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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

Anonymous Referee #1

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Review of "Quasi-hydrostatic equations for climate models and the study on linear instability" by Robert Nigmatulin and Xiulin Xu

This paper analyzes the sensitivity of the quasi-hydrostatic approximation to the atmospheric fluid equations to differences in horizontal and vertical wavenumber, and to different approximations to the diagnostic equation for vertical motion. 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. I have instead focused my review primarily on the framing of this derivation and the conclusions drawn from these results. Several comments are as follows:

Line 19: Please contrast the quasi-hydrostatic equations against other systems with filtered sound waves, e.g. anelastic and pseudo-incompressible equations. Also it would be advantageous to give other solutions for avoiding the problem of sound waves, such as implicit-explciit temporal integrators.

Line 21: "Most global climate models are based on a system of dynamic equations in quasi-hydrostatic approximation." I believe this statement to be incorrect. Most global atmospheric models either use the fully non-hydrostatic equations or the hydrostatic equations with shallow atmosphere approximation. To the best of the reviewers knowledge only the UK Met Office model has an option for the quasi-hydrostatic equations. See, for example, Ullrich et al. (2017).

Line 36: "Almost the entire mass of the atmosphere is located in the layer with thickness H of order 10km. So the atmospheric dynamics outside polar zones can be considered in quasi-Cartesian coordiante system..." Why does the relative thinness of the atmosphere affect the use of a Cartesian coordinate system?

Line 36-38: The authors should be more clear that they are using a planar approximation of the equations. Otherwise there isn't an explanation for neglecting the curvature terms in equations (1.2)-(1.4) below.

Line 138: Also see Kasahara and Washington (1966). You may also wish to refer to DeMaria (1995) equation (2.13), which is an example of recent mention of this equation.

Line 140: There has been some work recently showing that these non-hydrostatic terms may be more important in a moist context. See, for example, Gao et al. (2017) or Yang et al. (2017).

Line 145 (equation 2.23): Do the quasi-hydrostatic equations here satisfy any sort of energy principle? If the system exhibits instability for certain ratios of horizontal and vertical grid spacing, then the diagnostic vertical velocity equation must be responsible for the addition of energy to the system. Presumably one should be able to show which terms are responsible for this violation.

Line 220: It would be advantageous to show how the quasi-hydrostatic equations diverge from the unapproximated equations when it comes to instability. One should be able to show agreement between the quasi-hydrostatic equations and unapproximated equations for a certain regime of k_hor and k_ver

Line 460: If I'm understanding the authors correctly, this instability is present regardless of the values of kappa2. Even for small values of kappa2 the system will eventually go unstable without some external control. So wouldn't a better solution be to use an equation set that actually satisfies a closed energy principle?

Line 475: Can anything be said about the accuracy of these equations, analogous to Davies et al. (2003)?

References:

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