

This manuscript addresses an important and timely question: how the internal tide-eddy interactions and internal tide-topography interactions change the vertical modes of internal tides in the Amazon shelf and off shelf region. The work tackles this question with high-resolution simulations and case-based analysis. The three selected cases provide helpful and clear comparison of internal tide propagation features under three different eddy conditions. I think the manuscript has clear potential for publication. That said, the manuscript would benefit from more clarifications and more careful physical interpretations. My specific points are listed below:

1. Title: The key focus of the study is on how the eddy-internal tide interactions affect the internal tide characteristics. The phrase ‘eddy-permitting’ is not a central takeaway. I suggest revising it to ‘: Evidence from 3-km NEMO-AMAZON36 Simulations.’
2. For the difference among with-eddy and no-eddy cases, how do you determine that the eddy-encounter geometry is the dominant factor and rule out the contributions by different stratification, NECC intensity? I suggest showing a simple comparison of key background conditions, such as N2, mean current field. A simple 2D ray-tracing analysis could also be considered as helpful additional support, but I do not view it as essential.
3. L13, ‘to the flow’ is not standard phrasing. I suggest revising it to ‘... decomposition to the high-resolution (3 km) ...’.
4. L27, the statement that “Mode-2 energy is completely arrested” is physically striking, but the wording may be too absolute unless supported by very explicit quantitative metric. “Strongly blocked” or “sharply attenuated” may be more appropriate.
5. L55, are there more realistic simulations that capture the interaction and trapping processes? It would be helpful to know that these processes does occur and could be examined in more realistic ocean configurations.
6. L31, the energy transfer from low mode to higher modes and from internal tides to background is not ‘contrasting’.
7. L62-65, you may want to elaborate on the motivation for focusing on daily-timescale variability – i.e. why variability on this timescale is important.
8. L107, it would be informative to give a short but explicit resolution argument: what does 3 km imply for the horizontal wavelengths of Mode-1, Mode-2, and Mode-3 in this region.
9. L124, typo: ‘runned’ should ‘ran’.
10. L129, Could you clarify whether the internal tide signal was extracted from a long time-series and subsequently analyzed in 25-hour segments, or whether harmonic fit was applied independently to each 25-hour segments? In the latter case, the

potential frequency leakage errors should be carefully assessed and discussed.

11. L175, the phrase “horizontally homogeneous stratification” is misleading. My understanding is that the modes are computed locally at each grid point using a local profile of mean stratification profile; if so, that wording would be more accurate.
12. L174, because topographic scattering is central to the paper, the use of flat-bottom modes might be a potential caveat. Could you add more discussion to elaborate how much this approximation affects the higher-mode projections near steep topography?
13. Since the first three baroclinic modes are emphasized and used for analysis, it would help to quantify what fraction of the total baroclinic energy or flux they represent relative to the first 10 solved modes.
14. L269, typo: Weiss, 1991.
15. L272, one important point: Eq. (13) for the Okubo–Weiss parameter. The traditional form of this criteria is that the sign of $W=Sn^2+Ss^2-\zeta^2$. This sign convention should be verified carefully.
16. L301, the manuscript frequently refers to CE “core” and CE “edge,” but a quantitative definition would make the case selection much more rigorous, for example in terms of normalized distance from the eddy center.
17. I think that it would strengthen the physical interpretation to report a few nondimensional measures for the two main eddy cases, such as Rossby number, Froud number.
18. L328, the three selected cases are interesting and well chosen, but a short statistical summary would make the choice and related conclusions much more persuasive. How often do NE, CEC, and CEE situations occur?
19. L417, since energy fluxes from A and D merge along the propagation, I suggest discussing more explicitly whether some beam geometry changes could also reflect multi-source interference, not only eddy refraction.
20. L426, figure4, the local C_{mn} and H_{mn} maps are informative, but an integrated budget over a clearly defined interaction box would be of great interest and helpful for the dominant-mechanism interpretation.
21. L452, it is very interesting that the mode 2 is mostly blocked but mode 3 appears weaker and more spatially spreading. It would be helpful to clarify the physical reason for this difference in behavior. Is mode 3 less clearly blocked because it is already less coherent, or more locally generated near the seamount?