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Discussions

Interactive comment on "A possibility of large scale intrusions generation in the Arctic Ocean under stable-stable stratification: an analytical consideration" *by* Natalia Kuzmina

Anonymous Referee #2

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The author addresses an issue of destabilization of the geostrophic flow by vertical diffusion of density and speculates that such process might be the relevant in explaining the formation of intrusive layers in the Arctic Ocean. I enjoyed reading this paper and it is particularly pleasing to see the theoretical analysis. While I believe that there is a potentially great scientific merit to this work, I have several concerns about the clarity of the presentation, novelty of the theoretical results, and applicability to the Arctic Ocean. Please find my detailed comments and questions below that are aimed at improving the quality of the paper.

1. The title of the paper makes a clear reference to the Arctic Ocean, however the analysis of the instability presented here is not restricted to any specific part of the

ocean. In particular, a direct quantitative comparison of the theoretical results to the intrusions in the Arctic Ocean is not extensive and occupies only a small fraction of the manuscript. I would thus recommend focusing one topic: either on application to the Arctic intrusions or on a discovery of a new type of fluid dynamical instability.

2. The author claims that this manuscript shows for the first time that a diffusion can destabilize the geostrophic flow. However, the novelty of the presented findings can be questioned as a discussion of the relevant scientific literature on viscous instabilities is not present. In particular, the author should discuss the work of McIntire (1970), Baker (1970), and Calman (1976) who consider experimentally and analytically the diffusive instability. In addition, a discussion of Murno (2010) experiments, which show the importance of viscosity, needs to be present. These are just several of the papers that came to mind, I'm sure there is more literature on the visco-diffusive instabilities of geostrophic flows.

3. Writing out the QG equations with the stratification parameter N that depends on y is not common. There should be a reference to a book or a paper that presents its proper derivation i.e. an asymptotic expansion in Ro number where N=N0+O(Ro). I'm not certain, but there might be some terms might be missing in Eq. 7 if N=N(y) – please check and give a reference.

4. When neglecting the betta effect please provide quantitative estimates of a latitude at which betta-effect becomes less important (e.g. at 75 degree latitude betta is 25% of its value at the equator – is that beta negligible compared to shear term?). If betta-effect from your scaling end up being negligible at any latitudes then the instability that you consider should be of a small horizontal length scale.

5. A discussion of why mass and momentum diffusivities are assumed to be the same is missing. Note, that in a non-turbulent regime, which might be adequate for the deep Arctic, the viscosity is an order of magnitude larger than heat diffusivity and three orders larger than salt diffusivity.

6. Deriving Eq. 9 from Eq. 8 requires vertical integration since p_z=-g rho and I'm not sure what was done with the vertical integral of the term U*p_zzx when U=U(z). Perhaps showing more steps would clarify things.

7. Eqns. 2 and 10 should contain the physical mechanism behind the instability which was not explained throughout the paper. The authors take a dry math approach to the instability problem by calculating the growth rates; however, omitting the physical mechanism of the instability dramatically reduces the understanding of the problem for the readers. Perhaps a schematic showing a positive feedback loop would be helpful.

Also missing is a discussion of fundamental reasons for the existence of this instability i.e. does it release a potential energy of mean flow, does it feed on its kinetic energy or something else?

8. In assessing the stability properties of Eq. 11 the author assumes a special case of U(z)∼zˆ2 and present analytical solutions (Eq 15). It is not clear to me how was the growth rates in Eq. 16a,b calculated from Eq. 15.

9. Eq. 11 that determines the stability of the flow does not have any y-derivatives and hence the stability properties do not depend on the y-direction wavelength. Thus, it looks like this instability is a 2D (in x,z–plane) rather than the 3D instability that the author claims to have investigated.

10. The authors demonstrate that Eq. 11 has unstable solutions for U∼zˆ2 but it is not obvious that other, more realistic, profiles of U(z) can also lead to an instability. It would be useful to solve Eq. 11 via the eigenvalue decomposition in z and show that arbitrary profiles of U (that have curvature in z) are indeed unstable.

11. The meaning of discussion of limits at z=+-inf is not clear and needs to be organized better. In particular at z=+-inf U->inf which is unrealistic for the ocean and for the theory which assumes U to be small in a QG sense. Why not using finite domain size z-=[0 H] and a corresponding no flux boundary conditions?

12. It is not physical to have a growth rate that grows with increasing wavenumber as it implies that any kind of small scale noise would be preferentially amplified. The author motivates the paper with the idea that the size of intrusions in the arctic ocean might be explained by an instability. However, there seems to be no preferential wavelength at which the instability occurs and hence one cannot expect the appearance of intrusions of particular height.

In addition, the theory breaks down at a particular length scale which the authors choose as the scale of intrusions and use it to calculate the growth rates. It is questionable to use these estimates since the theory technically does not apply at this marginal scales (i.e. the neglected terms need to be included).

13. Because there is no high wavenumber cutoff it is questionable whether the numerical model results shown in Fig. 1 are realistic; the step formation shown in Fig. 1a might be at the size of the numerical grid and hence their dynamics is not adequately resolved.

14. A discussion of Orr-Sommerfeld equations seems unnecessary as it only makes a mathematical connection with insufficient improvement of our physical understanding of the problem; thus, it only makes the paper harder to understand.

15. Application to the Arctic Ocean can be questioned because i) there is no preferential length of instability that can be compared with the size of intrusions and ii) the growth rates are of the order of years are too large because the mean currents will most likely significantly change on the long time and very small spatial scales of the instability.

16. I'd suggest working on the brushing up the grammar and logical presentation of the paper. Many paragraphs do not contribute well to the clarity of the paper and can be outright deleted. The title can be clearer as well: e.g. Generation of large-scale intrusions via diffusive instabilities.

References:

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