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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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Reply to Anonymous Referee 1:

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. **The presentation of the momentum equations is in two forms. First, a non-conservative form that cleanly separates out the vortex force (VF) terms, and secondly a conservative form that is more consistent for numerical implementation but does not clearly separate the VF terms. For future use, if a momentum balance is performed it needs to be clearly identified if the**

balance is using the specific forcing terms or the conservative approach.

   We explain in a new appendix of the revised paper (appendix A), the calculation of our equations (equations (6) and (7)) from the ones of Bennis et al. (2011) (equations (2) and (3) in our paper). When we present in the test case of the beach the momentum balance (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). As far as the analysis of the momentum balance is concerned, we assumed that the readers would be much more familiar with the usual widespread formulation, that is the vortex and advection terms of Bennis et al. (2011).

2. **Turbulence closure ? is rather weak approach these days. Suggest developers code in a two equation model or link to GOTM. It has been shown in relatively recent literature that these 1.5 level models are not correctly characterizing the turbulence length scales.**

   Thank you for this remark, that we have taken into account. Now we have implemented the 'k-ϵ' turbulent closure scheme in our model (see section 2.1.3 in the revised paper), and we have performed the simulations using this approach.

3. **The modeling system is characterized as using a new version of WW3. Is WW3 able to be redistributed openly? If not, why was this wave model chosen?**

   Our numerical wave model is the WAVEWATCH III(R) code version 4.04 which will be redistributed openly in the next few months. This version of WW3 was chosen because it allows the use of unstructured grids. Besides, it includes parameterizations of the TEST405, described in Ardhuin et al. (2010), that allow the extension of the model validity to nearshore scales..

4. **Figure 1b- why does the cross shore depth-averaged velocity fall off quicker than the analytical solution?**

Thank you for this comment which has revealed a discrepancy between our model and the others, in our formulations of the stokes velocities. In the revised paper, we have taken the same formulation than Uchiyama et al. (2010), and we have also checked that our barotropic cross-shore velocity is equal to our cross-shore stokes velocity with a -1 factor, which is also the analytical solution (u=-ustokes, eq 66 in Uchiyama et al. (2010)). We thus obtain the same cross shore depth-averaged velocity than the others.

5. **p. 2433 lines 16: should this be ?as shown in section 3.1.2? not 4.1.2?**

   Thank you for this remark, in fact you are right, I have done a mistake.

6. **Figure 3: not very obvious what is being shown here. Can we see the important terms of the momentum that actually balance?**

   In the revised paper, I have taken into account your remark and I have represented all the momemtum terms of the equations (2) and (3), including advection, pressure gradient, vertical mixing force, instead of only the different wave forcings.

7. **section 3.1.2 ? maybe you need a better turbulence closure model.**

   Please read the answer to question 2)

8. **p.2437 line 18, rip current test case ? It seems that you really cranked up the bottom friction term here. Are the rips oscillatory, or heavily stationary? They can still oscillate, as that represents correct physics, and then take an average.**

   Theoretically, the longshore velocities should be near-zero in the channel because of the symmetric nature of the bathymetry and incident waves (Xie, 2011). However, the observed longshore velocities are scattered. In fact, 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 instabilities, that has been observed in a series of papers (e.g., Haller et al. (2002)). You were right, the drag coefficient was too strong, and the rip currents were heavily stationnary. In the simulation of the revised paper, we choose a lower bottom roughness length (a drag coefficient equal to 0.0015, instead of the value of the submitted paper, 0.005). In this case, the two rip currents oscillate together as in the observations. So we take a temporal average as you have suggested. We have thus modified the text and the results in the revised version of the paper.

9. **p. 2438 line 15-18: These test cases were for very shallow flows. May not be adequate for deeper water applications.**

   The two test cases deals with littoral scales, where the wave action is the more intense. In fact, there is no test case at the inner-shelf scale. It would have been interesting to do a test at the inner-shelf scale, but such a test does not exist. However, wave action is no more the predominant forcing, and previous studies focused on coastal scale, have shown the validity of our model (e.g., Ulses (2005), Estournel et al. (2003, 2005)). So the study considering the real case at these two scales, can be tackled with some confidence.

10. **Figure 14: difficult to really see the comparison. maybe the figure needs to be larger.**

    I have increased the size of this figure.

11. **Figure 13: text says on line 1-2 p. 2447 that the currents are stronger with the wave forcing than without. But the right middle panel has weaker currents with the wave forcing.**

    I clarify my comments in the revised form and distinguish the results at the surface and on the bottom. In fact, at the bottom, current are weaker with the wave forcing and especially at POEM and in the second part of the storm.

12. **the discrepancy is suggested to be caused by the (p. 2447 line 14) "underestimation of the wind speed." But text on p. 2443 line 7 says "... the Aladin model is in reasonable agreement with the data ..." Please clarify. Also, the figure 10 for wind speed does not show an underestimation of the wind speed.**

    I have to clarify my comments. I should say on p2443 line 7, that the comparison is made between our simulated results at sea with a meteorological station. In fact, 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 Toreilles which is on land, is not located in this strong 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. 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 Comptes Rendus Geosciences.

13. **p. 2448 line 15 " a perspective of this study could be to fully couple wave and circulation models..." So was this simulation fully coupled?**

    In fact in this paper, we have taken into account the wave forcing in the circulation model. Yet, we do not have taken into account the current forcing in the wave model. This will have required, for the realistic case, the use of a coupler (e.g. OpenPALM). This will be one of the future development of our numerical platform.

14. **Figure 13: text could provide some guidance as to what processes are driving these large currents. In the surf zone, is it predominately the Bernoulli head or wave breaking dissipation forces?**

    This is discussed more deeply in the revised paper.

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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