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

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Dear Anonymous Referee $\#1$,

I was referred to your review by the Authors (of the Paper under consideration). And the first thing I've noticed in your review is the phrase "In general the math appears to be sound, although as the derivations are not extensive. I have put some faith in the authors that their work is correct". Sounds nice, but it is a paper about quite unusual mathematical statements and conclusions. I've been dealing with this work (the first part of it was already published, see the comments https://arxiv.org/pdf/2001.08637.pdf) for

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Discussion paper

more than 3 years, since it directly affects my work. So I will try to most explicitly point your attention to what you are putting some faith. The mathematics here is quite simplistic and does not require sophisticated manipulations. But still I can't get the answers from the authors. Also I'm afraid you are using a bit different language from that of the Authors when you refer to the models, used by UK MetOffice. And that your concept of quasi-hydrostatic and shallow atmosphere approximation is quite more specific (according to the modern classifications) and differs from quasi-hydrostatic approximation as understood in the Paper, in which the equation of the vertical momentum is replaced just by hydrostatic balance $\frac{\partial p}{\partial z} = -\rho g$. This is another reason to look at the maths before comparing it to other models, just to understand **what** you are going to compare to **what**.

The authors do not give answers to the questions below, as you can see at the other branch of the discussion of this paper. So if you trust the maths of the Authors, may be you can help in answering the questions or participate in the discussion.

1 First, you are going to trust

is the theorem, which states that as the vertical acceleration approaches zero, the hydrostatic approximation (which admits the finite vertical velocity) is asymptotically exact. A proof of this controversial statement is not given in the paper. This statement is responsible for the strange scales, for which hydrostatic approximation is applied in the paper (from 1 km in horizontal direction) in full accordance with the Theorem.

2 Second thing, you are going to trust

are two incorrectness (in my humble opinion), due to which the system of equations is also incorrect for weather prediction at *any* scales.

The Authors take the traditional L.F. Richardson's framework without citation (Kasahara 1966, the minor issues of neglecting of spherical geometry, and the use of the true temperature instead of the potential/virtual temperature I even do not consider now). And after that, they simplify the expression for the total time derivative of pressure $\frac{dp}{dt}$ by omitting the horizontal pressure advection. Let's look at the procedure. First let us substitute the expression of hydrostatic pressure to the total derivative of pressure. We get the expressions which can be traced back to at least Richardson (1922):

$$p = g \int_z \rho dz \tag{1}$$

$$\frac{dp}{dt} = \frac{\partial p}{\partial t} + \vec{v} \nabla p$$

$$\begin{aligned}
\frac{dp}{dt} &= g \int_z \frac{\partial \rho}{\partial t} dz + && \vec{v}_{hor} \nabla_{hor} p + v_z \nabla_z p \\
\frac{dp}{dt} &= g \int_z [-div(\rho \vec{v})] dz + && \vec{v}_{hor} \nabla_{hor} p - v_z \rho g \\
\frac{dp}{dt} &= g \int_z [-div_{hor}(\rho \vec{v}_{hor})] dz + v_z \rho g + && \vec{v}_{hor} \nabla_{hor} p - v_z \rho g \\
\frac{dp}{dt} &= g \textstyle\int_z [-div_{hor}(\rho \vec{v}_{hor})] dz + && \vec{v}_{hor} \nabla_{hor} p
\end{aligned} \tag{2}$$

After that, the Authors neglect the second term in the last expression above, based on the very strange scale analysis:

1. They estimate the scale of the divergence of first term by the scale of only one component. But the divergence itself is by order(s) of magnitude less than scale its components.

2. They estimate the $\nabla_{hor} p$ *dynamically* as $\rho U^2/L$. This estimation could be correct if for example we had initially a layer at the state of rest, and then the large horizontal scale perturbation of pressure would produce the waves (propagating with about the speed of sound). But this estimation is absolutely incorrect for weather prediction in the real atmosphere which is set in motion after cyclogenesis with zonal winds, motions of air masses and weather fronts. Indeed, the pressure advection can be sometimes very small, as well as the divergence, or sometimes the total derivative of pressure is small. But for a general case the neglect of pressure advection in favor of the divergence of mass flux may result in accumulation of additional vertical velocity.

The pressure advection is very cheap from the computations point of view, if we know the pressure and velocity. But if there was a meaningful reason to get rid of it, the much less ambiguous way would be just to further expand the last expression (2) above:

$$\frac{dp}{dt} = g\int_z [-div_{hor}(\rho\vec{v}_{hor})]dz + g\vec{v}_{hor}\int_z \nabla_{hor}\rho dz \tag{3}$$

$$\frac{dp}{dt} = -g\int_z \rho\, div_{hor}(\vec{v}_{hor})dz - g\int_z \vec{v}_{hor}\nabla_{hor}\rho dz + g\vec{v}_{hor}\int_z \nabla_{hor}\rho dz \tag{4}$$

The 2-nd and 3-d term compensate each other if velocity does not depend on z. So this expression

$$\frac{dp}{dt} = -g\int_z \rho\, div_{hor}(\vec{v}_{hor})dz \tag{5}$$

is exact if v_{hor} does not depend on z, for such a case it expresses an obvious fact that

$$\frac{dp}{dt} = -p\, div_{hor}(\vec{v}_{hor}),$$

when the pressure is the height of the air column. And it can be a fair approximation, ***in contrast to omitting the pressure advection, which is not actually an approximation but an*** **unbalancing** ***of the expression for the total derivative of pressure.***

I will illustrate it with the motion of a wagon with sand (see the attached picture) which corresponds to the horizontal bulk transport of the masses of air. Of course, you will *never see such a pure motion* in the real atmosphere, since it may be a king of seconary flow compared to global waves. But here I put it separately just to illustrate the formula.

The pressure corresponds to the height of the sand, and obviously, when the wagon moves as a whole $\frac{dp}{dt} = 0$ for any column of sand. So in the expression of the total derivative $\frac{dp}{dt} = \frac{\partial p}{\partial t} + v\frac{\partial p}{\partial x} = 0$ and $\frac{\partial p}{\partial t} = -v\frac{\partial p}{\partial x}$.

Now if we "neglect" the pressure advection $v\frac{\partial p}{\partial x}$, we will get some finite, and may be very big value of $\frac{dp}{dt}$, instead of ZERO.

Returning to the atmosphere, ***this is why the additional vertical velocity may be accumulated***, and ***this is why the energy conservation is violated***, since $\frac{dp}{dt}$ is a part of the expressions for the vertical velocity and the change of the full energy.

3 The last thing you are going to trust is the approach to prepare the system for linear analysis.

The hydrostatic approximation is not an evolutionary system, since one of the equations is diagnostic, not prognostic. The traditional way to make it evolutionary is the discretizations of the vertical operator. There are numerous works devoted to stability and correctness of the hydrostatic approximation, which had proved that discretizations used in the major weather prediction systems are well posed.

Instead of this rich experience the Authors declare a new variable "$\dot{M}$" as independent of M and make linear analysis of this bigger system of equations for functionally dependant variables. Besides the fact that such an introduction makes the system degenerate, it becomes difficult to make comparison with the well known results. So my question is what are the benefits of such "evolutionarization" as compared to traditional way, and why not making the analysis the usual comprehensible way, compare with the previous results, and underline the new idea.

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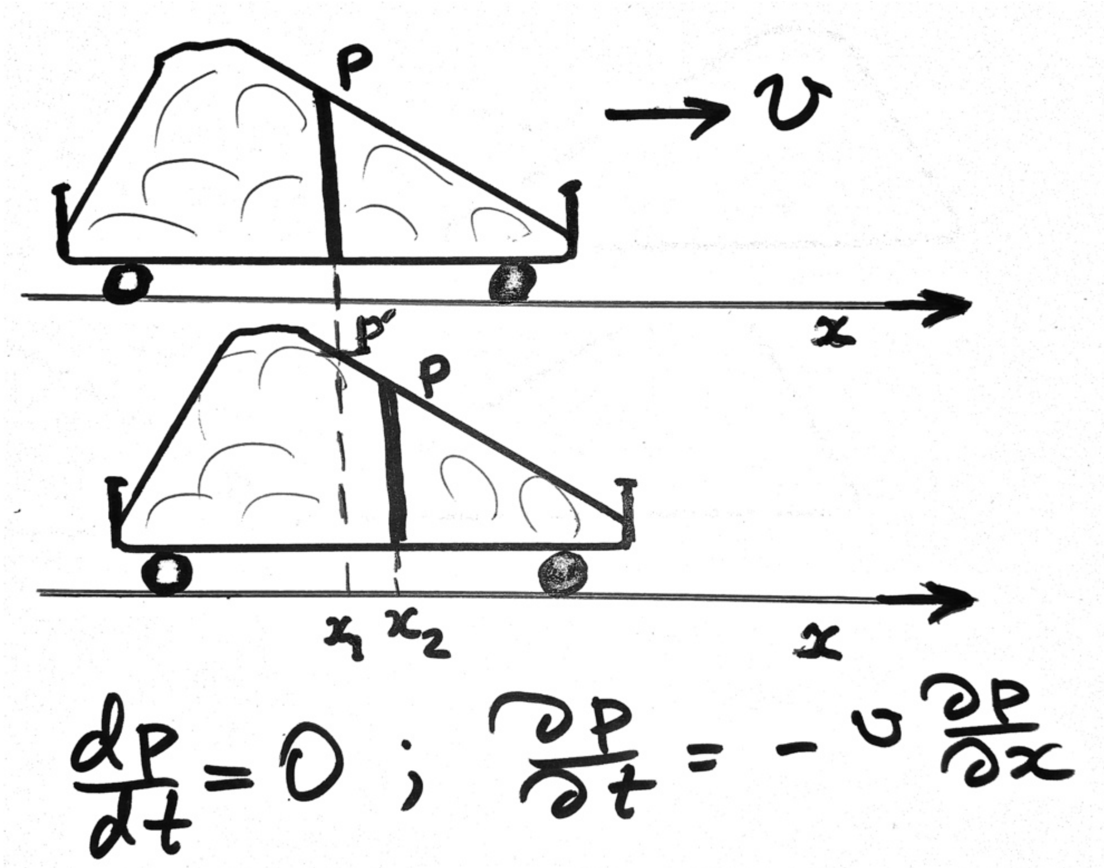
p
v
x
p'
p
x_1 x_2
x
$\frac{dp}{dt} = 0$; $\frac{\partial p}{\partial t} = -v \frac{\partial p}{\partial x}$

Fig. 1.