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

H. Weller (Editor)

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I have had off-line discussions with the authors and reviewer 2 has emailed me some comments off line. I therefore feel in a position to make my final decision. I post here the off-line discussion and the reviewer comments and the reason for my decision. The authors are not happy with my decision and I have offered that the manuscript could be transferred to another topical editor of their choosing. I await their response as to which topical editor they would recommend:

http://www.geoscientific-model-development.net/editorial_board.html

My decision is that this paper is not publishable. The authors have argued that they do not need to make the changes suggested by reviewer 2 and I disagree. I am also nervous that the wind stress formulation suggested is simply a fit to some very limited and problematic observational data.

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Comments from reviewer 2 after reading my comments: "EC C3723: 'Justification for expected rejection, pending reviewers' comments', Hilary Weller, 10 Jun 2015 gmdd-7-C3723-2015.pdf

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I read over your comments though, and they seem very valid.

For 1: I agree that fitting a curve to sparse data is risky, and their text (from the prior round) leads the readers to falsely believe that a mathematical formulation was derived based on the physical properties of the system. I would be hesitant to believe such a curve based on limited observations (is this standard procedure in their field?). I also agree that the text they provided you was very vague ("Different values of z0 fitted the observations better in different wind ranges…").

For 4: If they are trying to test the sensitivity of the vertical resolution, I would completely agree that there will be little sensitivity to resolution if a uniform wind profile is used. If they are trying to test the sensitivity of horizontal resolution and used the same uniform wind profile across the domain, I would also agree that there will be very little sensitivity to resolution. For a more realistic system with varying winds over the domain of interest, I would argue that a 3 km resolution grid will yield more realistic results than a 27 km resolution grid over shallow water regions that are associated with complex coastal geography. If not, I think it should be shown to the reader to convince them that it isn't the case. That's probably more information than you needed, but I agree with your comment :)

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Email discussion between editor (me) and authors (lead by Pedro Jimenez Munoz)

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Dear Dr. Weller,

Thanks for your email. I have included my co-author Dr. Dudhia (cc.) so it is easier for me to keep him informed about the state of the manuscript. We appreciate your openness for discussion and we would like to explain why your two remaining concerns should not prevent the publication of our manuscript:

1.- The uniform wind profile is just an initial condition which is a normal way of setting up idealized cases. To illustrate that we have a sheared profile, the wind at the tower levels after 30 min of simulations (the first time step we saved) is 15.2781, 16.4762, 17.8380, 18.8340, 19.8300, 20.8259, 21.4189, 21.8778 m/s. Actually, this sheared profile appears after a few time steps of the model.

We chose an ideal case to better understand any sensitivity to vertical levels. These tests are very easy to run and can be used to demonstrate the parameterization in controlled conditions. On the contrary, simulating the experiments that we show in the manuscript require hundreds of simulations and using this high computational resource is not necessary or even justified to illustrate the sensitivity to the vertical levels and the wind stress formulation.

We did not look at sensitivities to more realistic simulations. We can openly say this in the discussion if necessary. Actually, if we had done the simulations and these did not support our statements we would have said this clearly in our answer. We do not want to publish articles with statements that we do not believe.

If we have to guess what would had happen in a more realistic simulation we would say, after having performed the ideal experiment, that the sensitivity would have been of even smaller magnitude than the ideal case. The new wind stress formulation always reduces the wind speed whereas changing the vertical levels would introduce a more stochastic behavior and the small differences would cancel with each other.

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We can discuss the idealized runs in the manuscript if necessary. It is a common approach in developing parameterizations and surely used by many GMD authors/readers. We did not feel it necessary to illustrate the main ideas provided in the abstract, but we will be willing to incorporate it in the manuscript if this is more appropriate.

2.- It was not our intention to hide that the new formulation was derived fitting observations. We say that the z0-wind relationship was selected in such a way that it suppresses the wind speed bias. We thought that was clear enough but we can clarify it in the manuscript. Fitting observations has been done in previous publications on this type of parameterization like the following where a roughness relation was derived for high winds in tank experiments.

Donelan, M. A., B. K. Haus, N. Reul, W. J. Plant, M. Stiassnie, H. C. Graber, O. B. Brown, E. S. Saltzman, 2004: On the limiting aerodynamic roughness of the ocean in very strong winds, Geophys. Res. Lett., 31, 4539-4542.

Also, Andreas et al. (2012) used multiple field programs like Edson et al.

Andreas, E. L, L. Mahrt, and D. Vickers, 2012: A new drag relation for aerodynamically rough flow over the ocean. J. Atmos. Sci., 69, 2520–2537.

These are all curve-fitting approaches, which is the only approach possible due to the complexity introduced by wave variability.

Regarding just fitting observational data there are several aspects we would like to clarify: a. We isolated that the bias occurred at the surface rather than in the downward PBL mixing. b. In models, surface drag is controlled by the roughness length, so this leaves us little choice in how to correct the problem. c. Roughness lengths over water are typically empirical fits to data and the fit depends on the surface wind or u*, which is its proxy that also accounts (slightly) for stability. d. The whole wind profile is improved by our surface correction which demonstrates that the observations and model have a

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consistent profile that is defined by just the roughness length, and in fact adding the stability correction via using u* made a further improvement in stable conditions, which further confirmed to us that the approach was correct and sufficient. Note that we tuned Eq. (2) with a neutral assumption because high-wind cases tend to be neutral, and the improvement for stable conditions came as a bonus when we generalized it to Eq. (3). e. Our sense from the consistency we see is that the tower data is OK, but we can add more caveats if needed. Recall also our analysis/answers to the short comments regarding the quality of FINO1 data that you indicate were appropriate. Again, if we think that the FINO 1 data is not valid to support our statements we would not be pursuing the publication of the manuscript. f. Our shallow-water drag enhancement factor is within the range expected by previous estimates that have been published which we regard as further support.

We can also add that the original motivation for this study was only looking to evaluate existing formulations with long-term tower data, and it was only after we found a significant bias that we became aware of the observational literature on shallow water effects which explained our results and led to our contribution that provides the first modeling perspective on this issue.

We hope that this clarifies these two aspects and why should not prevent the publication of the manuscript but we are willing to clarify or enhance any of these explanations if you have any further comments or questions.

Sincerely, Pedro.

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On Thu, Jun 11, 2015 at 3:06 AM, Hilary Weller <h.weller@reading.ac.uk> wrote:

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Dear Dr Jiminez

I hope you do not mind me contacting you off line. Once our discussion has reached

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a conclusion, I will put a summary of it onto the GMD discussions cite for this paper. I have two remaining concerns with this paper, one of which you can easily address and one of which it may only be possible to address with some large caveats in your manuscript.

1. Your choice of resolution sensitivity was very strange; assuming a uniform wind profile. I was not surprised that the simulations were not sensitive to resolution when using a uniform wind profile. You must have gone out of your way to create this alternative experiment. I suspect that you have looked at sensitivity to vertical resolution in a more realistic simulations but the results were not as expected. I was also disappointed that the study on the sensitivity to vertical resolution was in the responses to reviewer's comments, not in the manuscript.

2. It is now clear from your responses to reviewer comments (but less clear in the paper) that your new formulation is simply a fit to some observational data. This is of course part of the way in which parameterizations are developed. However you are creating a fit to some data for which there are serious concerns as to its quality. You do not discuss these concerns with enough detail or prominence in the paper.

It is certainly an appealing argument that shallow seas are rougher, and you make this argument well. I can see plenty of merit in your paper. However we cannot ignore the possibility that the slight bias of the existing roughness formulations could be due to problems with the FINO1 data. This is of particular concern since there was no mathematical derivation or your new formulation. It was entirely data fitting.

It is not unusual for editor's to give most weight to the most critical reviewer and to give most weight to a reviewer who is prepared to continue to engage with the review process after the 1st round of reviews. Editors also give more weight to reviews from reviewers who are clearly expert in the subject and who appear to be unbiased. Editors also often lead reviewers, encouraging them that they can stick to their critical concerns. However usually this is not done in full site of the authors.

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Hilary

– Dr Hilary Weller Department of Meteorology University of Reading, Earley Gate, Reading, RG6 6BB, UK

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

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