ANSWER TO REVIEWER'S COMMENTS

"On the wind stress formulation over shallow waters in atmospheric models" Pedro A. Jiménez and Jimy Dudhia.

Reviewer 2

GENERAL COMMENT

Recommendation: Accept with major revisions

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 physical 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.

ANSWER

We would like to thank this reviewer for the time he/she devoted to review the manuscript. We have reproduced below each of his/her comments that include the ones stated in this general comment, and a detailed answer follow to each of them.

SPECIFIC COMMENTS:

COMMENT 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.*

ANSWER

It is generally accepted that the wind stress increases over shallow waters (e.g. Geernaert et al., 1986, 1987; Smith et al., 1992; DeCosmo et al., 1996; Taylor and Yelland, 2001; Foreman and Emeis, 2010). However, not so clear is the physical mechanism responsible for the enhanced drag and we explicitly say it on the manuscript (Page 9066, line 7). We also mention on the manuscript possible explanations such as the effects of the bottom of the ocean through shoaling or with form drag due to short (young) waves (Page 9066).

The Edson and Charnock curves are "*in agreement with the lower part of the observational scattering but with a clear underestimation of the recorded values at moderate-high winds.*". In particular, they do not reproduce the increase in drag for moderate-high winds shown by the observations. This is evident in the observations on Figure 2a. The new formulation is the only one showing a substantial increase of the drag for high wind speeds (see also the z0-wind relationship, Fig 2c. Notice the log scale). In the manuscript, we show that this added drag is sufficient to correct for the overestimation of the high winds (Fig. 1). We should especially recommend seeing Taylor and Yelland (2001) Figure 4b to see their expected effect as a function of depth for high winds.

No action will be taken.

COMMENT 2

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?

ANSWER

The impact of changing the vertical resolution is very likely to be dependent on the parameterization. The two turbulent closures based on TKE should be less sensitive to the changes than the other two parameterizations based on a first order closure. It is probably not a good idea to have the lowest model level much lower than 15 m specially for the first order closure parameterizations.

Our approach is to configure the WRF vertical levels in a standard approach using a higher number of levels closer to the surface. This configuration is therefore more similar to the one used by other modelers and to the one used by the developers of the parameterizations than the one resulting of doubling the number of the current vertical levels. We will not introduce any change in the manuscript regarding this comment since we are using a rather standard number of vertical levels.

COMMENT 3

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.

ANSWER

The domain size is 1539 km by 1620 km. The domain is shown on Figure 1 of this document together with the 2 nested domains of 9 km and 3 km that we used for the sensitivity experiment. The location of FINO1 is also shown (red star). We prefer not to include this Figure in the new version of the manuscript to focus on the derivation/results of the new formulation. However, we will add the coordinates of FINO1 on page 9066, line 27: "*The observations were acquired at the research platform FINO1 located at about 48 km from the German coast (54.01 degrees N by 6.59 degrees E) with ...*". We will also mention the coverage of the domain used in the WRF simulations on Page 9067, line 12: "*...were performed at 27 km of horizontal resolution. The domain covers the complete North Sea and the eastern part of the Baltic Sea.*".

COMMENT 4

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

ANSWER

We used a single domain for the numerical simulations herein presented. We will clarify this on page 9067, line 12 "... *the simulations herein presented were performed over a single domain of 27 km of horizontal resolution*".

COMMENT 5

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

ANSWER

There is not a single threshold that separates shallow from deep waters. The threshold depends on the wavelength of the waves. Depths of O[10-100 m] are affected by the bottom of the ocean and thus are considered shallow waters, see Figure 4b from Taylor and Yelland (2001).

COMMENT 6

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?

ANSWER

There are no observations at 10 m on the FINO1 platform. The observations were recorded at 8 levels : 33 m, 40 m, 50 m, 60 m, 70 m, 80 m, 90 m and 100 m. All of them show the same behavior in terms of the information provided on Figure 1. On the manuscript we show results for one of the middle levels, 60 m, and mention that all the levels show similar results. To understand better the selection of the 60 m level we will add the height of the observational levels in the new version of the manuscript on page 9066, line 25: "... *at a total of eight levels within the first 100 m of the atmosphere (i.e. 33 m, 40 m, 50 m, 60 m, 70 m, 80 m 90 m and 100 m)*". Additionally, the new version of the manuscript will explictly say that the 60 m sensor is one of the middle sensors (Page 9068, lines 2-3: "*data correspoinding to the 8760 h of 2009 is shown, for the sensor located at 60 m (one of the middle sensors), as a percentile-percentile comparison...*". Notice that on Figure 3 we show results for 3 heights, 33 m, 60 m and 90 m and all of them show a similar improvement.

It is a standard practice to analyze the wind-stress formulation for 10 m winds (e.g. Smith et al., 1992; Fairall et al., 1996; Foreman and Emeis, 2010; Edson et al., 2013). Indeed, the wind observations of the HEXOS programme shown on Figure 2 were recorded at 10 m. We will take no action with this second comment.

COMMENT 7

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?*

ANSWER

We use the wind at the nearest grid point to FINO1 to compare with observations. We did not use the wind averaged over the full domain. We simulated the atmospheric evolution over the complete year of 2009. The WRF output is recorded every hour. We compare the values of all the hours during a year, 8760, with the corresponding observations. So we are using the maximum amount of data available for comparison.

We will mention in the new version of the manuscript that we are using the nearest grid point to FINO1, page 9068, line 3: "*...percentile-percentile comparison in Fig. 1 (red area). The simulated wind at the nearest grid point to FINO 1 is used in the comparison. Clearly, ...*". We will also clarify that we are using data for the complete year of 2009, page 9067, line 10: "*Different simulations for the complete year of 2009 were performed at 27, 9 and 3 km with very little sensitivity...* ".

COMMENT 8

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?

ANSWER

There is only one observational point, FINO 1, where the lower level wind speed is recorded at eight heights within the first 100 m of the atmosphere. In fact our depth effect would be harder to see with multiple sites and different depths. The location of the tower is shown on Figure 1 of this document. We say that the site is at 48 km from the German cost. We will add the latitude and longitude of the tower on page 9066, line 27: "*The observations were acquired at the research platform FINO 1 located at about 48 km from the German coast (54.01 degrees N by 6.59 degrees E) with ...*". We use the simulated wind at the nearest grid point to FINO1 to compare it with observations. This is missing in the manuscript and we will add it on page 9068, line 3: "*...percentile-percentile comparison in Fig. 1 (red area). The simulated wind at the nearest grid point to FINO 1 is used in the comparison. Clearly, ...*". We compare the observations and simulations for all the hours with records available during the complete year of 2009 so there is a range of atmospheric stabilites. We will clarify that we use the complete year of 2009 on page 9067, line 10: “Different simulations for the complete year of 2009 were performed at 27, 9 and 3 km with very little sensitivity... “.

COMMENT 9

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?

ANSWER

The observations show an increase of the drag for moderate-high winds that is only reproduced by the new formulation (Fig. 2). We explicitly mention this in the text. We do not quantify the improvement in the drag wind speed relationship, instead, we quantify in Fig. 1 the ability of the model to reproduce the wind speed which is the variable under study. The increase of the drag allows us to reproduce the observed wind speed histogram within the range associated with the PBL uncertainty. The open ocean formulations do not reproduce the observed wind speed histogram for moderate-high winds within the range of the PBL uncertainty. So we are quantifying the improvement of the new formulation in terms of the reproduction of the wind speed histogram.

COMMENT 10

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.

ANSWER

This reference to grass was not very fortunate. It can lead to confusion as the reviewer points out. We will remove it from the new version of the manuscript. The phrase will say "*A value of 0.01 m is reached at 20 m/s giving a 10 m drag coefficient greater than 0.003 (blue, Fig. 2a) what would be considered high for a water surface.*".

COMMENT 11

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.*

ANSWER

We mention on page 9069, line 7-8 that Eq. 2 was selected by "*modifying the z0-wind relation in order to suppress the wind speed bias*". So Eq. 2 is an empirical formula applying to all stability conditions. Then, we mention that we assume that the average atmospheric condition is neutral in order to use the logarithmic wind profile to derive Eq 3 from Eq 2 (See text on page 9069, lines 19-21). The stability is introduced in the friction velocity, u*, which depends on the atmospheric stability. We will add a new phrase to explicitly indicate that the dependence on the stability is through u*, (Page 9069, line 25): "... *in the new WRF experiment. Hence, the effects of atmospheric stability are included through u*. The u* values obtained...*"

COMMENT 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.

ANSWER

We used only the simulation at the nearest point to FINO1. We are going to clarify this (see answer to previous comments). There is one unstable regime and another stable and the classification is based on the simulated z/L, z being height and L the Obukhov length scale. We will clarify this definition of the stability regimes on the caption of Fig. 3. We do not think is necessary to apply a statistical test, we just want to show that the new formulation is not damaging one stability regime (the stable or the unstable) and this is evident with the current Fig. 3.

COMMENT 13

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

ANSWER

We will reformulate this statement to indicate that we are using a large sample of data in the comparison. We want to highlight that the comparison goes beyond a case study. The new phrase will say "*Results herein presented are valid for wind speeds up to 20 m/s. We are confident generalizing the validity of the results given the length of the simulated period (1 year).*".

COMMENT 14

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.

ANSWER

We chose the observations at 60 m because is in the middle of the wind sensors. We mention in the text that the rest of the levels show similar results. The new version of the manuscript will explicitly say that the 60 m sensor is one of the middle sensors (Page 9068, lines 2-3: "*data corresponding to the 8760 h of 2009 is shown, for the sensor located at 60 m (one of the middle sensors), as a percentile-percentile comparison...*"

COMMENT 15

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.

ANSWER

The comparison of results from the 4 PBL schemes and the observations herein presented is adequate to quantify the uncertainty of the simulations associated with the turbulence closure. The four PBL parameterizations should reproduce the observations, the differences among them being an indication of the errors introduced by the representation of the turbulent mixing. The results indicated that the systematic error was not related to the PBL formulation. This is exactly what we want to quantify to isolate the effects of the ocean stress. No action will be taken.

References

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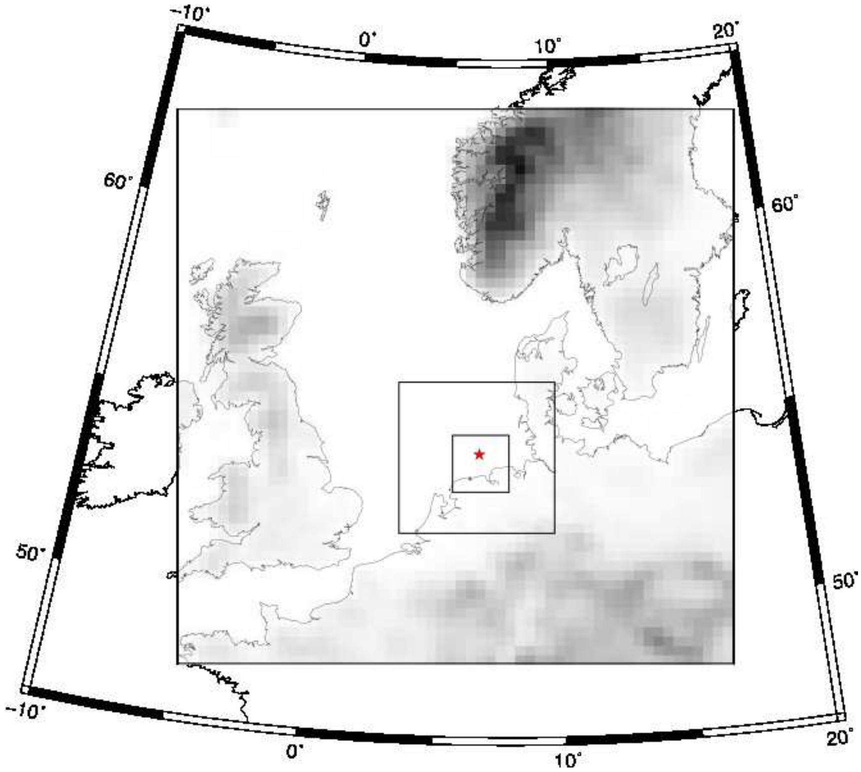

Figure 1: Area covered by the 3 domains (27 km, 9 km and 3 km). The star highlights the location of FINO1.