RC2: 'Comment on egusphere-2025-4158', Anonymous Referee #2, 16 Oct 2025

This paper presents a detailed analysis of measurements on a shelf in the Gulf of Oman that show predominantly diurnal internal tides. The results are carefully described and offer a valuable contribution to the understanding of internal tides in that region.

We appreciate the reviewer's thoughtful feedback and valuable suggestions, which have strengthened the manuscript. Our replies are listed in red after each reviewer's comment.

I recommend publication after a minor revision, in which the following points are considered.

The authors suggest that the diurnal internal tides may be generated remotely because locally the barotropic tidal current is mainly semidiurnal. This could be correct, but it would be worthwhile to consider the local setting more fully, perhaps with a simple internal-tide generation model. Diurnal and semidiurnal internal tidal beams have very different slopes and the local bathymetry may be more favorable to generate one or the other.

We agree that from the data presented in this manuscript alone, local generation can not be definitely ruled out. We have added qualifications in the respective sections (e.g. L265). Local shelf slope topography could indeed favor energy conversion in the diurnal band. We have in a parallel study performed a regional assessment of energy conversion, slope criticality and regional propagation. Since the present manuscript is however already long (as commented below) these results will be presented in a separate manuscript which is in preparation.

Another argument put forward in the paper is the shift and variability in fortnightly cycles between barotropic and baroclinic signals. However, such shifts have been demonstrated to occur even in locally generated internal tides, because the M2 and S2 (or similarly K1 and O1) beams propagate at different angles, creating spatially varying phase shifts in the baroclinic fortnightly cycles, plus a sensitivity to time-varying background stratification.

We agree that interference both vertical, from different beam angles, and horizontal, from separated generation locations, can produce spatially variable phase shifts in baroclinic fortnightly cycles. We have added qualifications in the respective sections (e.g. L256) with reference to relevant literature (Gerkema 2002).

As a general point, I think the paper could be shortened (leaving out details that do not really add much to the story), highlighting the main findings. It is now quite a long paper on a relatively limited dataset.

We have revised the whole manuscript and have shortened sections where possible.

Besides, I notice a number of smaller points that deserve attention in a revision:

1) line 88, if rho denotes (in-situ) density then the expression for N2 should have a term involving the speed of sound as well.

Thank you, we used potential density and have now specified it in the text.

2) eq. (7) is based on the hydrostatic approximation (which is fine for internal tides), not “non-hydrostatic” as stated in line 167.

Thank you for pointing this out. We have corrected the text. The reference to “non-hydrostatic” was a leftover from an earlier draft in which we considered solving the full non-hydrostatic vertical-structure equation. Because the non-hydrostatic corrections remain negligible during periods of weakened stratification, we reverted to the hydrostatic formulation.

3) In figure 3d, what direction do the vectors refer to? (cross-slope?)

Vectors are in geographic orientation with north up (north arrow is shown in the figure).

4) In line 422, the authors state, as if it were evident, that internal tidal frequencies can deviate by “Doppler shifting”. How? In any situation where both the source and observer are fixed in space (as is the situation here), no Doppler shifts occur (and the presence of a mean flow does not change this!). In other words, this statement needs a more careful consideration (or perhaps removal).

Our point concerns the Eulerian phase modulation of the internal tide at a fixed site due to space- and time-varying background currents that advect and refract the wavefield. In that case, the observed phase rate can differ from the intrinsic value ($\omega_{obs}=\omega+k \cdot U$), producing apparent shifts within the diurnal band. We thank the reviewer for correctly pointing out that this should not be called Doppler-shift. We have replaced “Doppler shifting” with “phase modulation by space- and time-varying background currents”.