

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 patterns, 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. You 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 clarify) 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 misspelled 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).”