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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Ocean–atmosphere–wave characterization of a wind jet (Ebro shelf, NW Mediterranean Sea)" *by* M. Grifoll et al.

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

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issues presented in the following.

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 oceas 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.

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.

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!

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?

Sometimes COAWST is mispelled as COWAST

Results from what coupled simulation are described in section 3.1?

The authors say that the modification of the whitecapping dissipation term has im-

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proved 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.

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