Review of:
On the hydrostatic approximation in rotating stratified flow
by Achim Wirth

Summary and recommendation This paper develops a formal analysis of the differences between hydrostatic and non-hydrostatic equations of motion for incompressible rotating stratified fluids. The author's approach is based on projecting the terms in the space of divergence-free vectors by using Fourier transform, which the author used in previous work to construct a non-hydrostatic model. The proposed method is interesting, but highly technical and often hard to follow. Moreover, as the paper appears to focus exclusively on regimes for which the hydrostatic approximation is generally accepted as valid, it is hard for me to understand what the truly new contributions to existing scientific knowledge are. Indeed, it seems to me that the conclusions reached by the author are essentially the same as those that can be obtained much more simply by comparing the dispersion relations for balanced and inertia-gravity wave plane wave solutions in the hydrostatic versus non-hydrostatic regime. I believe that most readers would be more interested in understanding the errors arising from using the hydrostatic approximations in non-hydrostatic regimes, but this remains unaddressed.

In terms of presentation and structure, the paper is very technical and hard to follow, primarily because the author rarely feels the need to explain why he does what he does and to what purpose. The paper is also parsed with numerous typos and small mathematical errors/inaccuracies. While I could follow the derivations up to Section 3, I started to lose the plot at the beginning of Section 4, when the author introduces an extension of Fourier space to 4 dimensions without explaining why this is needed or useful. The abstract's final statement: 'A special emphasis in on unveiling the physical interpretation of the calculations' is enticing, but I suspect that most readers are likely to disagree with the author's definition of 'physical interpretation'.

On the basis of the above and of the specific comments listed below, reaching publishable status will require a very substantial amount of work aimed at: 1) better identifying the scientific goal of the paper and of the gaps in knowledge it aims to fill; 2) simplifying the mathematical derivations and explaining what these aim to achieve and why; 3) discussing the limitations and caveats of the method arising from the real ocean being not triply periodic; 4) explaining precisely what additional physical insights the proposed method brings compared to simply looking at the linear wave dispersion relationships in the hydrostatic versus non-hydrostatic regimes.

Specific comments

1. Abstract, lines 5-6. 'The validity of the hydrostatic approximation [...] wave motion to balanced motion' What do you mean by validity here? For internal waves, the hydrostatic approximation causes some error on the frequency, but then what? Even

at high resolution, one cannot really expect to resolve a full wave spectrum. In any case, isn't that kind of conclusion something that can be deduced from existing linear wave theory? My impression is that a non-hydrostatic treatment was only essential for resonantly excited trapped lee waves, which cannot exist in the hydrostatic regime.

2. Lines 15-16: Equilibrium solutions are in the kernel [..] are in the kernel of its adjoint' What does that mean physically? What are readers supposed to make of this?

3. Lines 17-20 'Using the Heisenberg-Gabor limit [...] physical interpretation of the calculations' I don't understand how these conclusions follow align with the work presented, which seems to pertain only to the discussion of regimes generally regarded as hydrostatic. In many places in the paper, the author mentions instances for which the hydrostatic breaks down, but which are not otherwise discussed theoretically. My reading of the paper is that the author essentially concludes that the hydrostatic approximation is appropriate to hydrostatic regimes, and possibly not appropriate to non-hydrostatic regimes, which is a bit tautological. Or did I misunderstand the paper?

4. Lines 42-43. 'The present paper discusses the gray zone [...] using a purely analytical approach'. I don't understand how the concept of gray zone is pertinent here. Please clarify what you mean. Moreover, if you define identifying the gray zone as an objective of the paper, the expectation is that you should come back to it a the end of the paper.

5. Line 51-52: 'Furthermore, the effect of resonances are not included' Why not? Does that mean that such resonances cannot occur in realistic modelling? What is the impact of the hydrostatic approximation on these? If the hydrostatic approximation cannot correctly capture such resonances, doesn't that imply that the hydrostatic approximation is not necessarily valid for realistic modelling, in contradiction to what is stated in the abstract?

6. Lines 68-85. This is incomprehensible at this stage. Can't this be translated in physical terms? Ultimately, the reader would like to understand whether the formalism developed can help go beyond the conclusions derived from linear wave theory. The author could first remind the reader that when considering plane wave solutions of the linearised equations of motion for a f plane and uniform buoyancy frequency, only the dispersion relation for internal gravity wave is affected by the hydrostatic approximation, but not that for balanced motions. The author should first start by summarising the present state of knowledge and explain what he expects to learn from his considerably more complicated formalism. In doing so, it might be useful to mention the case of trapped lee waves as an example of internal waves that exist only in the non-hydrostatic system. In this regard, can the author's more compli-

cated treatment describe trapped lee waves in the non-hydrostatic case? How can we compare the two systems in that case?

7. Line 150. Is this decomposition physical and mathematically well defined (i.e., unique)? Why is it useful or necessary to consider such a decomposition of the pressure? Moreover, why is the treatment of P_b different from that of the other? I.e., why is it not
$$\Delta^2 P_b = \frac{\partial b}{\partial z}$$
To me, this different treatment makes the approach mathematically and physically inconsistent.

8. Equation (11). I don't understand this equation. The projection operator is supposed to be an operator acting on 3D vector, which is not the case of $\nabla \cdot \mathbf{u} = 0$.

9. Section 3. The method appears to filter out all non-trivial solutions of the Laplace equation $\Delta^2 P = 0$, which in practice are generally essential to construct the full solution of the equations of motion. Can the author comment on this? Moreover, the oceans are clearly not triply periodic. Can the author comment on the limitations and caveats that result from making this clearly unrealistic assumption?

10. Lines 212-215. 'In numerical models based on Fourier representation [...] prescribed to the elliptic solved' - This seems misleading to me because the author compares the accuracy for one Fourier mode making up the solution for the full eliptic problem versus the accuracy of the full solution of the ellipic problem. Since the oceans is not triply periodic, I would expect that one still needs to add a solution of the Laplace equation $\Delta^2 P = 0$ to construct the full solution of the elliptic problem when using a Fourier representation. If not, I don't know how the author can avoid the development of the Gibbs phenomenon near boundaries, which can cause a lack of convergence of the full solution near boundaries. To be fair and meaningful, discussion of accuracy should focus on the full solution for both approaches, which would require that the author discusses how he handles topography and boundary conditions in his approach.

11. Section 4. Fourier space extension. I started to get lost at this stage, so gave up on trying to understand what the author is after. It would be helpful if the author could be somewhat more pedagogical in this approach. What may be clear to him may not be as clear to the reader.

12. As a general comment, could the author clarify what is his intended readership? How are the readers supposed to make use of his results? For instance, an important issue in non-hydrostatic codes using an ellpitic solver is preconditioning. Can the author's results be potentially be used to understand how to improve on existing preconditioning techniques?