

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 Xiulin, you did not show up the scale analyses of the horizontal divergence, I was writing about.

Let's look at it carefully, namely eq. 2.15 (in picture below this text) and put here again the scales of the horizontal divergence, that were used in it:

$$\text{div}_{hor}(\rho\vec{v}) = \frac{\partial\rho v_x}{\partial x} + \frac{\partial\rho v_y}{\partial y} \approx \hat{\rho} \frac{V_{hor}}{L_{hor}}$$

This estimation is *just wrong*, because the components of the divergence compensate each other to large extent.

Want an example?

Take a layer of incompressible fluid and push on its surface with your palm from above. The water will escape from below your palm, and you if you estimate the divergence with only one component it will have a finite value, but of course the full divergence just exactly 0, since the components of the divergence are mutually compensated.

The components of the horizontal divergence in atmosphere also compensate each other to large extent.

I understand that you are in no way responsible for this analyses, since it was present earlier in a paper by the First Author

<https://doi.org/10.1134/S0015462818040201>

I was pointing at these inconsistencies in this paper already for two years. And now I am amazed to see in again here.

So my comments (attached) were addressed not to you, but to the author of this analysis, Dr. Nigmatulin. The comments await for the answer at the public review portal

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

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

The interest in this analyses is warmed up by the fact that, due to the inconsistencies, the new theorem (new asymptotics) denies the existence of convection and internal waves, which also may have small vertical acceleration. So if such a wrong theory is accepted by a faculty of a university for teaching, it becomes not only wrong but harmful.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-146>, 2020.

$$\begin{aligned}\frac{g\dot{M}}{\gamma p} &= \frac{\dot{M}}{\gamma M} = \frac{-\int_z^H (\partial(\rho v_x)/\partial x + \partial(\rho v_y)/\partial y) dz'}{\gamma \int_z^H \rho(t, x, y, z') dz'} \sim \\ &\sim \frac{\bar{\rho} V_{\text{hor}}(H-z)/L_{\text{hor}}}{\gamma \bar{\rho}(H-z)} = O\left(\frac{V_{\text{hor}}}{L_{\text{hor}}}\right),\end{aligned}\tag{2.15}$$

Fig. 1.[Printer-friendly version](#)[Discussion paper](#)

On hydrostatic approximation by R.I. Nigmatulin
and L.F. Richardson's equation.

I.N. Sibatullin^{1,*}

¹*Shirshov Institute of Oceanology of Russian Academy of Sciences*

The theorem given in "Equations of Hydro- and Thermodynamics of the Atmosphere when Inertial Forces Are Small in Comparison with Gravity" (2018) is wrong. The scales given in the paper are not suitable for application of hydrostatic (quasistatic) approximation. The modification of Richardson's equation for vertical velocity by neglecting horizontal advection of pressure results in violation of symmetry of equations and incorrect uncompensated additional vertical velocity.

Keywords: *hydrostatic approximation, quasistatic approximation, synoptic scales, microscale meteorology, mesoscale meteorology, force of inertia*

1. INCORRECTNESS OF THE THEOREM FORMULATED IN [1], AND SCALES OF APPLICABILITY OF HYDROSTATIC (QUASISTATIC) APPROXIMATION

Traditional asymptotic analysis of hydrostatic approximation for different geophysical flows is based on the smallness of the ratio $\varepsilon = H/L$ of the vertical to the horizontal scales of motion, which is often introduced as the *hydrostatic parameter* (see, i.e., R. Zeytounian, [2], eq. 3.9, 19.2). The author of [1] proposed a different approach, which is based only on the smallness of the vertical acceleration normalized by the gravity acceleration, as the parameter of applicability of the hydrostatic approximation, and formulated it as a theorem. Smallness of the amplitude of oscillations, which corresponds to the smallness of the acceleration at a fixed frequency, usually can imply only linearization of the equations. But until now, no one could formulate a theorem on elimination of the short-wave motions and application of the hydrostatic approximation (which is a long-wave approximation according to the traditional asymptotic analysis) based only on the smallness of amplitude or the vertical acceleration.

The transition to the limit from the full Navier–Stokes equations to the equations of hydrostatic approximation when *the vertical acceleration approaches zero does not exist*, since this is directly contradicted by the *finite vertical acceleration of the nontrivial solutions of hydrostatic (quasistatic) approximation equations, in which the equation with the vertical acceleration is replaced by the hydrostatic balance*. In [1] the author also did not give a proof of the existence of such a transition. Therefore, in view of the indicated contradiction, the theorem formulated in [1] is not true.

The author applies his own or unusual terminology ("inertialess vertical velocity", "climatic scales"), which is usually not necessary. However, here is the source of confusion and the reason for

*Electronic address: sibat@ocean.ru