**Anonymous Referee #1**

The present paper describes the effect of a coupled ocean-atmosphere-wave modeling system on the simulation of a wind jet in Ebro river shelf. The results of the study are interesting and suitable for publication in Nonlinear Processes in Geophysics; however, some improvements in the analysis and in the presentation of the results are needed. Also, English language is poor and requires a deep revision.

The authors acknowledge the helpful comments and corrections of the reviewer #1 that helped to improve the quality of the paper. The English have been improved in several parts of the manuscript. Besides, the manuscript has been corrected by a native English speaker (Kevin Callon).

Minor points:
It is not the task of the Reviewer to make a grammar revision of the text, however some points are addressed here:

- P2L26-27: rephrase in this way “these regions are preferential sites for the installation of offshore wind farms (Nunalee and Basu, 2013). In case of coastal regions, the resultant : : :”

Ok, done.

- P2L29: “Despite the relatively limited : : :”

Ok, changes included.

- P3L9: “allowed the : : :”

Ok, included.

- P4L1: “in an orographically complex region”

Modified.

- P4L2-3: “the feedback relative to the air-sea momentum transfer : : :”

OK, manuscript modified.

- P4L8: “comparing them with : : :”

OK, included.

- P5L20: “for the assessment of offshore wind energy potential, : : :”

OK, modified.

- P6L27: “: : :large enough to : : :”

Ok, modified.

- P7L29, P16L22: what do you mean with “typical value for rapidly seas”?

We deleted this statement because is not clear. Only we provide a “typical value”.

- P8L16: “remain strong : : :”

Corrected.

- P11L2: “joint occurrence: : :”

OK, thanks.

- P11L27: “who note : : :”

Corrected, Thanks.

- P12L27: “As a consequence: : :”

Corrected.

- P12L32-33: “: : : due to the spatial wind variability : : :”

Ok, corrected.

- P13L6: “in the region”

Modified.

- P13L20-21: “due to the increasing waveinduced ocean bottom roughness”

OK, corrected.

- P14L15: “persistence : : :”

OK. Modified.

- P22L2: “: : : the mesh name in Fig. 1 is shown”

OK. Corrected. Thanks.

The analysis of the results should be improved:
- P8L29-30: This is true for the high wind speed. For example the mode and the low wind speed regime is reproduced worse in COAWST runs.

We agree. Thanks for the appreciation. We modified the manuscript specifying that the better agreement occurs for middle range and high wind speed (Lxxx-Lxxx). Our model is worse for low wind speeds (<2.5 m/s).

- P9L2: here and elsewhere, I understand you use a reference run: which is the implementation of COAWST you choose among the ones you mention?

This point was not clear in the old version of the manuscript. In the new version we have added a sentence clarifying the configuration of the reference simulation run used for the skill assessment of COAWST (Lxxx-Lxxx).

- P9L6, P9L12, P10L14: add some comments to Table 2;

Several comments addressing Table 2 are included in the new version of the manuscript for the different physical variables associated at waves, wind and currents description.

- P11L9-10: which days are included in Table 3? it is really OOST better? It does not seem it is the case;

The days considered are included in Table 3 caption. The paragraph related to the inter-comparison of air-sea formulation has been modified because was not clear (also noted by the other review):

"*Numerical coupled results does not present better agreement at the observational point than the uncoupled mode results. Comparing the error statistics for the observational point among the three coupled numerical simulations we cannot assure which formulation ensures a better skill assessment (Table 3). At control point the magnitude of the wind intensity and the significant wave height is larger for the uncoupled simulation (CHK) in comparison to coupled simulations (Figure 10, bottom sub-plots). Maximum differences of 3 m•s-1 in wind intensity and 0.3 m in significant wave height are obtained if we compare OOST and CHK simulations. In consequence, small differences are found between coupled and uncoupled simulations when wave conditions increases.* "

- P12L2-9: please can you provide some quantitive indications on the improvement due to the change in the whitecapping dissipation?

Our skill in wave modelling highlight that the whitecapping dissipation is a relevant process in a limited-fetch areas. Some special test were carried out in Pallarès et al., (2014). We don't test different whitecapping terms parameterization but our skill assessment prove the good fitting with the data is achieved in comparison to previous contributions. We modified the manuscript to clarify this point:

"*Also, the wave modelling deserves a particular comment related to the good fitting of wave results in comparison to previous investigations (Bolaños et al., 2007; Sánchez-Arcilla et al., 2008). Statistical errors were reduced significantly due to the young sea developed in the wind jet region likely thanks to the modification of a parameter relative to whitecapping dissipation (Pallares et al., 2014). In particular, smaller root mean square errors were obtained in the mean wave period variable, which presented a large uncertainty (Bolaños et al., 2007; Sánchez-Arcilla et al., 2008; Alomar et al., 2014).*"

Other points:
- P2L12: "wave climate : : :": climate is not appropriate in this context; use for example pattern;

Ok, changed by "*pattern*".

- P3L22: remove "induced by the lee of the Pyrenees mountains": the mechanisms are more complex than simply described here;

Ok, deleted.

- P6L13: remove "boundary layer physics schemes and : : :": they are part of the parameterization schemes mentioned afterwards;

OK, deleted.

- P6L16-18: how frequent are the data exchanges between the different models?

Information added in section 2.2.: "*The bottom boundary layer was parameterized using a log profile with bottom roughness equal to 0.005m. The time-interval for data exchange between the models is 600s".*

- P8L25: This scatterometer product is different from that mentioned at page 5; - P9L1: this is mainly an effect of the horizontal resolution;

Corrected. It is the same product. We homogenize the horizontal resolution information (i.e. 0.25º).

- P11L20: which reanalysis?

As a final product provided by the Spanish Ministry of Energy we don't have any additional information that appears in the mentioned web. We include in the new version of the manuscript: *"15 years using the MASS model".*

- P12L1-2: please make clearer this sentence;

Modified. *"The second implication is related to the momentum transfer: several authors have highlighted that under mixed wave-train conditions the ocean surface roughness may increase appreciably (Sánchez-Arcilla et al., 2008)."*

- P13L29-31, P14L15-16: please refer to Ricchi et al. (2016) Ricchi A., M. M. Miglietta, P. P. Falco, A. Bergamasco, A. Benetazzo, D. Bonaldo, M. Sclavo, S. Carniel, On the use of a coupled ocean-atmosphere-wave model during an extreme Cold Air Outbreak over the Adriatic Sea, Atmospheric Research 172–173, 48–65, 2016;

Ok included. Thanks for the reference.

Talbes and Figures:
Table 2: results for V are not shown

V (depth-averaged velocity in the cross-shelf direction) is not included because the results are not significant: in the cross-shelf circulation 2D circulation is developed (see Figure 8), and the depth-averaged velocity is too small. However a comment is added at the section 3.1:

"*Skill assessment is better in depth-averaged along-shelf flow in comparison to cross-shelf (e.g. R equal 0.82 to vs. 0.24) due to the frequent two-layer flow structure observed in cross-shelf measurements during wind-jet events giving rise a weak depth-averaged cross-shelf velocity."*

Alternatively, a qualitative comparison is provided in the Figure 8 and the corresponded section of the manuscript.

Table 3: which days are considered?

For the wind-jet sequence. Included the information in the table caption in the new version of the manuscript.

Figure 2: the arrow length-scale is missing; also, use "hPa" instead of "HPa"

Ok, corrected and the length-scale vector included.

Figure 3: caption: rephrase: "results obtained for COAWST at mesh M3 are plotted".

Rephrased.

Figure 4 caption: “: : : the entire 12 months analysed”.

Ok, modified.

Figure 5: which model run do you show? why do you show the results at that date?

Ok, information included. We show these results because are illustrative of a cross-shelf wind event.

Figure 6: I cannot see the blue line

Ok, thanks. Caption corrected.

Figure 7: I understand the two panels are inverted with respect to the caption;

Thanks, figure has been corrected.

Figure 8: units are missing

Units included in the figure caption. Thanks.

Anonymous Referee #2

Received and published: 10 March 2016

The authors use a coupled atmosphere-ocean-wave model (SWAN) to simulate winds in a particular region on the continental shelf in the Mediterranean Sea. After showing that the model does a good job in reproducing the observed wind and wave patters, the authors use different parameterizations for the atmospheric bottom roughness length: in one case it depends on wind intensity ("uncoupled" simulation), and in three cases it depends on the ocean surface wave field as well ("coupled" simulations). The authors conclude that, despite the differences between the different parameterizations are small, the coupling becomes important for wind power assessments, which depend on the third power of the wind intensity.

The research is interesting and worth being published, after consideration of some issues presented in the following.

The authors acknowledge the helpful comments and corrections of the reviewer #1 that helped to improve the quality of the paper. The English have been improved in several parts of the manuscript. Besides, the manuscript has been corrected by a native English speaker (Kevin Callon).

My main concern is in the conclusion that "coupled" is better than "uncoupled". The differences between the coupled and the uncoupled simulations are minor when compared to the discrepancies between any of the simulations and the observations at the mooring site (see fig. 10 and table 3). For this reason, I would conclude that there is no reason for choosing a parameterization (either coupled or uncoupled to the ocean surface wave field) versus another one. On what base do the authors conclude that coupled is better than uncoupled? Should I believe the wind power estimations reported in the discussion section coming from the coupled simulations better than those from the uncoupled simulation simply because the former incorporates more physical mechanisms? It seems to me that if this was the reasoning, then one should always prefer a more complex model versus a simpler one, which is something I don't really feel comfortable with. Please add a discussion on this issue in your manuscript.

We agree with the reviewer that this point was confusing. We tried to highlight the importance of the coupled simulations when a high-accurate solution is required (i.e. the case for wind power estimations), so the small differences in coupled/uncoupled mode may lead at significant differences in wind power estimations. Your are right that, according to our data set we can not assure that the coupling simulations presents better agreement with the observations than the uncoupled one.

In consequence, we have modified the manuscript in several sections to address (and calrify) this point:

- In the section 3.2, we address this point that according to the skill assessment the uncoupled and uncoupled results does not present significant differences:
  "*Numerical coupled results does not present better agreement at the observational point than the uncoupled mode results. Comparing the error statistics for the observational point among the three coupled numerical simulations we cannot assure which formulation ensures a better skill assessment (Table 3). At control point the magnitude of the wind intensity and the significant wave height is larger for the uncoupled simulation (CHK) in comparison to coupled simulations (Figure 10, bottom sub-plots). Maximum differences of 3 m•s-1 in wind intensity and 0.3 m in significant wave height are obtained if we compare OOST and CHK simulations.*

*In consequence, small differences are found between coupled and uncoupled simulations when wave conditions increases. "*

- We highlight that in the section 3.2, we pointed out that small differences are obtained in the control point:
  "*Comparing the error statistics for the observational point among the three coupled numerical simulations we cannot assure which formulation ensures a better skill assessment (Table 3).*"

- In the discussion section (4th paragraph), we compare with other authors the coupling/uncoupling differences but we recall that according to our data set we can not discern if uncoupled differences enhance the skill assessment. However, numerical results presents differences at offshore point (control point with larger fetch), however significant differences are suspected. A clarifying sentence at the end of this paragraph has been added:
  "*Unfortunately, the lack of measurements offshore of the observational point (i.e. larger fetch in comparison to observational point) has not allowed to investigate if the coupling simulations present better skill assessment than the uncoupled case. "*

Other points:

The language is poor. Sometimes subjects and verbs don't match, in other cases the adjective should be an adverb or viceversa. There are many sentences that need to be rewritten, here I list just a few of them: P1: L24-25, L29-30; P2: L6-7; P5: L15-16; P15: L12-14.

The English have been improved in several parts of the manuscript. Besides, the manuscript has been corrected by a professional English corrector (Kevin Callon).

Please don't write "ocean bottom roughness", as this induces the reader to think about the spatial structure of the batimetry. You mean the "ocean surface roughness", which is the "bottom roughness" for the atmosphere!

Ok, the words "*ocean bottom roughness*" has been modified by "*ocean surface roughness*".

page 6, line 1: the Blended Sea Wind product has a spatial resolution of 0.25 degrees, as you state on page 8. Why do you say here that the resolution is 15 km?

Ok, corrected.

Sometimes COAWST is mispelled as COWAST

OK, modified

Results from what coupled simulation are described in section 3.1?

As we mention in the manuscript, the comparison between observational data and numerical results depends of the set of available data. For instance, remote wind comparisons is carried out in M2 mesh and waves in mesh 03 (see first paragraph in section 3.1). The meshes configurations (and also coupled/uncoupled) are explained in section 2.2. However we try to clarify this point at beginning of section 3.1. adding this sentence:

"*The skill assessment of the model is carried out for different meshes in function the spatial domain of the observations.*"

The authors say that the modification of the whitecapping dissipation term has improved the simulation results, but they never really show it or assess it in any way. On P12: L2-7, the authors say that such a modification has significantly reduced statistical errors, but I did not see any representation of that change.

Our skill in wave modelling highlight that the whitecapping dissipation is a relevant process in a limited-fetch areas. Some special test were carried out in Pallarès et al., (2014). We don't test different whitecapping terms parameterization but our skill assessment prove the good fitting with the data not achieved in previous contributions. We modified the manuscript to clarify this point:

*"Also, the wave modelling deserves a particular comment related to the good fitting of wave results in comparison to previous investigations (Bolaños et al., 2007; Sánchez-Arcilla et al., 2008). Statistical errors were reduced significantly due to the young sea developed in the wind jet region likely thanks to the modification of a parameter relative to whitecapping dissipation (Pallares et al., 2014). In particular, smaller root mean square errors were obtained in the mean wave period variable, which presented a large uncertainty (Bolaños et al., 2007; Sánchez-Arcilla et al., 2008; Alomar et al., 2014)."*