



Interactive  
Comment

## ***Interactive comment on “Hydro morphodynamic modelling in Mediterranean storms: errors and uncertainties under sharp gradients” by A. Sánchez-Arcilla et al.***

**A. Sánchez-Arcilla et al.**

agustin.arcilla@upc.edu

Received and published: 12 March 2014

In answer to the anonymous referee #2 comments, we have several remarks to make. First of all we thank him for going through the manuscript in such a short time and providing input for improvement. Leaving aside the English that can of course be easily corrected our main points regarding scientific contents are summarized in what follows:

- The anonymous reviewer noted: “In this paper the authors consider an intense storm on the Catalan coast, make a simulation with the SWAN model comparing the output with buoy data, run a morphodynamic simulation using as input the SWAN results, and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



compare the output with LIDAR images.” The authors believe that it is important to stress that we have carried out a regional analysis of the wave field based on satellite images. This has required adapting the spatial routines to carry out the comparison to deal with sharp gradients such as we can find in the Northeast part of the Spanish coast (North Western Mediterranean). This allows comparing regional with local errors. - The anonymous reviewer also noted: “Filtering the verbose text, the results are poor and there is an extensive discussion on the reasons for the poor results (poor notwithstanding the claims) that leads to the conclusion that better and more complete data are required”. The authors believe that there may have been a misunderstanding of the “poor” term. If that is applied to the quality of the fitness, the authors disagree with this statement and would suggest a rereading by the reviewer of chapters 3 and 5. More precisely page 1701 (where a reference to Holt et al. 2005 qualified deep water results is included) where the data are compared with satellite images. In the following paragraph there is now a further explanation of the quality of the predictions (fitness) using buoy data, which can also be seen in Figure 2). For the sake of clarity we have taken out bold statements such as “only” (page 1703), etc, so as to convey better the message. - The anonymous reviewer noted in his review: “This is already a poor start because all the considerations done in the paper are standard stuff and there is nothing new provided with the paper”. We assume the reviewer refers to “standard stuff” instead of “staff”. In that case, we certainly agree that the SWAN and morpho-dynamic models selected are state of the art. However, their application to evaluate high resolution (local or regional scales) errors and uncertainties within a forecasting framework has been going on and it is still an active research topic for a wide range of scales. More specifically for the Spanish Mediterranean coast this kind of analysis has not been performed before to the best of our knowledge. - The anonymous reviewer also noted: “However, the whole is much worse than this because the fragile background the study starts from was already a sure condition of failure. The methodology and the derived conclusions are far fetched and well beyond what can be concluded from a single one week exercise.” The authors disagree with such statement since our

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



main purpose was to provide a first quantitative estimation of the error transmission chain, distinguishing between the error “that we know” and the uncertainty “that we do not know”. More specifically we wanted to illustrate the process during an energetically extreme event that affected almost eight hundred kilometres of coast and produced a morphodynamic impact comparable to that happening during a full year. The main message, which was probably not clearly stated in the text, was to show how the quality of the fit under a few energetic events is responsible for a large part of the fit through the year. We agree with the reviewer’s point that a more extensive analysis should be done using different extreme event conditions. Nevertheless, the availability of information before and after a storm is not so easily available and finding a time interval with a co-existence of hydrodynamic and morphodynamic variables, (in-situ data and satellite images) plus LIDAR data available and with significant damages is not so readily found.

- The anonymous reviewer noted also: “At the end of the day this is a single case study on the Catalan coast. After an accurate reading I reached the conclusion that this does not teach anything to anyone. The results are poor and I see no reason why this paper should be published.” Since this is a subjective statement reflecting the reviewer’s view we of course note the remark but will not enter more into it.

- When talking about depth gradients the anonymous reviewer mentioned: “Talking about depth gradients, 600m depth in 60 Km is a 1% average slope, not so much.” Here we agree with the reviewer’s point and we have rewritten the paragraph that now reads: “The Catalan sea shores feature a continental shelf between 0 and 200 meters depth and an average slope below 1.5°. The width of this shelf is highly variable, going from a few kilometers in the Northern part (about 5km) to almost 100km south of the Ebro Delta”. We believe such an statement reflects better the spirit of the description.

- The anonymous reviewer states that: “There are not so many deltas on the Catalan coast”. The authors, although recognising that “many” is a subjective statement, tend to disagree because we can identify three big deltas (compared to the spatial scales to the Catalan coast) in our area: the Ebro, the Llobregat and the Tordera deltas. There are also a larger number of smaller deltaic features corresponding to smaller river or streams.

- The anonymous

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



reviewer states that: “Calibrating the results is not a scientific practise”. The authors disagree with this statement since calibration without a physical basis may indeed be considered outside the scientific realm. However, when dealing with large errors and uncertainties such as found in the meteo-hydro-morphodynamics of this part of the Mediterranean, calibration is indeed needed and the analysis of the poor fit between simulations and observations, if based on physical principles, can indeed lead to a better understanding of the underlying processes and therefore an improvement in the simulations. - The anonymous reviewer states: “Too much emphasis on the Catalan coast. Incidentally, between Pyrenees and Alps there is also the Massif Central”. The authors indeed agree with the reviewer that between the Pyrenees and the Alps there is the Massif Central. Leaving that aside the main reason for analysing the Catalan coast is the presence of sharp gradients in meteo-hydrodynamics, the existence of errors in wave and corresponding uncertainties in the morphodynamic simulations. This for a coastal stretch with a high pressure of use and therefore where accurate predictions are of high economic and social significance. - The anonymous reviewer states: “I wonder if there is a double peak spectrum, from NW and E, 50% of the time”. The authors want to stress here that this has been reported before by the authors and in other scientific papers (e.g. see Bolaños, R. and Sanchez-Arcilla, A. (2006). A note on near shore wave features: Implications for wave generation. Progress In Oceanography, Volume 70, Issues 2-4, pp. 168-180). However the statement should never be understood as granting that 50% of the recorded spectra are bi-modal. It is just to indicate that about half of the sea states correspond to crossing wave trains. For that reason we have re-written the paragraph on page 1700. - The anonymous reviewer states: “SWAN is not a hydrodynamical model”. The authors of course agree and have corrected the sentence. - The anonymous reviewer remarks that: “The description of the standard physics of SWAN is not needed”. The authors agree with that and just want to add that the description of the standard physics of SWAN was never intended. We have only included in the paper the physical parameterization that was selected for the SWAN simulations and we considered that this information is useful in order to

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



properly assess the quality of the simulations. This is because SWAN, as many other open source codes nowadays, cannot be interpreted as a single, fixed model. The selection of closures, parameterizations, etc, may lead to indeed different modelling tools and results. - The anonymous reviewer states: “The global bathymetries are known for not being correct close to coasts”. The authors certainly agree that this is an important limitation. Because of that they have combined local bathymetry (where available) with the larger scale charts. However the requirement of our high resolution simulations is 1km and we thought it was important to show how the horizontal mesh size can become part of the uncertainty. We have added a sentence to the text in that spirit. - The anonymous reviewer asks: “What is meant with “differences between the input and dissipation terms were relevant”? It seems meaningless to me. The authors want to stress in here that wave generation models are based on an energy balance equation (more specifically wave action) where the important element for the simulations is the budget between input and output, i.e. wind momentum transferred to the sea and wave energy dissipation. The authors have rewritten the paragraph stating that “the wave field prediction is based on third generation wave models specifically, on the action balance where the differences between input and dissipation terms represent the physical net action accumulation and is therefore critical for the computations. - The anonymous reviewer states that: “Such a detailed simulation cannot be done with daily average current input and wind input at 6h interval. The authors certainly agree that daily averaged currents and a wind input at 6h may not be enough for the high resolution pursued. However, the authors and other teams have carried out simulations with higher spatial and temporal resolutions finding that the enhanced turbulence introduced into the computations does not always lead to an improved wave field (Alomar et al, 2010. Wave growth under variable wind conditions. Proceedings of the 32st International Conference on Coastal Engineering, Shanghai, China (June 30th – July 5th). ASCE. Paper n°78). Because of that we have now introduced a sentence that says “the time scale of waves and currents is certainly different. For circulation an average time step of 1 day can be considered enough since our highest resolution aim

[Interactive  
Comment](#)

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



would be the mesoscale eddies with average life spans of 7 to 14 days. However for the wave field it is necessary to use a time step for the meteorological input commensurate with the storm duration in this part of the Mediterranean. Considering that the average storms last about 24h we have selected 6h for refreshing the wind field. Former simulations carried out with a 3h interval have not shown improved wave results (Alomar, M. et al, 2010. Wave growth under variable wind conditions. Proceedings of the 32st International Conference on Coastal Engineering, Shangai, China (June 30th – July 5th). ASCE. Paper n°78). - The anonymous reviewer states that: “It is not possible to extract any possible meaningful conclusion from one week simulation. Incidentally two days warming-up is too little for a basin like the whole Mediterranean Sea”. The authors certainly agree that for the whole Mediterranean Sea the warming-up period should be larger. However in the morphodynamic analysis we have never run the whole Mediterranean Sea. The Italian peninsula and the coast of Africa allow to start the simulations close to Sicily and the presence of the French and Spanish islands in the middle of the West Mediterranean basin also contribute to enhance the relative weight of these more locally generated waves. In our simulations we have seen how after the warm-up time, the error was stable both for moderate and extreme wave conditions. We have because of that added a sentence to the paper better explaining the situation. - The anonymous reviewer states that: “The white-capping dissipation coefficient normally used to balance wind input (...). I find this absolutely meaningless”. The authors have rewritten the sentence because they want to convey a clear and meaningful message about the importance of fitting parameters in wave predictions for semi-restricted domains such as the North Western Mediterranean. The more uncertain term in the wave action balance is the white-capping dissipation coefficient, particularly when there is a co-existence of younger and older sea waves with some pre-existing swell outside the conventional range of swell parameters (due to the small fetches in the Western Mediterranean). Because of that we still believe that the dissipation term in the balance is one of the weakest links and that is why it has been often used as a fit term for the simulations, in order to reduce the mismatch between

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



computations and observations. - The anonymous reviewer states that: “There is a lot of talking about obvious things, as the effect of groins, that waves produce a coastal current, that coastal waves depend on the ones offshore. All this is useless and has to be taken for granted”. The authors main aim was to refresh the basic concepts for specialists from more circulation-related areas and to emphasize the main coastal processes that are responsible for the large mismatch between morphodynamic observations and computations at local scales. We still consider that this information can be useful but have rewritten the paragraph putting more emphasis on their contribution to uncertainties. - The anonymous reviewer states that: “LIDAR images are OK, but if we do not have the bottom profile before and after the storm, there is nothing conclusive we can say about the sediment transport”. The authors want to remind the reviewer that there is information on the emerged part of the profile before and after the storm. However the submerged part of the beach was not available precisely before and after the storm. Because of that we have assessed the discrepancies between observations and computations in terms of the emerged part of the profile, stating now more clearly than it was before that this is a partial measure of the error, describing only the emerged part of the beach not because we believe that is the only component in the balance but because the only reliable data before and after correspond to that part of the profile. However we believe that this component of the quality of the fit is an important one since most of the coastal erosion conflicts came from the emerged beach and also because in Mediterranean coasts the inter-play between emerged and submerged beaches typical of California or Australia and reported in many text books does not verify due to the small energetic content of swell wave trains. - The anonymous reviewer states that: “There is no stationary assumption in wave modelling. How was SWAN run?”. The authors have improved this statement in the text and explained how SWAN was run in a non-stationary mode where the initial condition was provided by a JONSWAP (uni-modal) wave spectrum, with parameters derived from the initial wind field.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

