We would like to thank the reviewer for their careful and constructive review, which helps us refine and improve the manuscript. We copy here comments where we provide a direct answer. Full comments will be addressed in the revised manuscript. This will be provided at the end of the Preprint discussion period.

"1. Validation of the updated ROMS-NCWEC implementation

The first half of the manuscript introduces several modifications and extensions of the original ROMS-NCWEC framework, including updates to the wave-current interaction terms, accounting for wave spectra, and code organization. These developments represent a substantial methodological contribution, although they appear to be largely presented in Romero et al. (2021). Could you clarify what is genuinely new in the present study? Is the manuscript intended to provide a summary of the novel aspects introduced by Romero et al. (2021) compared to Uchiyama et al. (2011)?"

Yes, the present study aims to present the current state of WEC in ROMS including non-conservative effects. It is the result of merging elements of Uchiyama et al. (2010) original WEC implementation in ROMS with the broadband approach of Romero et al. (2021) in the latest UCLA-ROMS, plus additional changes, arising both from the new code infrastructure and from small corrections made to the original implementation. As such, the manuscript recalls what is retained from both Uchiyama et al. (2010) and Romero et al. (2021) while presenting the new changes. These include:

- Now the WEC forces and KPP mixing are computed at both corrector and predictor stages (while Uchiyama et al. (2010) original implementation only computed them at predictor stage).

- We introduce a modified bottom boundary layer thickness criterion accounting for wave streaming (Sec. 4.4).

- We remove a wave-induced diffusivity from the WEC modified tracer equation, because it represents reversible wave-orbital kinematic spreading and its time-derivative form (Eq. 6 in U2010) goes negative whenever the wave amplitude envelope decreases, producing unphysical anti-diffusion in the implicit tracer solver.

- We permit orbital velocities to be read from an offline wave field.

- We introduce the nearshore treatment of a coarse offline wave field (Sec. 6.1)

The new manuscript will present the new changes more clearly.

"Given the extent of these modifications, I expected a more comprehensive validation. The Duck94 experiment is an excellent benchmark and has become a standard reference for wave-averaged coastal circulation models. However, only a relatively limited subset of the

available observations is presented. Earlier studies, such as Uchiyama et al. (2010), devoted significant effort to validating multiple aspects of the wave-driven circulation, including mean current profiles and sensitivity to parameterizations (vertical mixing, CEW, wave roller …). Since the present manuscript introduces new formulations, a more complete validation would considerably strengthen confidence in the updated implementation. “

We fully agree. We now have access to the full Duck94 observational dataset, thanks to Yusuke Uchiyama, allowing us to perform a more complete validation of our implementation, alongside sensitivity experiments to vertical mixing, bottom roughness (for offline wave coupling), and to vertical mixing, CEW and wave roller (for WKB configurations). This will be presented in an extended Sec. 5. For this substantial contribution, we have requested Yusuke to be listed as a co-author.

“It should be noted that Case 8 in the study by Uchiyama et al. (2010) yields one of the poorest solutions (in terms of current profiles) among all the cases presented, largely due to the absence of a "wave roller," which would otherwise transfer a significant portion of the breaking-induced momentum into the trough between the bar and the shore. “

Thank you for pointing this out. We originally wanted to compare to Run 8 as it was the closest experiment we could reproduce with an offline implementation (no CEW), but indeed without the roller. We have now extended the validation section with one offline case using a roller diagnosed from Q_b (run 13, see Table R1 hereafter), but it scores poorly, likely because the roller injects momentum too close to shore. Using the inline WKB solver, we now also compare to U2010 cases with the roller on and off (runs 14-18, see Table R1). In these cases, the full roller consistently improves u by about 25%, consistent with the mechanism described by the reviewer: it delays the momentum release and shifts it into the trough, which mainly shapes the undertow. The alongshore jet is less sensitive: its error is lowest at an intermediate roller fraction (alpha_r=0.5) and slightly larger with the

full roller. Table R1:

Run	U2010	Parameter change	This study u_{err}	This study v_{err}	U2010 u_{err}	U2010 v_{err}
Offline wave forcing (one-way)						
1	–	α_r=0 (no roller), z_0=0.001	0.639	0.139	–	–
2	–	z_0=0.002	0.639	0.168	–	–
3	–	z_0=0.005	0.635	0.319	–	–
4	–	z_0=0.01	0.634	0.435	–	–
5	–	C_b=0.07, z_0=0.001	0.625	0.156	–	–
6	–	C_b=0.05, z_0=0.001	0.636	0.184	–	–
7	–	C_b=0.03, z_0=0.001	0.700	0.223	–	–
8	–	c_b=0.5, z_0=0.001	0.636	0.138	–	–
9	–	c_b=1.0, z_0=0.001	0.673	0.146	–	–
10	–	c_{wd}=1, z_0=0.001	0.638	0.139	–	–
11	–	c_{wd}=5, z_0=0.001	0.640	0.139	–	–
12	–	C_{Ek} off, z_0=0.001	0.639	0.139	–	–
13	–	α_r=1 (roller from diagnostic Q_b), z_0=0.001	0.766	0.516	–	–
WKB inline wave solver, z_0=0.001						
14	Run 1	α_r=1, CEW on	0.555	0.166	0.427	0.092
15	Run 6	α_r=0.5, CEW on	0.619	0.106	0.565	0.111
16	Run 2	α_r=0, CEW on	0.746	0.130	0.756	0.428
17	Run 8	α_r=1, CEW off	0.526	0.160	0.410	0.096
18	–	α_r=0, CEW off	0.725	0.149	–	–
19	–	c_{Kv}=0.6	0.601	0.226	–	–
20	–	c_{Kv}=2.4	0.538	0.188	–	–

Table 1. Duck94 sensitivity experiments. Runs 1–13 use offline wave forcing (COAWST Duck94 test case, one-way); runs 14–20 use the inline WKB wave solver. The reference configuration has breaking-induced mixing coefficient C_b=0.1, breaking eddy-viscosity scale c_{Kv}=1.2, breaking and streaming body-force penetration scales c_b=0.2 and c_{wd}=3.0, and no roller (α_r=0). Errors follow U2010 and Newberger and Allen (2007): $\sqrt{\sum(o-m)^2/\sum o^2}$ over the U2010 observation points (49 for u, 42 for v). The last two columns give the errors of the corresponding U2010 run, recomputed with the same metric from the original U2010 output; they reproduce the published values. For reference, U2010 report u_{err} = 0.41–0.90 and v_{err} = 0.08–0.56 across their experiments, and Newberger and Allen (2007) report 0.45–0.70 and 0.12–0.50.

"Further reading of the article reveals that the validation based on Duck94 data is intended to be purely qualitative; it serves as a precursor to the regional study, which also relies heavily on a qualitative approach to compare dynamic balances between the nearshore and offshore zones."

We agree that the purpose of the Duck94 comparison is to verify that the model recovers the main features of the wave-driven circulation, which is the check needed to have confidence in the physics at play before the regional illustration. In the revised manuscript, this check will also be quantitative: errors are computed with the same metric as U2010 for

all sensitivity experiments (Table R1) and fall within the range of published wave-averaged models.

“2. Scope of the manuscript

The manuscript effectively contains two papers:

* a methodological paper describing an updated ROMS-NCWEC implementation;

* a regional application demonstrating its use.

Each topic is substantial enough to justify publication, but combining them leaves both somewhat underdeveloped. The first part provides only limited validation, while the second focuses largely on qualitative interpretation of a single regional case. The authors should consider either expanding the validation section substantially or, alternatively, better presenting the objectives of the regional application as an illustrative demonstration rather than the primary scientific result.”

Thank you for this important comment. As discussed in response to the reviewer’s first comment, we will provide a more complete validation section. In addition, we also will rephrase the text to better present the objectives of the regional application as an illustrative demonstration.

“3. Novelty claims regarding the regional application

Several statements describing the regional simulation appear stronger than warranted. The manuscript repeatedly emphasizes the 30 m ROMS resolution and describes the application as "surfzone permitting", "capturing both inner shelf and surfzone", or even as a first realistic simulation spanning both regimes.

I believe these statements require more careful qualification.

The circulation model indeed operates at 30 m resolution, but the wave forcing is provided offline by a WW3 simulation with approximately 300 m resolution. Consequently, the nearshore wave transformation is fundamentally limited by the WW3 solution rather than by the ROMS grid itself. Increasing the circulation resolution cannot recover wave processes that are absent from the prescribed forcing.

The regional application provides an interesting demonstration of the updated framework, and many aspects of the simulated circulation appear physically plausible. Nevertheless, the discussions seem to imply a level of realism that exceeds what can reasonably be expected given the wave forcing. For example, statements suggesting that the model

captures the surfzone balance should be moderated if the prescribed offshore wave field does not contain the appropriate depth-induced wave transformation."

Thank you for pointing this out. We will modify the text and remove "surfzone permitting", "capturing both inner shelf and surfzone" or "captured surfzone balance" language. The new text will highlight instead that this range of resolution resolves the inner shelf, and is more cautious about nearshore dynamical interpretation.

"4. Gray zones

On a few occasions, the authors suggest that reducing the grid resolution below 30 m would be problematic due to entering a gray zone for turbulence closure — thus apparently justifying the chosen coarse resolution. I do not share this point of view. The reference to Chen et al. (2025) is completely misleading in this context. In a free surface model of the breaking zone, the turbulence linked to breaking is never resolved; these are the large eddies of the surf zone. All wave-resolving free-surface models (SWASH, CROCO, NHWAVES) successfully employ a RANS-type closure, as they cannot resolve the large eddies directly generated by wave breaking, even though their resolution can sometimes reach a few centimeters."

This is a very good point, and our text was unintentionally misleading. We agree that the turbulence generated by breaking in the surf zone is never resolved, and that free-surface models successfully use RANS-type closures there at any resolution. Our point concerned only a limitation of the KPP parameterization used here. At horizontal resolutions of O(10 m), comparable to or smaller than the boundary layer depth, Langmuir-scale motions in the surface boundary layer start to be partially resolved and KPP may then double-count small-scale motions it is meant to parameterize. This is not meant as a justification of the chosen resolution, but rather as a reminder that replacing KPP with a turbulence closure suited to these resolutions is needed before running ROMS-NCWEC at such resolutions, regardless of proximity to the shore. We will remove this ambiguity from the discussion of the surf-zone resolution.

"5. Discussion of offline wave forcing

The implications of the offline one-way coupling deserve a more explicit discussion.

Unlike fully coupled systems such as COAWST (SWAN) or CROCO (WW3), the present framework does not allow feedback from the evolving circulation to the wave field. Current-induced refraction, modifications of breaking, and water-level feedbacks are therefore absent. This does not diminish the usefulness of the approach, but these limitations should be acknowledged when discussing the applicability of the model and comparing it with two-way coupled systems."

We agree that this deserves a more explicit discussion. Our current implementation is only one-way coupling (unless using the WKB solver which has a CEW option). In the revised manuscript, we will explicitly state the lack of current-induced refraction, of current-modified breaking, and of water-level feedback in Sec. 6.2.

“Minor comments

- New formulations of the WEC terms are presented, but it is not clearly specified which of them—or among the earlier formulations by Uchiyama et al. (2011)—is used for the Duck94 case (Section 5).”

  The revised manuscript will contain a clear identification of each WEC term formulation used for the Duck94 case (Section 5).

  “The discussion regarding Langmuir-type roll structures (Fig. 3a) should clarify whether these are interpreted as physically significant features or as numerical manifestations of unresolved wave-induced turbulence. This is an example of the "gray zone" between resolved and unresolved large eddies — more relevant than the "KPP gray zone" discussed in the context of the surf zone.”

  We believe the roll cells shown in Fig. 3a are under-resolved Langmuir circulations. We know they are under-resolved because their scaling at 30m is not converged but resolution dependent (linear dependence observed under similar forcing at 270, 100, 30 and 10m see Figure hereafter). Yet, they display the dynamical properties of Langmuir circulations (occupy the entire surface mixed layer, large vertical velocity, low density gradient, unskewed vorticity or divergence), and importantly they appear only under strong wave and wind forcings (the case in Fig.3a). KPP here is not augmented to the KPP-LT extension, designed to account for Langmuir turbulence. We believe this is a better choice when the model resolution starts to naturally partially resolve them (to avoid double-counting). Their manifestation, even partial at 30m, is undoubtedly another argument for re-considering the turbulence parameterization in ROMS.

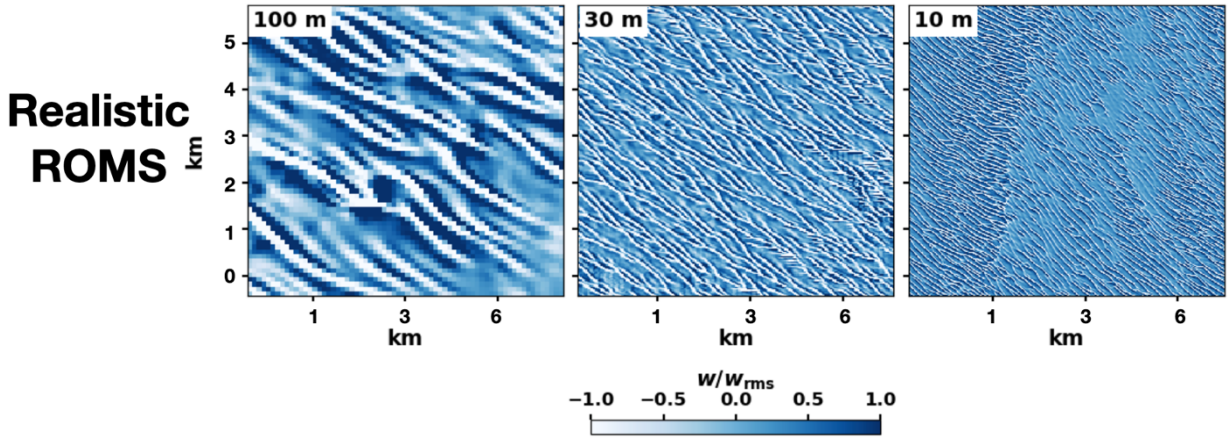
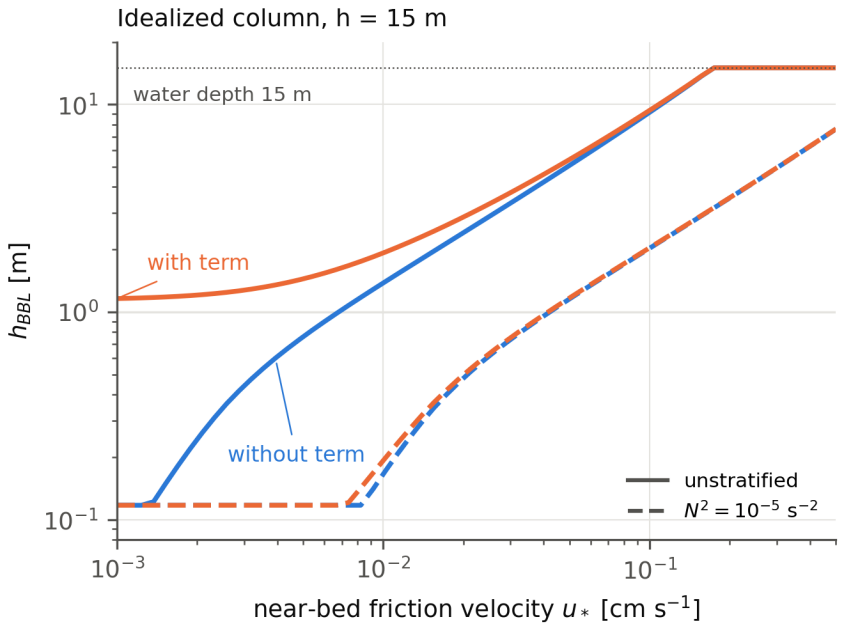


Figure R1: Vertical velocity normalized by its root-mean-square at surface from ROMS runs under strong wind and wave conditions (Lat ≃0.03) at Δx = 100, 30, 10 m respectively in deep waters using 128 sigma levels (Δz ≃1 m in the first 30 m). Only one LC streak spacing appears per resolution and its scale is resolution dependent, with l_LC ≃ 600, 160, 50 m in the respective nests (l_LC ≃1.8 km at Δx = 270 m, not shown).

- "When the effect of streaming on the bottom boundary layer thickness is taken into account, an additional term is added to Cr(z). I may be mistaken, but wouldn't that amount to double-counting the same effect, given that the increase in u at the bottom should naturally increase l_int via du/dz?"

  We thank the reviewer for this legitimate question. The added term does not double count the streaming effect. Once alpha is fixed by the 1 m condition, the streaming friction velocity cancels out: the term reduces to C_ek f^2 delta |z|/(h - delta) (delta = 1 m) and depends only on the Coriolis parameter and the water depth, not on the streaming stress. Streaming therefore affects h_BBL only through the near-bed shear it produces in l_int, as the reviewer describes. The added term is a minimum-thickness regularization. It ensures that, in weakly sheared and unstratified water, the bottom boundary layer spans at least about 1 m, so the bottom stress reaches the levels above the bottom grid cell.

  The figure R2 hereafter shows when the term acts and when it is negligible:



  In an idealized 15 m water column, the term raises h_BBL from 0.12 m to 1.16 m when there is no current. Its effect decreases as the near-bed shear increases, and it vanishes once the shear-driven boundary layer is thicker than a few meters.

Under even weak stratification (N^2 = 1e-5 s^-2), the stratification term dominates and the term changes h_BBL by at most 0.03 m.

In the Pt Arguello simulation, the term changes h_BBL by more than 0.5 m at no more than 1.1% of ocean points considering all depth and at most 0.2% beyond 20 m depth.

- "The argument that the code does not account for lateral diffusion is raised repeatedly. In reality, lateral diffusion is not absent but is handled via upwind-biased advection (likely third-order upwind-biased advection)."

  That is correct. What we mean is no **additional/explicit** horizontal diffusion (via nu_h) is required on top of the implicit numerical diffusion (D_h) associated with the numerical scheme, which is indeed a third-order upwind-biased advection scheme. We will clarify the text, while noting that running without explicit diffusion helps preserve small-scale motions.

All other minor comments are much appreciated and will be addressed in the revised manuscript. This will be provided at the end of the Preprint discussion period.