Geosci. Model Dev. Discuss., 7, C3751–C3753, 2015
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7, C3751–C3753, 2015

Interactive Comment

# *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: 26 June 2015

Dear Editor,

Thanks for your time with the revision of our manuscript. We would like to comment about your two remaining concerns. The first one is not clear. Your comment does not indicate which changes suggested by reviewer #2 we say we do not need to incorporate in the manuscript. We would appreciate if you can clarify us this because we have been very constructive during the revision process and we feel that this should not be a major concern. We have carried out the vertical resolution test and mentioned that even if the initial profile is uniform the model quickly produces the sheared friction layer based on the surface drag formulation. This test showed that the model response to the surface drag formulation difference was robust to changes in vertical resolution, which helps to



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verify the generality of our results.

Regarding your second concern, "the wind stress formulation suggested is simply a fit to some very limited and problematic observational data." we would like to emphasize again that we used an entire year of observations to derive the wind stress formulation. We provided evidence in one of our previous answers of why the quality of the data should not be of major concern in this study and this seemed to be accepted at the time.

We should also clarify that Edson and Andreas and others so far have used eddy covariance measurements to derive surface fluxes and then parameterized Cd based on those estimated fluxes. We took a more direct approach by using the wind profile to formulate a suitable surface stress. This removes a step of uncertainty and more directly corrects the wind bias. In all previous cases, fitting observations is also the method, and usually these were field programs of a few days to weeks, while were able to use long-term annual data by our direct method. The scatter plots in our figures illustrated some typical field program data, but that was not the data we used for our formulation. It was only used as independent corroboration of shallow-water observations.

We therefore believe that this information on prior studies addresses your second concern.

We mentioned in an email after your suggestion of changing to another topical editor that we would appreciate if you can transfer the manuscript to any topical editor with experience on mesoscale modeling or on model physics related to the PBL or ocean/atmosphere interactions. We did not find any member of the editorial board with expertise in these areas so we do not have a strong suggestion regarding the new editor. You know the editorial board better and probably can find the most suitable new editor.

We can suggest additional reviewers of the manuscript. We believe it is fair to ask for additional opinions since we had 4 favorable reviews (see our answer to one of your



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comment; our comment posted on June 11) so far. This is 80 % of favorable reviews. These are the names of potential reviewers that can assist you taking the best decision regarding the publication of our manuscript:

James B. Edson, University of Connecticut, james.edson@uconn.edu

Edgar L. Andreas, NorthWest Research Associates, andreas@nwra.com

Christopher W. Fairall. NOAA, chris.fairall@noaa.gov

Jian-Wen Bao, NOAA, jian-wen.bao@noaa.gov

Sincerely,

Pedro Jimenez and Jimy Dudhia

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