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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.

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Dear Anonymous Referee, First of all, thank you very much that you have taken the time to read and correct our article. We try in the following document to answer on all your notes and recommendations, and to correct our model and paper accordingly.

1. **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.

In fact, you are right, we cannot neglect these terms since currents in the surf zone are strongly vertically sheared. We have added in the model for the revised version of the paper, the shear-induced term given in equation (40) of Ardhuin et al. (2008). In the plane beach test case, this term has a very low effect as we can notice in the momentum balance that is shown in the revised paper. But in the realistic case, this term is more important.

2. **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.**

   The mistake has been corrected. Please see the answer to the question 4 in the reply to the referee 1.

3. **p.2434, ll. 18-20 This interpretation is wrong. In steady, alongshore-uniform cases, votex 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.**

   In the revised version of the paper, thanks to your comment, we have represented in figure 3, all the momemtum terms of the equations (2) and (3), including advection, pressure gradient, vertical mixing force. In figure 3, we actually present the vortex force and the advection terms as defined by Bennis et al. (2011) (equations (2) and (3) in our paper). This figure shows that the vortex force is compensated by the Eulerian advection.

4. **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.**

   We have revised this section.

5. **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 τ_{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 ϵ_b should be more seaward, and so is T_s. Please explain why.**

   Yes you are right, T_s is the surface stress and stands for the breaker acceleration related to τ_{wo}, and as you have noticed, an error was done in the representation of this term. When we have corrected it, we have noticed that its shape was similar to τ_{wo}. To improve the representation of the breaker acceleration near the surface, we have now added a vertical distribution function to this force, which is no longer a surface stress. The vertical distribution function is the same as the one described by Uchiyama et al. (2010) in equation 53 type III. So this force is now represented in the momentum balance, instead of the surface stress. Besides, we have also displayed the bottom stress $-T^b$, in this figure for the revised version. This stress is part of the vertical mixing force.

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

   We have added this citation at this position.

7. **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.**

   The grid is refined only near the mouth, since we use a curvilinear grid. The cell layout ot this curvilinear grid is shown in Figure 8. The spatial resolution of the LIDAR data is 5 m so it is not inconsistent to refine the grid to 8 m.

8. **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.**

   We have removed this sentence. Recirculations, meanders and rip currents are generated by the bathymetry and also because waves are frontal as we have previously noticed in section 3.2.

9. **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.

With the 'k-ϵ' turbulence scheme (that has been suggested by the other referee), and with an addition of a vertical distribution function of the breaker acceleration as suggested by Uchiyama et al. (2010) (equation 53 type III in their paper), the vertical mixing is less sheared and results for the currents are more correct.

10. **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.**

    This point is also raised by the referee 1 at question 12, so my answer to this question will be very similar to the other one.

    We see that the high waves associated to the storm, are able to induce strong current over depth lower than 15 m while beyond this region, their effects are low and even seem to induce a decrease of the current. As the strong observed current is limited to the period of the storm, the strong underestimation of the simulated current over the whole water column should likely be an underestimation of the wind intensity or possibly of the wind event duration. The second hypothesis does not seem realistic when considering the general agreement of the characteristics of the observed and simulated wind and wave time series at SODAT and POEM. Furthermore, the wind underestimation could be confirmed by the underestimation of the wave height. Indeed some authors have pointed out that the largest source of errors in a wave model is due to the wind (Ardhuin et

al., 2007). More precisely, applying a wave model has been suggested efficient to assess the quality of wind data (e.g. Bauer et al. (1992)). We hypothesise that the wind structure could be poorly resolved by meteorological models resulting in a smoothing of local maximum. As the meteorological station of Toreille is not located in this wind structure, this hypothesis is not contradictory with the fact that the comparison between observation there and the Aladin model does not indicate such an underestimation. Finally, satellite wind data have been examined to find evidences of this underestimation but the absence of valid data near the coast did not allow to draw a conclusion. It was then decided to do a sensitivity test to the wind intensity. As the meteorological situation is spatially and temporally complex, the objective is not to determine the wind intensity through an adjustment of the simulated current to the observation. We rather expect to understand how the current is sensitive to wind intensity in coastal regions where the processes are more complex than in the open ocean. A crude increase by a factor 1.2 allowed to well reproduce the observed current in the inner shelf, and improved the results in the wave model. In fact the main process responsible of the strong currents in the inner-shelf seems likely to be the pressure induced coastal jet due to the alongshore wind. All of these facts lead us to think that the wind speed is underestimated at the storm apex in our atmospheric model. This discrepancy in current between model and observation, during storm at coastal scales is the focus of the study of Michaud et al. (2012), submitted to the Comptes Rendus de l'Académie des Sciences.

11. **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...).** We have modified these terms:

- Our surface elevation agrees with both... section 3.1.1
- Intensities of depth-integrated currents are comparable to the data... section 3.2
- In conclusion, this simulation is reasonable compared to the observations of ... section 3.2
- Orientation and intensity are comparable to the data... see section 4.2.3.

12. **p.2426, l.16 If τ_{aw} is due to neither wind stress nor white caps, what mechanism is expected to cause this stress?** Actually, τ_{aw} is a wind stress not to currents but to waves corresponding to the flux of momentum from atmosphere to waves and τ_{wo} is the release of wave momentum to the currents due to breaking, interactions with turbulence or viscous effects. τ_a is the flux of momentum from atmosphere to currents.

13. **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.**

    We have added a representation of K_v in Figure 2.

14. **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?**

    Actually, experiments and SHORECIRC model suggest that the rip current near the channel is equal to 0.25 $m.s^{-1}$. We obtain the same value. Theoretically, as I have pointed out in my reply to the referee 1, the longshore velocities should be near-zero in the channel because of the symmetric nature of the bathymetry and incident waves (Xie, 2011). However, in the experiment the rip current has unstable features and a trivial perturbation (like an interaction with the Stokes drift) could lead to a deflection of the current direction and create instabilities(e.g.,

Haller et al., 2002). In our case, in the revised paper (we choose a drag coefficient equal to 0.0015, instead of the value of the submitted paper, 0.005), the two rip currents oscillate together as in the observations. So we take a temporal average and obtain a new figure 6. The symmetric nature of the bathymetry and incident waves allow then to obtain the symmetric pattern of the current.

15. **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.**

    Results at the three instruments without WEC are visible at Fig. 13 and Fig. 14. The littoral drift and the rip current are not generated at the nearshore scale.

Besides I would like to draw the attention of the reviewers on the fact that we have modified the boundary conditions close to the bottom (section 2.1.2). In fact, instead of using the equation (22) of Bennis et al. (2011), which sets that the horizontal velocity is prescribed as velocity at the bottom given by the streaming solution (Longuet-Higgins, 1953), we have prefered to use another solution. We have added the momentum lost by waves due to bottom friction $\vec{\tau_{wob}}$ in the bottom boundary condition of the momentum equation.

$$\begin{cases} K_z \frac{\partial \hat{u}}{\partial z}|_{z=-h} = \tau_{bot,x} + \tau_{wob,x} \\ K_z \frac{\partial \hat{v}}{\partial z}|_{z=-h} = \tau_{bot,y} + \tau_{wob,y} \end{cases} \tag{1}$$

$\vec{\tau_{bot}} = (\tau_{bot,x}, \tau_{bot,y})$ is the bottom stress linked to current. The momentum lost by waves due to bottom friction is given by:

$$\vec{\tau_{wob}} = \frac{\epsilon^{wd}\vec{k}}{\sigma} \tag{2}$$

with ϵ^{wd} the wave bottom drag calculated using the parameterization of Reniers et al. (2004): $\epsilon^{wd} = \frac{1}{2\sqrt{\pi}}\rho f_w |u_{orb}^{\vec{w}}|^3$. This solution is more consistent with the parameterization of the boundary condition at the surface, and has been already used by Uchiyama et al. (2010).

I agree on all comments and revise the manuscript accordingly.

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