RC1 Response

Dear Referee #1,

Thank you for your thorough review of our manuscript. Your comments helped us understand a critical point in our manuscript that needs to be addressed to make our work better understood. We just realized that we had used ambiguous terminology that misled the interpretation of our intentions and methodology. We appreciate your feedback and have addressed your comment as follows:

- **Comment:** The title is misleading: the approach is not semi-analytical. It is a numerical model. I am also uneasy with the notion of vertical propagation on PV when the latter is materially conserved in absence of diffusion and that there is no vertical advection in QG.

  Response: We acknowledge the concern regarding the title and description of our approach. In the title and the text, we used the term propagation to give the idea of the transmission of PV signal throughout the water column, given the rearrangement due to the non-linear coupling. As you highlighted this term may result in a misunderstanding given the absence of vertical advection in the QG approximation, hence we clarified in the text. Since we do not use primitive equations to evolve and model the whole system and find a solution numerically for a limited case after deriving the governing equations for the system we think that "semi-analytical" is representative in our case. However, this may mislead in the title, given that we are presenting a toy model to investigate an observed process so we are changing the title to:

  "Study of the role of abyssal ocean stratification in the rearrangement of potential vorticity through the water column"

  and lines 5-7:

  "A quasi-geostrophic level model, with four coupled layers, a free surface, and a mathematical artifice for parametrizing decadal time evolution has been considered, proving that the relative thicknesses and the density difference among the layers are the two critical factors that determine the dynamical characteristics of this rearrangement."

- **Comment:** I am uneasy with the justification of the equations using the depth z instead of ρ, as the justification (based on crude finite difference) differs significantly from the rigorous derivation (based on vertical averaging, see e.g. [1]) of layered models. There must be other ways to justify the change (such as arguing that at the relevant order in Ro, isopycnals are flat - corrections only dynamically matter at higher order in Ro).

  Response: This comment is pivotal for us to improve our discussion and better explain our motivations. As we tried to highlight in sections 1 and 2, our study was inspired by local observations. This led us naively to use interchangeably the terms "layer" (since we are describing observed layers in the stratification of the Ionian Sea) and “level” (since our model is technically a level model and not a layered model) and we understand now that this confuses, instead of helping in the interpretation. In fact, using the depth z instead of ρ is a standard derivation for level models, which do not assume a linear relationship between the density and depth like in layer models which are designed to better follow isopycnals. As we explained in the text, given the abyssal density structure observed we cannot use a layer model approximation, and this is why we resolved to a level model. We revised our text using the correct terminology, avoiding this ambiguity that you helped us identify.

Relevant changes in the text:

Lines 51-56:

“Given the presence of the dense abyssal layer observed in the Ionian Sea, the best representation is depicted by a QG equation with four non-linearly coupled levels having parameters based on in-situ data. We used a level model instead of a layer model, meaning that the discretization uses a z vertical coordinate instead of a ρ coordinate to follow isopycnals. The two schemes are equivalent when ρ can be considered as a linear function of z, but this is not our case. In fact, the density structure observed in the Ionian Sea abyss is not linear in depth, hence we needed a coordinate that accounted for this variability through the entire water column. In numerical models of the ocean both approaches are combined, the vertical coordinate is typically used only in the surface layers, while the interior, which is considered more stable, is treated using isopycnals (Griffies et al., 2000).”

lines 128-136:

“Equation (2) contains derivatives in z, which must be discretized to conform with a 4−layer representation. We use a level model, i.e., the finite-difference discretization considers fixed vertical levels to define the layers instead of density. Since the discretization is performed on z directly, instead of taking ρ as the vertical variable, the formalization of the level model is slightly different than the more common layer models (Vallis, 2017; Cushman-Roisin and Beckers, 2011). However, the two approaches are equivalent when considering long-scale systems where density is a linear function of z, and are commonly combined in more comprehensive ocean models (Griffies et al., 2000). Using z as a vertical coordinate makes it possible to use the simplest discretization approach, accurately representing pressure gradients and equation of state for a Boussinesq fluid (Griffies et al., 2000).”

lines 376-379:

“We applied a quasi-geostrophic level model on a two-dimensional ocean, with four non-linearly coupled layers and parameters observation-based, to investigate the role that abyssal stratification can have on the rearrangement of potential vorticity through the water column. The transmission and stabilization of potential vorticity through the bottom depend on the relative thicknesses and stratification of the layers, which have proven to be critical factors in reshaping the water column circulation patterns.”

lines 385-390:

"The formulation of our model did not follow the usual layer models employed in studying vortex stability (Sokolovskiy, 1997; Carton et al., 2014; Katsman et al., 2003) or potential vorticity propagation from the surface (Smith and Vallis, 2001; Zeitlin, 2018), because we were not focused on the evolution of a perturbed vortex on short time scales, nor in higher order processes like those involved in the upper layers. Instead, we wanted to investigate the problem with an upside-down point of view, trying to understand the impact on the potential vorticity structure in the water column due to long time scale stratification changes in the abyss, like those observed in the available data."

- **Comment:** line 145: the statement is technically erroneous. $\partial\psi/\partial z$ in equation (2) is a rescaled buoyancy anomaly. The vertical velocity is proportional to the material derivative of this buoyancy anomaly. So, although $\partial\psi/\partial z = 0$ implies no vertical velocity, the reciprocal is not true: no vertical velocity does not imply $\partial\psi/\partial z = 0$. **Response:** We apologize for the oversight. Our intention was to keep the mathematical notation as straightforward as possible to enhance readability and accessibility, but unfortunately, we oversimplified it. We did not mention that we applied the method of separation of variables to the streamfunction to apply the boundary conditions, which is a commonly used but important assumption to follow the mathematical passages. To clarify this, we added a more detailed derivation and explanation.

  Relevant changes in the text:

  lines 142-165:

  "To apply the boundary conditions in the vertical we employed the variable separation method for the streamfunction, i.e., we assume that $\psi(x,y,z,t) = \tilde{\psi}(x,y,t)\psi'(z)$. " and we changed eq. (4), (5), (6), and (7) accordingly.

- **Comment:** Please clarify where equation (5) comes from.

  Response: As for the previous comment, we added a more detailed derivation and explanation of the boundary conditions application.

- **Comment:** Equation (12) is unnecessarily general since only U_1 6= 0. It would be simpler to just define $\bar{\psi}_1$ in equation (12) and state in the text that $\bar{\psi}_j$, $j = 2,3,4$ is set to 0 at $t = 0$. There is no point in plotting curves $\bar{\psi}_j = 0$ in Fig. 3(a).
  Response: We agree with the suggestion on equation (12), hence we modified it accordingly. For what concerns the figure, we found it helpful to have a visualization of the activation of the bottom levels in the final state starting from zero, so if you agree we would like to keep Fig. 3(a) as it is for completeness.

- **Comment:** The parameter β appears in the equations but its value is not given (unless I am mistaken).

Response: The β was kept in the derivation of the scheme but in this work is not used, since in our case the β-effect is negligible, we clarified this in the manuscript and dropped the term in eq. (8).

We hope that these revisions address your concerns satisfactorily. Thank you again for your insightful comments, we think that it helped us improve the manuscript significantly. We uploaded the revised manuscript with highlighted corrections, and remain at your disposal for any further suggestions and discussion.

Best regards,

Beatrice Giambenedetti, Nadia Lo Bue, and Vincenzo Artale