is the "law off the wall" analysis corresponding to Fig. 14. It worries me that there appears to be no resolved so-called viscous sublayer.
See: https://en.wikipedia.org/wiki/Law_of_the_wall
Also it would greatly help interpretation of the profiles if they were presented in wall units.

Sect. 4.3 Reports a "rigid-roof" boundary condition. I am not familiar with these type of boundaries. Please explain what it means.

Also sect 4.3 does not present enough information on how the outflow boundary condition is implemented. In general, any outflow condition is not able to accurately describe the turbulence near that model boundary. A naive Neumann condition for the velocity component and pressure fields typically destroys turbulent structures that arrive at the boundary. This makes the resolved flow close to the boundary unphysical. I would therefore propose that the authors omit the data that is obtained close to the outflow boundary. Or in any case to quantify how close 'too close' in this respect.

Did the authors do tests to check if the presented results are converged with respect to the chosen setup of domain size, grid resolution, boundary conditions?

It also remains unclear that the DNS approach has actually added something compared to the "default" LES approach of Meso-NH. At this moment I think that the presented results may as well be obtained with the adoption of an Eddy-diffusivity-type closure. Since it is pivotal for the authors' motivation to run a DNS, I'd like to be proven wrong.

Conclusion:
From my previous comments it is clear that I disagree with the statements that the presented results prove the DNS capabilities of Meso-NH for 3D turbulent flows.

L27P16, "*To our knowledge, this is the first time an atmospheric model is successfully run in DNS*." Such a statement does not do justice to the vast amount of atmospheric literature that appeared in the past decades that does employ a DNS strategy. It is even inconsistent with the introduction of this manuscript itself. In my opinion, these type of statements are not acceptable for publication.

More issues:

0. Title:
The title creates the impression that Meso-NH is adapted based on lab-experimental results. This is not the case. Rather it is extended to run in a DNS mode, motivated by a wish to explicitly resolve the lab experiments. Also, I disagree that the paper provides clear information on the implementations nor does it provide a validation (only tests).

Abstract

l3, The viscous diffusive fluxes in a LES are typically not simply neglected, rather their effect on the flow is parameterized. e.g. The dissipation of kinetic energy.

l7. The “very high resolution” claim is subjective. In the text it is approximated to be 20 times the viscous length scale, and hence, can also be considered to be coarse, as it operates at the limit (at best) of what a DNS should do.

1. Introduction

In the comparison between LES, DNS and lab experiment efforts to model the atmospheric boundary layer:

I think an important part for the motivation to use DNS in atmospheric research is missing. In fact all of the three aforementioned methods assume that it is possible to represent the enormous range of lengthscales present in a typical atmospheric turbulent flow with a much lower degree of (represented) scale separation. Either by employing closures (LES) or a reduced Reynolds Number, all method rely on the assumption that there is a presence of a large enough intertial subrange that displays a (down-scale) cascade and that the smallest scales do not directly influence the larger scales that dominate the overall dynamics.

J. Eggels et al (1994) presented very relevant work in JFM under the title: Fully developed turbulent pipe flow: A comparison between direct numerical simulation and experiment

Please consider to place the work presented in this manuscript in its context.

L35 p3. Expresses interest in the stable boundary layer. The (DNS-based) works of Nieuwstadt (2005), C. Ansorge & Mellado (2014), Donda et al. (2015), Van Hooijdonk et al. (2017), may be helpful references for the reader interested in this topic:

Nieuwstadt (2005): *Direct Numerical Simulation of Stable Channel Flow at Large Stability*

Ansorge & Mellado (2014) *Global Intermittency and collapsing turbulence in the stratified planetary boundary layer.*

Donda et al. (2015) *Collapse of turbulence in stable stratified channel flow: A transient phenomenon*

Van Hooijdonk et al (2017). *Early warning signals in the stable boundary layer: A model study*

2. Imprementation

To my knowledge, Meso-NH in LES mode works with an eddy-diffusivity-type closure. Apart from evaluation the eddy-diffusivity, it seems (implementation wise) very similar to how it affects the tendency terms compared to using a fixed-viscosity in the DNS. Why have the authors chosen to implement an additional viscous diffusion term? Or did they just override the turbulence-model and plugged-in a constant diffusivity? If no, why not?

Eq 10 and 11 report how the ghost cell values for the horizontal windcomponents are defined in an identical way. Do other fields get the same treatment? Like the Pressure, or potential temperature?

I would expect that the the 5th order WENO-advection-scheme would require atleast two layers of boundary-ghost cells (also for w). Yet the authors define only one layer? Also the authors seem to have opted for a second-order accurate definition of the ghost cell values. Please note this explicitly in the text.

3. test cases

I do not see why the diffusion tendency is tested with the U, p and Theta field. The inclusion of variables for momentum, pressure and potential temperature should not do anything in this case. It gives the false impression that there are all kinds of dimensionless parameters to be identified that define the problem. Whereas for a 1D-case, an analytical expression can be derived that has no explicit dependence on any of these parameters. Please consider to present the results in terms of a normalized species concentration.

The plotted initialized solution in fig. 2 is not consistent with eq. 12

Also eq 12: Capital Pi is introduced and not explained in the text. Later is appears to be replaced with lowercase pi.

The vector **x** in eq. 12 Should maybe read **e_x** to indicate that it is an normalized vector? In any case, please explain more clearly the intended meaning of the used symbols.

Please do not use subjective terms as ‘very close’ or similar statements for these simple numerical problems. I find the results wildly inaccurate.

4. Experiment

The Reynolds number is sometimes written with subscript (R_e) and sometimes not.

The Kolmogorov lengthscale is estimated to be a single value. I expect it would not vary throughout the domain? So what does the calculated Kolmogorov scale actually represent here? (a BL-Mean? A minimum?)

The text and numbers in fig. 9 are not legible.

It seems like the 8 panels in fig 12 and 14 display the same data? It that necessary?

How was the vertical gridstreching defined exactly and on what a priori knowledge was this based on?

L30p11 Please define turbulence intensity (I) at the first usage and not over 2 pages later.

L29P13 Adding a white noise for the velocity components seems to be inconsistent with Eq 2. Are there references I can read that show that this is a good idea to represent the inflow (or outflow)?

5. Conclusions:

I agree with the last section of the conclusions (starting with the sentence citing Stiperski et al. (2017)), that the DNS capabilities may well be used for these interesting purposes. However, they are hardly conclusions from the presented work.