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Interactive comment

Interactive comment on "Quasi-hydrostatic equations for climate models and the study on linear instability" *by* Robert Nigmatulin and Xiulin Xu

Ilias Sibgatullin

sibgat@imec.msu.ru

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Dear Dr. Putike,

Actually it is a kind of simplification of GCMs, which were still used in 1980s, with altitude as the vertical coordinate.

But the 'simplification' is made in such a way that additional (uncompensated) vertical velocity appears and conservation of the full energy is violated. Scales (2.1) and the new theorem is another story.

Holton, actually Charney could be the first who expressed the idea, gave a simple

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Discussion paper

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approximate (even in frames of hydrostatic approximation) expression for the vertical velocity, which worked fairy well.

But L.Richardson in 1922 gave an ***exact*** expression for vertical velocity in frames of hydrostatic approximation. It is just a simple expression of $\frac{\partial v_z}{\partial z}$ from continuity equation, followed by substitution of $\frac{d\rho}{dt}$ by $\frac{dp}{dt}$ with the help of thermodynamic equality.

The 'simplification' made by the First Author of the Paper is the neglect of the horizontal advection of pressure in $\frac{dp}{dt}$ upon scale analyses of ***one component*** of the horizontal divergence of mass flux. But he did not take into account the very well known caveats of the scale analyses in stratified flows and experience of Margules, Richardson, Charney etc., who discussed specific problems with the calculation of the horizontal divergence in the atmosphere. Sometimes $\frac{dp}{dt}$ may be almost zero, with pressure advection and divergence of mass flux compensating each other. If one drops one them, the balance is violated.

I suppose that if the approach of the Paper would be realized numerically, the simulation would just diverge, so the comparison you've asked would not be even possible.

The Second Author of the paper made a careful linear investigation of the system given to him by the First Author up to the section 3.1. Unfortunately the system in 3.1 is not suitable for simulation of the Earth atmosphere at ***any*** scale. I would suggest the Second Author to publish a single-author paper on linear analysis as an elaboration of the Arakawa's work, it will be a pity if the careful techniques would be in wane.

My detailed comments on the model by the First Author were submitted to the journal of the original publication and are openly available:

https://pubpeer.com/publications/446D764678B603CC6EF997C8C5EF00#2

https://arxiv.org/pdf/2001.08637.pdf

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Best, Ilias Sibgatullin

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