Reply to Reviewer's Second Revision

General comment:

Overall, I was not very satisfied with the authors' responses to my comments, as they were often lengthy, sometimes irrelevant or off the point. Some are highlighted below, where I either provide an answer to their response or further clarify my point.

We appreciate the Reviewer's feedback and recognise the need for brevity and relevance in our responses. We have carefully reconsidered the points raised in this second revision and aim to address them more directly in this reply.

Remaining major comments:

1) *The authors argument to neglect the effect of waves on the wind drag and those from the wind and waves on the turbulence and vertical mixing at the surface remains unjustified. Since most parameterisations depend on bulk parameters such as the significant wave height, such effects can similarly be accounted for in both model configurations (monochromatic versus fully-spectral). These effects have been proven to have substantial effect on vertical current profiles, and should therefore be included given that the impact of the fully-spectral configuration is discussed with velocities taken near the surface or at the bottom. For instance, the role of whitecapping in accelerating currents near the surface cannot be discussed without accounting for the generation of turbulence by the same process, and the associated vertical mixing. As a result of the consistency claimed by the authors, some contributions might be biased.*

First, it should be noted that we have not neglected the impact of waves and winds on the turbulence and vertical mixing. These contributions are accounted for as described in subsections 3.3.4 and 3.3.5. The only thing that was neglected in our approach was the eventual spread in the drag coefficient associated to various sea states for a given wind speed. Indeed, for the ocean model wind stress computation, as we considered a constant Charnock coefficient, the drag coefficient was parameterized as a function of wind speed only, assuming that the sea state impact on the drag coefficient is only a function of the wind, and therefore neglecting an eventual variation of the drag for various sea states at a given wind speed.

To address the effects of considering wave-influenced wind drag on the computed vertical current profiles, we conducted a supplementary fully-spectral simulation. In this simulation, the wind stress is prescribed from that computed in the wave model (TWOX,TWOY in WW3), therefore accounting for the impact of varying sea states on the drag coefficient. The comparison of the current profiles predicted by this new simulation with those obtained using a wind stress computed with a constant Charnock parameter are shown in Figure 1 for various snapshots discussed in the paper. The impact is weak and will not affect the main outcome of our study. We have added a sentence mentioning this sensitivity experiment in the manuscript.

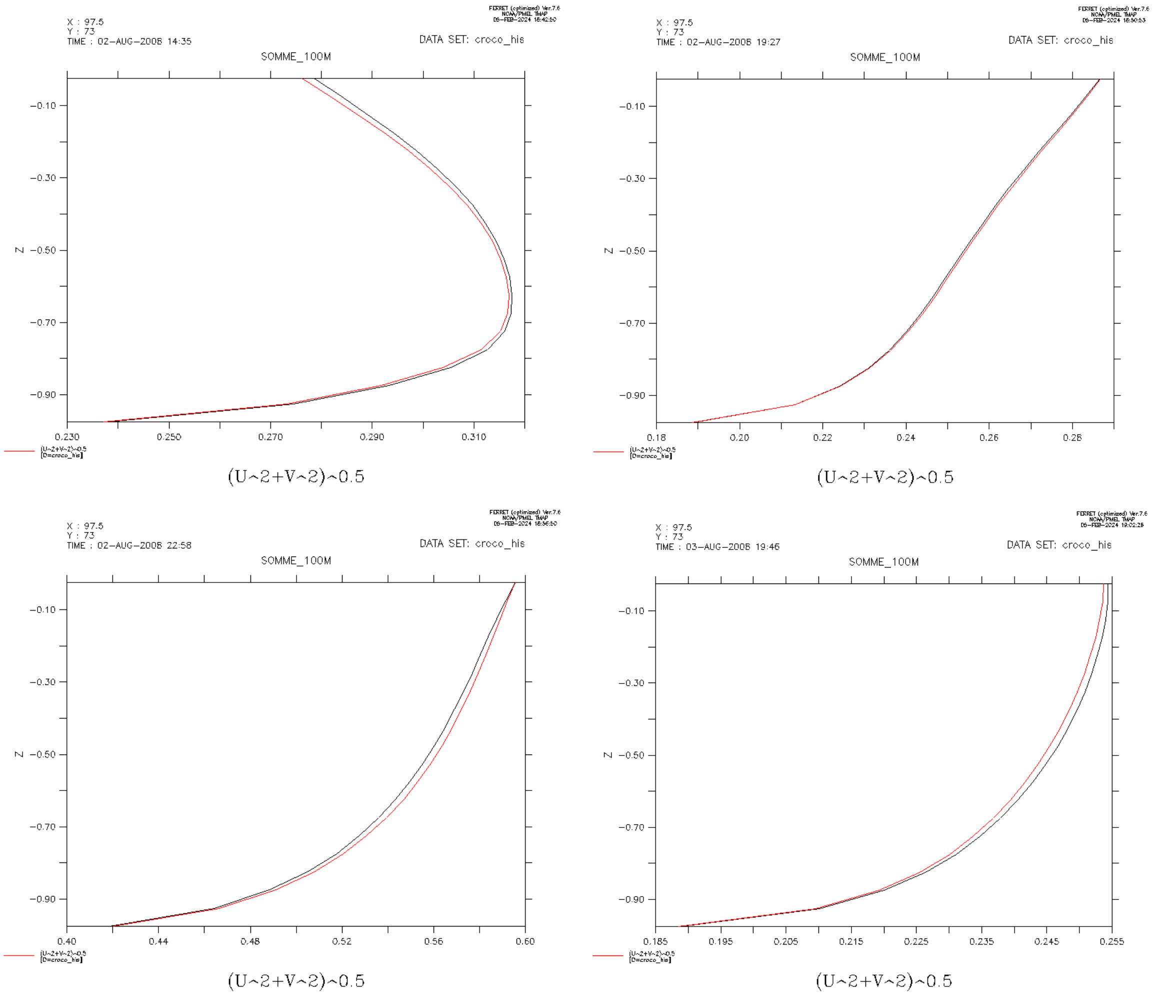

Figure 1: Vertical velocity profiles computed by the original (black line) and new simulation (red line) at the adcp location at time instants used to discuss the main findings of the study, namely 02/08/2008 14:30 (top-left panel, see Figure 11–C), 02/08/2008 18:00 (top-right panel, see Figure 10–C), 02/08/2008 23:00 (bottom-left panel, see Figure 11–D), 03/08/2008 17:30 (bottom-right panel, see Figure 10–D).

2) *The novelty claimed by the authors remains a novelty within the present modelling system (CROCO). This should be stressed more clearly in the manuscript. As opposed to what the authors respond, other modelling systems employing the proposed fully-spectral approach have been applied in regional macrotidal settings.*

In the revised version of the manuscript we have stresses more clearly that the novelty of our work is innovative within the CROCO modelling system.

Comments on the authors' responses:

Response to major comment 1): "While the references cited by the Reviewer describe developments akin to ours, all aimed at achieving a more comprehensive description of wave-current interactions, our study delves into aspects specific to the employed modelling system, setting it apart from the frameworks mentioned by the Reviewer in several key ways. Regarding technical aspects, our coupled model uses different hydrodynamic and wave spectral models than the cited systems. [...]" *I do not agree. All studies I mentioned already compute forcing terms from the full spectrum. This is completely independent from the modelling system, if the equations are the same. My point is: what the authors claim is a novelty in their study, is actually only a novelty within their modelling system since it already exists in others (e.g. SCHISM, MOHID, not fully in ROMS and probably others). "Our work, in line with these recent advances, concentrates on its adaptation for intermediate-water macro-tidal conditions, distinguishing it from the cited modelling frameworks which are more nearshore-oriented." Have the authors actually read the references I provided or just cited them in response? Some actually consider macrotidal environments at the regional scale. A modelling system like SCHISM can nowadays actually be used at the global scale, thanks to the use of unstructured grids.*

In the revised version of the manuscript we have changed the text accordingly.

Response to specific comment 2): "We will explicitly narrow down the study objective from 'modelling wave-current interactions in coastal areas' to 'modelling interactions of macro-tidal currents with winds and waves'. This adjustment will ensure greater clarity and alignment with the primary goals of our research." *No, it does not bring more clarity: macro-tidal currents are also found in shallow water regions. What, I think, the authors mean is that their model configuration is at the "coastal" scale, in a macro-tidal environment and away from the nearshore breaking wave region (justifying to switch off this latter term).*

We agree with the Reviewer's comment and appreciate her/his guidance in enhancing the clarity of our work's objectives. In the revised manuscript we clarify that our main objective is to model macro-tidal currents affected by winds and waves within a coastal scale configuration, forced by tidal atlas along with wind and wave forcing, to investigate wave-current interactions in intermediate water depths, away from the nearshore breaking wave region. The approach will enables us to inform higher-resolution nested models in the nearshore. This refined objective more accurately reflects our research's scope and primary goals.

Response to major comment 3): "To provide a comprehensive context for our study and elucidate the rationale for our choice of modelling application, it is essential to place our research within the framework of its funding project." *I get that funding bodies expect specific deliverables, but how does that justify shortcomings in scientific articles? In my opinion, this answer has nothing to do in the response to the question: why is the selected site relevant to the overall aim of the paper?*

The selected site is relevant to the overall aim of the paper because it provides comprehensive measurements of macro-tidal currents across the water column, alongside free surface wave data. These observations were crucial for validating our modelling, which aims to replicate tidal currents affected by winds and waves within a coastal scale configuration. Our study leverages these measurements to investigate wave-current interactions in intermediate water depths. This focus aligns closely with our objectives, enabling us to utilise the data to not only validate our model but also to understand the impacts of winds and waves on tidal currents in such environments.

Response to major comment 2): “These additional results convincingly demonstrate that the boundary was situated at a sufficient distance from the measurement location where vertical velocity profiles were examined.” *This is quite of a shortcoming. Differences in terms of bottom velocities between new and old model configurations are of the order (if not greater) of the differences obtained with the monochromatic and full-spectral approaches. Thus, it completely seems relevant to keep the boundary away from the measurement site. Furthermore, it is more consistent with the regional scale the authors claim to aim. From the authors response, it is not clear whether or not they kept their original configuration or switch to this extended domain.*

It is important to place our answer in the context of the original comment from the Reviewer: ”Considering the importance given to the vertical shear, how can we be sure that the boundary is taken sufficiently far away from the location where measurements were obtained and vertical velocity profiles discussed? I suspect that the boundary is too close to the measurement stations in order to discuss differences of the order of 1 cm/s at the top layer of the water column.”

In response to the Reviewer’s concern about the model boundary’s proximity to measurement stations, our additional analyses compared the original simulation forced only by tide with an extended model configuration in terms of current profiles at the measuring station. These analyses revealed that differences in vertical shear between both configurations are minimal at those instants at which we extracted and discussed vertical velocity profiles, especially in the top layers of the water column, confirming the adequacy of our boundary placement. While a single vertical profile showed bottom velocity differences (Figure 2) of a similar magnitude to those observed when comparing monochromatic and fully-spectral runs with the original tide-only configuration, these differences diminish rapidly within the water column, becoming negligible near the surface. Also, these differences likely stem from the different interpolation of boundary conditions, both in terms of hydrodynamic forcing associated with the considered tidal constituents and modelled bathymetry, and can be appreciated only at tidal current reversal.

Overall, our analysis indicate that extending the model boundaries further does not improve the model prediction of the observed vertical shear pattern in the mid-water column shown by the ADCP data, which we believe is the main reason for the Reviewer’s original comment. These findings suggest that our model configuration is sufficiently robust to discuss observed vertical shear at the top layer of the water column. Given the study’s focus on efficiently predicting wave-current interactions by means of a coastal scale configuration forced with tidal atlas, expanding the model domain would increase computational costs without yielding substantial improvements in outcomes. Thus, our modelling approach balances accuracy with computational efficiency, aligning with the study’s main objective.

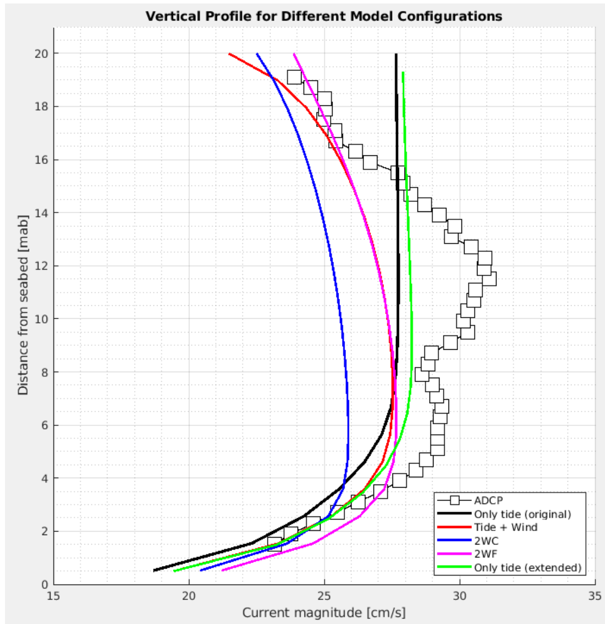

Figure 2: **Vertical profiles of newly computed tidal currents versus ADCP data**. Measured (squares) and modelled (continuous lines) current vertical profiles on 2 August 2008 at 14:30 pm during tidal reversal.

Response to specific comment 12): *My point was that waves generated within a 400 km fetch are not usually considered swell.*

The text has been corrected accordingly.

Response to specific comment 21): *I do not understand the authors' answer. My question, which is extremely relevant for the present work: what is the formulation used for the wind drag coefficient? How is the wind surface stress computed? Short waves as those found in the English Channel will have a strong impact on those quantities. Consequently, the authors choice will affect the wind contribution to mixing and the vertical velocity profiles.*

Please see the response to the first major point.

Response to specific comment 29): *I understand the logic, but I think it is wrong for two reasons: 1) it first means that the wind contribution to changes in vertical velocity profiles is overestimated; 2) the wind stress will not change with the choice of the coupling approach (monochromatic vs fully-spectral), which means that the authors can still perform consistent comparisons, but with better estimates of the effects of waves on the wind stress (and not just "the effect of waves on wind" as written in the revised manuscript).*

Please see the response to the first major point.

Response to specific comment 32): "Also, since the entire paper is focused on the 3D modelling of wave-current interactions, we do not see the point in computing stats associate to depth-averaged values." *Then please discuss the sensitivity of current vertical profiles to turbulence (both TKE and mixing length), including that produced by waves and wind. These will have a major impact on the value at specific height along the water column. For instance, how does the choice for the mixing length parameterisation impact the value of surface currents: does it vary more (or less) compared to the differences between monochromatic and fully-spectral configurations?*

We agree that the interaction between turbulence, wind- and wave-induced mixing is crucial for accurately modelling wave-current interactions. We have indeed accounted for turbulence generated by winds and waves, as detailed and discussed in various part of the manuscript (see response to major point 1). These contributions are treated consistently across both the monochromatic and fully-spectral model configurations, with a focus on discussing the differences between these approaches. While our study does provide a foundation for future work in this area, including a detailed examination of turbulence modelling would require a dedicated sensitivity analysis, which is out of the scope of the present study.

Response to specific comment 36): "It's crucial to emphasise that the primary focus of our paper is to evaluate the added-value of incorporating additional spectral wave terms in the computation of wave-current interactions, rather than concentrating on the details of turbulence modelling in these interactions. We employed a standard turbulence model with standard boundary conditions to ensure a fair and consistent comparison between simulations with and without the spectral terms." *Similar to the waves effect on the wind stress, the two configurations can still be compared with standard practices when dealing with turbulence production and dissipation at the surface within the coastal region. That is including the contribution of winds and short waves in the production of TKE near the surface, which will have a major role in mixing and hence vertical velocity profiles. Again, this is because the two configurations tested here (monochromatic or fully-spectral) will not affect the source of TKE from waves.*

Please see the above response to the previous Reviewer's comment and the that to the first major point.

Specific comments on revised manuscript:

1) *Line 83: "the use of...".*

The text has been correct.

2) *Line 117-118: "This indicates a not fully developed wind sea with energetic swells that develop over 400 km-long fetches.". Please adjust (cf. my comment above).*

The text has been correct.

3) *Some references cited in the text are missing in the revised version of the manuscript.*

The references have been updated.