Geosci. Model Dev. Discuss., 7, C3725–C3728, 2015
www.geosci-model-dev-discuss.net/7/C3725/2015/

Geoscientific Model Development Discussions Open Access

Interactive comment on "On the wind stress formulation over shallow waters in atmospheric models" *by* P. A. Jiménez and J. Dudhia

P. A. Jiménez and J. Dudhia

jimenez@ucar.edu

Received and published: 11 June 2015

Dear editor H. Weller,

We would like to clarify that the main intent of our manuscript is to make researchers aware that shallow water is not correctly handled by standard wind-stress formulations. This is the main message of the abstract and it is clearly supported by the results provided in the manuscript. The formulation that we used to illustrate this is of secondary importance and we recognize it in the conclusions (and abstract) where we point out that a more complicated formulation is anticipated and even that bathymetry should be taken into account. The main message is that we need better wind-stress formulations over shallow waters where modelers would currently be using deep-water formulations that lead to the types of biases that we documented. You cannot find this in any previ-

ous publication. GMD readers should be interested in this original contribution.

Indeed, two of the reviewers saw the value of our contribution and only suggested minor comments. There were two short comments as well that saw the value of the contribution and their main motivation was to point out concerns with the quality of the FINO1 data. You mentioned in your reply that "I am happy with your responses describing why the problems may not be so severe and saying that you will acknowledge the problems with the FINO1 data." So far we had 4 favorable reviews and one revision pending.

We believe we were able to answer the main concerns of the remaining reviewer, and hope that your comment as Editor did not sway his/her opinion of our response. We think that the details required by the reviewer were not critical to the main subject of the paper, which we reiterated above and which other reviewers accepted. The purpose was not to introduce a fully tested new parameterization for all depths, but to point out issues by using the substantial data we had at one tower, and showing a correction that worked for this case as guidance for future development. We think we were clear enough with caveats, and now also were able to improve our confidence that the level thicknesses did not affect our conclusions.

Regarding your comments, we should clarify that

1) We are not using very limited set of data but a whole year of observations of the wind profile (eight wind sensors). Sometimes parameterizations are developed with only one case study with observations during one day. We use data at one location because 1) observations of the wind profile over the ocean are not very frequent, and 2) having observations at different sites will have different depths of the ocean and this is not desirable for isolating this type of bias. We pointed out the caveat of using one site in the conclusions wherein we mention that the wind stress formulation should be tested at other locations and that it may depend on the depth of the ocean. But again, results are sufficient to reach awareness about the wind stress formulation over shallow

waters, which is the main message of the manuscript.

We can introduce the phrase you extracted from our answer to a reviewer's comment in the manuscript if this is more appropriate.

2) u* is calculated in the surface layer parameterization and here is where we have incorporated the wind stress formulation presented in the manuscript. We can clarify this on the manuscript as well.

3) While the observations in Figure 2 are for a different shallow-water site, the comparison in Figure 1 is the direct comparison with wind observations at our site. We believe this is the more appropriate comparison, while the observations in Figure 2 are only given as support that these types of drag coefficients are derived from observations elsewhere. Figure 2 is considerably more scattered due to having less observations. As a practical matter, it is better to match the raw wind profile than a derived drag coefficient. The Charnock and Edson formulations introduce a positive bias in the wind speed whereas the new formulation does not (Fig. 1). This means that increasing the drag over shallow waters in agreement with observations is necessary and sufficient to reconcile model results with observations and thus supports our main message of the manuscript. The same cannot be said regarding Edson and Charnock formulations.

4) The experiment that we designed is adequate to test the sensitivity to the vertical levels and the wind stress formulation. We should clarify that the wind profile responds almost immediately to the wind stress formulation and in a few model steps we have a wind profile that is highly non uniform and sheared. If the sensitivity to the models levels was more important than the wind stress formulation it would have been very clear in the results of the experiment. We didn't change the first model level because it is not desirable to have a vertical level too close to the earth surface. We can mention this sensitivity experiment in the manuscript. We added our chosen levels in the manuscript in case others want to match them, but they are in the range of normal practice.

We hope that these considerations would help to clarify the originality of the manuscript

and why we believe it should be published in GMD. It was intended to not only point to a problem, but used simulations of robust amounts of data to show a path to a solution, and we also supported this with a theoretical rationale from the previous literature. In addition, we hope you take into account the favorable reviews that we already had and even consider the possibility of seeking additional reviews to assist you in taking the best decision regarding the publication of this manuscript.

Sincerely,

Pedro Jimenez and Jimy Dudhia.

Interactive comment on Geosci. Model Dev. Discuss., 7, 9063, 2014.