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Discussions
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# *Interactive comment on* "Direct numerical simulation of intermittent turbulence under stably stratified conditions" *by* P. He and S. Basu

**Anonymous Referee #1**

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In general, the paper is rather well written, addressing an interesting subject of intermittent turbulence by means of numerical simulations. The main subject of the study is well described with number of relevant citations, pointing out some of the problems associated with the notion of the term "intermittency" in turbulent flows.

My major concerns related to the present manuscript are thus rather focused on the chosen methods, obtained results and conclusions based on them. Although the general concept of using DNS methods to verify the hypothesis of generation of turbulent intermittency seems to be a good idea, there are several issues that have to be clarified in order to draw any definitive conclusion based on the obtained numerical results.

1) Description of the numerical method/solver in the paper The DNS is performed using an open-source package OpenFOAM. When it comes to the description of the numer-



ical method itself, the authors claim (on page 187) that: "The governing equations are discretized using the finite-volume method. The spatial and temporal derivatives are discretized with the second-order central scheme and implicit second-order backward scheme, respectively. The pressure equation is solved using pressure implicit with splitting of operators (PISO) algorithm." Such a description seems to be insufficient and incomplete, as there are infinitely many second order central schemes for discretization of spatial derivatives and also infinitely many second order implicit backward formulas can be constructed for temporal discretization. The detailed description of these methods is not only important for allowing other authors to repeat and verify the simulations, but are also of a key point for interpretation of numerical results that do depend on numerical method being used. This is related with next comments. 2) Choice of the numerical methods It is well known and documented that the numerical methods do affect to certain extent the results of simulations. Especially the convection dominated flows and DNS are prone to numerical artifacts caused by adopted solvers. This includes both the non-physical (numerical) oscillations in solution as well as excessive numerical diffusion introduced by the applied numerical discretization. Thus I will be very careful about justification of the choice of the specific numerical scheme, and will never base any conclusion on a simulations obtained using only a single numerical method. Especially an intriguing case like the one being solved in this paper necessarily needs to clearly distinguish and separate numerical artifacts from physical phenomena. So at least at this point I consider the presented numerical results as insufficient and unconvincing. 3) Choice of computational domain and grid As noted above, the DNS itself is very sensitive to numerical setup and prone to numerical artifacts. Thus one of the things I am missing in this specific case is a kind of sensitivity test to these factors. In the presented case all the simulations were performed on a computational domain of the same length using a periodic boundary conditions. Using this setup the simulation leads to occurrence of periodic (or better to say) recurring phenomena in the computational field, however there is no clear evidence on if (how much) these events depend on specific computational setup. Will the spatio-temporal evolution be affected by a change of size or aspect ratio of the computational domain or by the grid resolution? Without such a verification I will hesitate to accept all the observed phenomena as being really physical. 4) Grid resolution versus accuracy The authors opted for a numerical solver using a second order accurate discretization for both spatial and temporal derivatives. It's a question if this level of numerical accuracy is sufficient for reliable DNS simulations. It would be nice to have at least some references based evidence for similar cases that second order method (for given grid resolution) is enough to properly capture and resolve such fine scale an low amplitude events as those intermittent bursts studied here. The details on available DNS simulations given in Table 2, page 215, shows for example information about grid cell sizes, but fails to show what was the order of accuracy for the applied numerical method. The combination of grid resolution together with the order of accuracy is what defines the size of the error. So the grid resolution that is sufficient for one numerical method (for a given case being solved) doesn't necessarily has to be sufficient for another numerical method. So again, the independence of the presented numerical results on the chosen spatial (and temporal) cell sizes is not clearly demonstrated.

To sum up the comments, the problem is interesting, chosen approach seems to be appropriate, but the numerical results are not convincing and insufficient to support (without any doubts) the conclusions given by authors. I recommend the paper to be revised before being reconsidered for publication. I hope these comments will help the authors to improve their manuscript.

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