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Ocean Science Discussions

Interactive comment on "Three-dimensional modelling of wave-induced current from the surf zone to the inner shelf" *by* H. Michaud et al.

Anonymous Referee #2

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Review of "Three-dimensional modelling of wave-induced current from the surf zone to the inner shelf" by Michaud, Marsaleix, Leredde, Estournel, Bourrin, Lyard, Mayet, and Ardhuin

The authors implement wave effects on oceanic and coastal circulations based on the method described in Benneis et al. (2011). The hydrodynamic circulation model SYMPHONIE is thus forced by two operations spectral wave models, SWAN and WAVEWATCH III in such a way that feedback from waves to slowly-evolving currents is not taken into account. The wave forcing is prescribed with a state-of-the-art vortex-force formalism in (quasi) Eulerian frame of reference that has widely been accepted by others (e.g., McWilliams et al, Newberger & Allen, Ardhuin, Uchiyama et al, Benneis et al.) in the last several years as fully three-dimensional oceanic theories and applications

with wave effects. Rigorous assessments of the model are made against a canonical plane beach test case and a laboratory experiment, then the model is applied to the real coastal situation to be compared with extensive field data. In general the manuscript is well written, and sufficient materials are presented to convince readers that the present approach might be usable to further coastal applications. However I feel that the manuscript has several deficiencies in multiple aspects as specified below that will require moderate revisions before I can recommend publication of this article in Ocean Sciences Discussion.

1. General comments:

p.2423, l. 15 Vertically-sheared condition leads to non-trivial higher-order Bernoulli head (BH) effect on momentum balance (Fig.13 in UMS10) while the leading-order BH (del. J term) is considered here. UMS10 suggested the higher-order BH could exceed the leading-order BH in realistic settings. The modeled flow fields in the plane beach test and the synoptic cases clearly indicates strong vertical shear, not only in Eulerian current but also perhaps in Stokes drift, which should be causing the higher-order BH. The authors need to provide an appropriate comment to defend the approach taken here where the higher-order BH is neglected.

p.2433, ll. 12-13 & Fig. 1b The depth-averaged cross-shore Eulerian velocity in this case must be strictly equal to the depth-averaged anti-Stokes flow because of the mass balance. Even the worst model (HW09) can well reproduce the cross-shore velocity profile. I am skeptical about the correctness of the implementation since such an error could be readily ported in the barotropic-baroclinic coupling part if it exists in the code, or elsewhere. This must be addressed.

p.2434, ll. 18-20 This interpretation is wrong. In steady, alongshore-uniform cases, vortex force (VF) is compensated by Eulerian advection (e.g., Uchiyama et al., 2009, JGR). Throughout the manuscript, the authors treat VF as if it is an external forcing. However VF is an adiabatic (conservative) term which intrinsically represents an interaction between wave Stokes drift and relative vorticity of mean current, and is originated from the advection terms of the primitive equation. The manuscript must be revised properly with this regard.

p.2435, ll. 6-8 This statement is wrong. The cross-shore momentum input associated with depth-induced breaking leads to competing opposite pressure gradient force that is achieved by so-called wave set-up. The authors have to describe the surf-zone dynamics much more accurately.

Fig. 3 The definition of the surface momentum flux T_s is missing. I presume that T_s stands for the breaker acceleration related to \tau_wo. If so, its cross-shore and alongshore components would be incorrect because the shapes looks quite different from the one found in Fig. 7 in UMS10. The peak location of \epsilon_b should be more seaward, and so is T_s. Please explain why.

p.2437, ll. 7-8 This was originally analyzed by Yu and Slinn (2003, JGR). Their work must be cited.

p.2443, ll. 17-20 Does this statement mean the horizontal spacing of the grids varies from 8 m at the shore to 180 m at the offshore boundary? It can also read that the grid is refined only near the river mouth. The strategy here would be to resolve the surf-zone as fine as possible, so I would guess the former approach was taken, but it is quite ambiguous. Please be explicit. It may be a good idea to show the grid-cell layout of the inner-most one. Besides, the spatial resolution and error statistics of the LiDAR data must be addressed for the fairness of refining the grid near the shore down to dx = 8 m.

p.2445, ll. 11-14 The shear-wave argument seems to be irrelevant here. Of course there could be shear waves, but it is known to be rather rare in realistic situations. Newberger and Allen (2007, JGR) implied the three-dimensional model yields much less distinct shear waves. To justify the statement here, the authors needs to represent a wavenumber-frequency spectrum to identify the propagating signals of shear waves.

Fig. 17 Both the panels are seemingly suggesting vertical mixing is poorly reproduced since the top layer in, say, z > -0.5 m is too thin with large surface velocity driven by breaker momentum. The more the surface stress is induced by wind and wave, the deeper the mixed layer should be formed owing to the mixing through enhanced vertical eddy viscosity (K_v) in such a highly sheared velocity field. The author should check validity of K_v more carefully, as I suggested in the plane beach test case. Moreover, spotty bluish surface u velocity in the lower panel looks unrealistic over the reralatively smooth topography. Please address why and make an acceptable interpretation on this.

pp.2447, ll. 13-20 I do not understand why increased wind by a factor of 1.2 could reproduce the inner-shelf velocity |u| well to increase it to the observed order of magnitude. Looking at the lower-right panel of Fig. 13, the modeled |u| is merely less than 50 % of the observed one. How does the factor 1.2 fill this big gap? Why doesn't the modified wind enhance wave field that is responsible for surf-zone currents? Please explain.

Throughout the manuscript The authors tend to use "good agreement" and "very good agreement" whereas I observed all the results presented here are not surprisingly better than the previously-developed three-dimensional circulation models with wave forcing, in terms of model skill. Please use more appropriate words for those expressions (e.g., fair, comparable, reasonable, etc...).

2. Specific comments

p.2426, l.16 If \tau_aw is due to neither wind stress nor white caps, what mechanism is expected to cause this stress?

Fig.2 The distribution of v highly depends on vertical eddy viscosity (K_v) as in UMS10. The authors should better present an example of K_v, perhaps in this figure.

Fig. 6 Both the experiment and the SHORECIRC model suggest a larger mean rip current near the channel at around y = 13 m. Why does the present model produce the symmetric pattern showed in panel c?

Fig. 16 How the flow looks like if WEC is turned off? The meandering flow patterns could also be attributed to interaction of the complex nearshore topography and alongshore pressure gradient induced by the parent grid solution even without waves. Please assure if these recirculation patterns are totally wave-driven.

Interactive comment on Ocean Sci. Discuss., 8, 2417, 2011.