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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 Robert Nigmatulin and Xiulin Xu,

1 "However, you are not satisfied with the process of estimating A. Thus, we only give an estimation of B"

It's not about my satisfaction, but whether your estimation of A (the horizontal divergence of mass flux) in this manuscript and in

https://doi.org/10.1134/S0015462818040201, was correct for the considered scales. I've shown that it is wrong at synoptic scale, and I would expect a discussion from the point of view of logic and mathematics, rather than pleasing me.

Now I will show again that the estimation of B, the advection of pressure, given in https://doi.org/10.1134/S0015462818040201 and replicated in your manuscript is also wrong for synoptic scales. You have estimated it as

$$\frac{\partial p}{\partial x}, \frac{\partial p}{\partial y} = \rho\, O\left(\frac{V_{hor}^2}{L_{hor}} + \frac{V_{hor}^2}{L_{cor}}\right).$$

For a general case of large-scale atmospheric dynamics this estimation is wrong for the reasons, I've already mentioned in my previous messages SC10 and comments on pubpeer.com.

Recall again the exact expression (SC9) for the total derivative of pressure in case of hydrostatic approximation:

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

I gave you a straightforward scale analysis of the pressure advection by directly substituting the expression of pressure as weight of the atmospheric column in the SC14 message and pubpeer.com, but you just ignore it.

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

Now I repeat the counterexample to your estimation from SC10. At synoptic scales the pressure gradient force is almost balanced by Coriolis force. From the geostrophic balance

$$f \cdot v = \quad \frac{1}{\rho}\frac{\partial p}{\partial x} f \cdot u = -\frac{1}{\rho}\frac{\partial p}{\partial y},$$

the pressure gradient would rather be estimated as $\frac{\partial p}{\partial x} \approx \hat{\rho} f U$, than $\hat{\rho}\frac{U^2}{L}$. I wrote about that in SC10, but instead of answering about this estimation at synoptic scales, you have answered that Coriolis force is taken into account in the equations (it looks like an art of replying without answering questions). If you estimate $\frac{U}{fL}$ as $\frac{1}{10}$, which is common at synoptic scales, your estimation of ratio of the advection term to divergence term is wrong by at least two orders of magnitude.

Finally, both advection of pressure B and divergence of mass flux A, as well as total derivative of pressure, are small as compared to pressure in hydrostatic approximation.

2 "The purpose of this paper is to analyze the shortwave instability of the quasi-hydrostatic equations."

Is it really? Please look at your abstract:

> *An advanced "quasi-hydrostatic approximation" of 3-dimensional atmospheric-dynamics equations is proposed and justified with the practical goal to optimize atmospheric modelling at scales ranging from meso meteorology to global climate. For the vertically quasi-hydrostatic flow with inertial forces negligibly small compared to gravity forces, the asymptotically exact equation for vertical velocity is obtained...*

Since your terminology may differ from the conventional, is it so that you still stick to the horizontal scales from ≈ 1 km for the system of equations given in this manuscript and in `https://doi.org/10.1134/S0015462818040201` ? Or you consider it wrong already and the scales of applications of your systems are now redefined?

The correctness of the instability analysis is another question, which is addressed in other comments.

3 Please do not make any comments on anything that has nothing to do with the manuscript's content. Malicious comments on the authors are not welcomed.

Dear Robert Nigmatulin and Xiulin Xu,

All my comments are about the manuscript `doi.org/10.5194/gmd-2020-146`. And since this manuscript up to (3.1) is a replication of another manuscript `doi.org/10.1134/S0015462818040201` , written by the First Author, I refer to that paper and critical reviews on that paper.

`doi.org/10.1134/S0015462818040201` was a subject of critical reviews over three years (even before its publication), and open comments to the Editorial Board of the journal where it was published in September 2019. The comments are published at arxiv.org and pubpeer.com since the beginning of 2020. This is why I refer to them also. Is it not true, or what is malicious about that?

The claims which are made in the Theorem are very important. The analysis of the total derivative of pressure, violation of the energy conservation, persistent non-citation of L.F. Richardson, strange notations, linearly dependant "independent" variables, strange results of linear analysis: all these issues are of minor importance, as compared to the major claim in your Theorem, reflected in the title of this manuscript

that hydrostatic approximation can be applied on account of the smallness of the vertical force of inertia,

Despite my persistent enquiries for a proof of such a statement for years, I see now the

Theorem with this claim being published again, and again without any proof.

I considered such a claim as a typical mistake of a student of the 3-d year, who have heard about small vertical velocity and small acceleration in models of shallow water equations or more generally in hydrostatic approximation, but had never bothered to look in the manuals for the reason of these approximations. And a very “logical” conclusion for such a student would be the conclusion of your Theorem: vertical acceleration is small, hence we get hydrostatic approximation. I’d just send such a student for reexaminations or explain the mistakes.

But the story here is not about a student or a researcher, who’ve published a mistake or overseen a previous research. Happily, if such a case could be forgotten. But here the paper `doi.org/10.1134/S0015462818040201` had received both private and public critical reviews. And after that the Theorem is again replicated here. The importance of this discussion is emphasised by the fact, that the First Author was recently appointed as the “scientific director” of the Institute of Oceanology in Moscow and head of the subdivision of Mechanics at Moscow University. Is it malicious or something wrong about mentioning this public information inhere? I explicitly remind about that inhere, to underline the fact, that the First Author have the power to influence on decisions about the fate of the scientific groups and teaching programs. In particular, the Theorem is incompatible with studies of internal waves. So the questions of mathematical correctness are particularly important here. (Is it not, or something is wrong or malicious?)

To my opinion, the questions here are quite serious to put it on the shoulders of a student who was not even an Author of the Theorem, which was formulated by the First Author earlier in `doi.org/10.1134/S0015462818040201`. I hope that the First Author will explicitly put his name under the replies (if any name will be present at all, I remember SC5 from the Executive Editor).

Best wishes, Ilias Sibgatullin.

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