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Geoscientific Model Development Discussions

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

Anonymous Referee #2

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On the wind stress formulation over shallow waters in atmospheric models

P. A. Jiménez and J. Dudhia

Recommendation: Accept with Major Revisions

General Comments

The authors address sources of the positive boundary layer winds biases in numerical models over shallow water regions. They assert that these biases are due to poor representation of the roughness formulation, a formulation more representative of turbulent mixing over the open ocean than shallow water. I feel that this is a worthwhile numerical modeling problem and appreciate the authors' approach in isolating the potential source of these errors. I would appreciate an explanation of the physi-

cal reasoning of why the author's expect that the bathymetry would cause a change in the turbulence parameters and thus the winds, since I do not see a clear argument through the data alone. I would also encourage the authors to provide more information concerning the source of the observations, the analyses used to construct the figures, and the development of their new formulation. I found it difficult to assess the article given the lack of information provided about the author's methods, specifics on data, configuration of the numerical model, and specifics on the model-obs comparisons. I am uncertain whether my comments/questions may only require clarification or may change the overall conclusions, and therefore I recommended that the paper be accepted pending major revisions.

Specific Comments

1. Can you explain why physically the depth of the coastal ocean will impact the drag coefficient, friction velocity, and roughness length? I understand that you argue a low bias for these variables under high winds, but it looks like the Edson and Charnock curves fall within the range of observations (Fig. 2). I'm trying to understand the physical reasoning behind the need for your improvements since I don't see a strong case in the observations.

2. What is the sensitivity to the WRF vertical resolution? The vertical resolution will impact turbulent mixing within the boundary layer parameterization scheme and thus the boundary layer wind profile. If the number of vertical levels within the lowest 200 meters was increased from 5 to 10, I would assume that this will impact the 60 m wind, especially if the lowest model level is at 15 m?

3. What is the size of your WRF domain? It would be useful to have a map illustrating the WRF domain as well as the location at which the observations were taken.

4. Did you use only one domain for the numerical simulations or a nested domain?

5. What is the bathymetry threshold that separates shallow water from the open ocean?

6. Figure 1: Why did you choose 60 m for the model-observation comparisons? The parameters illustrated in Figure 2 are based on the 10 m wind speed. I think it is important to understand the relationship between the Charnock, Edson, and your new formulation for the 10 m wind given the relationship to the turbluence parameters presented. Related to that, what is the sensitivity of the height selected to the parameters illustrated in Figure 2? Do you see similar relationships at other levels?

7. Figure 1: Just to be clear, is this for wind speed at 60 m in height for all grid points over the full domain? Also, why did you choose to evaluate model output only at 8760 hours?

8. It would be helpful to provide more information concerning the observations used in the analysis. How many observation points are included in the analysis? Are they all from one location or multiple locations? Where are these locations exactly? Did you compare the observations at this/these location(s) to WRF output at the latitude-longitude points of the data (it is unclear from your description)? At what times of the year were these data gathered? What is the boundary layer stability regime associated with these observations?

9. Figure 2: It would be helpful to quantify the improvement in the drag coefficient, friction velocity, and the roughness length using your new formulation. Is this improvement statistically significant?

10. P9069 Line 14: Why are you referring to a typical z0 for over grassland when your primary concern is differences over water? I'm not completely clear of the significance of this grassland roughness length given the context of this paper.

11. I'm not totally clear on how you developed your new formulation (i.e. equations 2 & 3) or how this is dependent on stability. I would appreciate it if you elaborated on this further.

12. Figure 3: What model points did you include this figure (i.e. all points across the

domain? If so how may is that?)? How many stable (c,d) and unstable (e,f) model regimes are used in this figure? How did you categorize these environments? How many observed data points were used to calculate the difference plots? There is an also the issue of statistical significance to support the improvement.

13. P9070 Line 21: It would be helpful to include the statistical significance of the results to support this statement.

14. P 9071 Line 12: Is this the reason you chose 60 m for your comparisons? If so, I think it would be helpful to include this information when first discussing the 60 m observations and model output.

15. I appreciate that you chose to include 4 different PBL schemes to get a representative cross section of the changes in friction velocity and the roughness length within the WRF. I was under the impression that each parameterization schemes calculates the turbulent mixing of momentum differently. It may be worth separating out the difference between these schemes, rather than presenting the evaluation of the model mean verses the observations. It would be helpful to confirm that your new formulation improves not only the mean of the 4 PBL schemes but the individual members as well.

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