Reply to the Editor and the Reviewers

Dear Dr. Yool,

We have extensively revised the submitted manuscript gmd-2015-38 according to the suggestions of the reviewers. We have also replied to all of their comments separately. The revisions can be summarized as follows:

1. Addition of an extensive paragraph clarifying the saturation of the Charnock coefficient above a wind speed threshold (in page 16). This important issue has been raised by the Reviewer #1.
2. A new paragraph has been also added in response to the Reviewer #2 comment (pages 14-15). The manuscript references are also updated.
3. Many revisions have been made throughout the manuscript in order to fulfill the entire list of the reviewer #1 comments.

We would like to thank the anonymous reviewers for their useful and constructive comments especially during this second review.

Finally, we would like to thank you for your efforts towards improving this manuscript.

The corresponding author,

Petros Katsafados

Reply to Reviewers’ Comments

We have read the comments of both reviewers and all the suggested modifications, additions and corrections have been made in the revised manuscript. The replies for each specific comment follow.

Anonymous Referee #1

Second review of "a fully coupled Atmospheric-Ocean Wave modeling system (WEW) for the Mediterranean Sea: interactions and sensitivity to the resolved scales and mechanisms"

I feel the paper still needs some adjustments to make it acceptable for publication.
As I have stressed in my first review, the concept of coupling an atmospheric model and ocean wave model is not new. Such a system has been operational at ECMWF since 1998 and the method described here is essentially the same, albeit with a different software implementation. So I'm recommending to replace "With this in mind," (line 22) by "Following and adapting concepts already developed and implemented in large scale numerical weather models and in hurricane simulations,"

Reply: Done.

P3:, line 21: "Charnock drag coefficient" do you mean the drag coefficient obtained when the sea surface aerodynamical roughness is specified using the Charnock relation (Charnock 1955).

Reply: The phrase was modified accordingly.

P3, line 24: "decrease in sea surface friction arising from the breaking waves" Do you actually mean that? Later at line 27, it is mentioned that "sea surface roughness were much larger". Shouldn't it read instead: " increase in sea surface friction due to strong wave generation" (momentum is lost by the atmosphere because more waves have to be generated. A large part of that momentum is then lost to the oceans due to wave breaking)

Reply: This comment is not clear to us since the phrase "decrease in sea surface friction arising from the breaking waves" is referenced to the tropical winds (>33 ms^{-1}) and the phrase “sea surface roughness were much larger" is mentioned to the roughness length simulated by the coupled and uncoupled systems.
Those statements are consistent with many other studies because under high wind conditions, breaking waves, sea spray, and foam may play a significant role in determining the wind stress (Andreas and Emanuel, 2001; Makin and Kudryavtsev, 2002; Powell et al., 2003). Flow separation from the surface contributes to the limiting aerodynamic roughness at extreme wind speeds (Donelan et al., 2004). This is likely due to the existence of sea spray

(Powell et al. 2003), as well as flow separation induced by wave breaking, which causes the airflow to not ‘‘see’’ the troughs of the waves and skips from breaking crest to breaking crest (Donelan et al. 2004). Therefore the drag coefficient levels off at high wind speeds (above 33 ms^{-1}) under tropical cyclones, and it is consistent with previous and recent observations and modeling studies (Moon et al., 2004; Liu et al., 2011).

P4: I would move the new line 4 to line 13 (In a recent ... impact) to after line 30.

Reply: Done.

P4, line 26: I'm not sure we need the exact resolution of the ECMWF system. These will probably be obsolete by the time this paper is published as ECMWF is due to upgrade its resolutions in the spring of 2016 (28km -> 14km ; 16km -> 9 km).
In your review of work, in the My-Wave project, there was also another project that did the same type of work of coupling an atmospheric model with a wave model. It would be good to quote their contribution.

Reply: We removed the sentence with the characteristics of the ECMWF system and the paragraph has been modified accordingly. We assume that the reviewer means “…another partner…” in MyWave project. Thus we added the phrase in P15L13-18 “In a parallel to WEW research effort within the MyWave project the Italian team consisting of the Institute of Marine Sciences (ISMAR) and the Italian Meteorological Service (CNMCA) coupled WAM with the COSMO atmospheric model over the Mediterranean Sea (at a lower horizontal resolution though) showing similar results especially in terms of winds and significant wave height RMSE reduction (Torrisi et al., 2014)” to acknowledge the contribution of the Italian research team.

P7, line 27-28: "using OMP directives" add "only" as it is an interesting point to make that the version of WAM you have used did not use MPI directives (you obviously used them for the coupling). As I believe the ECMWF system should serve as a reference, it would be good to point at this obvious difference with the system based on ECWAM. ECMWF makes full use of MPI directives (as well as OMP) and for this reason, it could be fully included in the IFS as a subroutine. Both approaches have merits no doubts. ECMWF system has the advantage that it avoids global field MPI communication among other things.

Reply: We followed the reviewer’s suggestion.

A note outside the paper review: ECWAM software is available on request to ECMWF.

Reply: We thank the reviewer for offering this info.

P7, line 7: Fig. 1 -> Fig. 7 (?)

Reply: Done.

P8, line 10: the value 0.01 is the value in WAM cycle-4 and in the study presented, you want to show the impact of the coupling without changing the defaults but it would be good to mention in the discussion that the coupled ECWAM is using a lower value of 0.006 (Bidlot 2012 or IFS documentation) because it was found that the mean Charnock value was a bit too high. A sea state dependent Charnock is beneficial but its mean value might need tuning to return to right (unbiased) wind speed in the coupled system.

Reply: A sentence mentioning this adjustment has been added in P8, L11-12. We are also grateful to the reviewer for this very useful suggestion. We also noticed that WAM returns high values of the Charnock especially in wind speeds exceed the 10 ms^{-1} as it is shown in Fig. 12. Therefore, we intend to perform a series of simulations with adjusted a-hat parameter in order to check the response in the entire wind-wave spectrum.

P10, line 21: we know the advection time step is set by the CFL criteria but could you elaborate a bit more on the choice of the source term integration time step (I know it's probably a compromise between calling WAM every atmospheric time step (too expensive) and a time step that is a multiple of the advection time step but still small enough to capture the high variability of the ETA model. Note that in the ECMWF system, the relatively large atmospheric time step is used instead to determine how often IFS and ECWAM exchange information.

Reply: Time scales of the physical processes associated with the source/sink terms in WAM model (which are calculated as point processes) do not necessarily scale with grid resolution in the same way as the CFL condition imposes time step restrictions on the propagation of the wave spectrum propagation. In order to decrease the run time of WAM model for our coupled system (mainly consumed on the calculation of the source terms) we decided to use a source time step greater than the propagation time step (360 over 120 s). In our coupled model setup, the atmospheric model is using a much smaller time step (15 sec) compared to WAM implementation. To have the two sub-systems exchanging information more frequently than 360 s the only possibility would have been to drastically decrease the source time step and keep it equal to the propagation time step (120 s) increasing the CPU time needed for the system integration. At the same time we feel that exchanging information (10m winds and sea surface roughness) every 6 minutes is frequent enough to capture the atmospheric variability as represented by the ETA model.

P13, line 9: if I read you correctly, the validation against altimeter wave heights (please state so) was only done for the high-impact selected case (4-11 January 2012), then if it is so, ERS-2 was not longer available.

Reply: We got a tar ball of data from Ifremer (Globwave project) for the period 2010-2012 and the reference to ERS-2 data was accidentally remained in the manuscript. Now it is corrected.

P13, line 17: bergeron -> Bergeron

Reply: Done.

P14, line 7-8: I insist, please add Janssen 2004 as all this was already documented in Peter's book.

Reply: A reference to P. Janssen textbook has already been added in the first revision of the manuscript. It has been added at P14L9 as well.

P15, line 20:-27: your Figure 14 does indicate that for winds above 22 m/s, there is an apparent levelling-off, even a decrease of the Charnock coefficient. If what is presented is the Charnock as modelled by WAM, there is a better explanation for it. It is exactly what you expect from a sea state dependent Charnock. Those winds are for very young sea states and short fetches (i.e. around the fast turning winds). Those young waves are unable to carry the full stress that a slightly more mature sea state could. The process presented in Donelan et al. (2004) of flow separation and continuous wave breaking is not represented in WAM-cycle 4 physics, and so could not be acting. If you were to sample many more storms of different scales, you would find a cloud of points, some still indicating an increase of Charnock with wind speed (see Bidlot 2012). The apparent limitation of the surface drag for high winds is still an ongoing issue as it has become apparent that more than one mechanism is at play. In future, you might contribute to the debate with your WEW system...

Reply: We adopt the recommendation of the reviewer and we are grateful for pointing out this important issue. We added the suggested explanation in the manuscript modifying the paragraph accordingly (P16L11-26).

Table 1: add the spectral resolution of WAM.

Reply: Done.

Figures 12,13,14,15: indicate which period/date(s) they cover.

Reply: Done.

Figure 15: what are the solid lines? Why are the data points stratified?

Reply: The solid lines stand for the polynomial curve fitting to the data. In general, the data plotted over logarithmic y-axis appear similar stratified form because small differences of the data are grouped on the same value.

Finally, referring to Figure 14, I hinted in my previous review that ECMWF has made some changes to the numerical scheme of ECWAM and this can remain outside the paper correction. I did not mean the physics (i.e change around the source terms) but adjustments that made the calculation more accurate. In the case of Figure 14, the apparent increase in Charnock around winds of 6 m/s can be explained by the lack of frequency resolution in the spectrum at high frequency because of the logarithmic frequency spacing and the choice of cut-off frequency. This deficiency affects how the wind input is determined. There is a fix in ECWAM that relies on extending how the parameterised high frequency wave induced stress is determined to slightly lower frequency (Jean Bidlot personal communication). With the frequent exchange of information between the atmosphere and the wave model, it was also found that time integration scheme for the source terms had to be made fully implicit and modified to account for an update on the calculation of the wave induced stress after the update of the spectrum. This change had a beneficial impact, in particular in cases of rapid decreasing wind speeds when otherwise too large Charnock values could be returned to the atmospheric model.

Reply The reviewer is pointing to three very useful and interesting adjustments to the WAM code that could potentially improve model performance. As one of them (additional calculation of the wave induced stress after the update of the spectrum) is already used in our implementation of WAM model we intend to test and apply the other two fixes in future upgrades of the coupled system after we contact Dr. J. Bidlot to make us available his fix accounting for the deficiency caused by the lack of frequency resolution at the high frequency part of the spectrum.

Anonymous Referee #2

Although I am still not particularly impressed by the results, I am willing to accept the paper as the authors have responded to the comments of the reviewers.

The authors should discuss the very small differences between the coupled and uncoupled run shown in Fig 12. After that I am willing to accept the paper.

Reply: The paragraph below has been added in P14L15-P15L24 as a response to the reviewer’s comment.

Despite the known problems of the issues associated with comparing point measurements with area-averaged predictions, the in situ measurements from the buoy network are valuable in providing wind data for comparing the error statistics between the uncoupled and coupled simulations. Fig. 12 summarizes the main statistical scores for both simulations. As indicated in Figure 12a both simulations slightly underestimate the near surface wind speed (negative bias scores). Although the CTRL gives less biased wind speed estimation than WEW, the latter exhibits a slight improvement of the RMS error by approximately 2%. Additionally, WEW reduces the standard deviation of the model towards that of the buoys measurements. In accordance with the wind speed, the bias scores of the SWH indicate an underestimation which is more prominent in the WEW simulation (Fig. 12b). However, WEW exhibits an overall improvement of more than 7% regarding the SWH RMS error, with 0.53 instead of 0.57 m, and better correlation coefficients.

The respective error properties are quite similar in the open sea. Comparison with the remote sensed data referenced in this section, showed that WEW has slightly better statistics (e.g., lower RMS error) than CTRL, despite the fact that it seems to enhance the underestimation of the wind speed and the SWH. In particular, Fig 12c indicates that WEW tends to increase the underestimation of the wind speed already present in the CTRL, reducing the respective RMSE by 1.5% at the same time. Also, Fig. 12d shows that the RMS error is smaller for WEW SWH values compared to CTRL values by almost 11%, in contrast to the slight overestimation of the CTRL SWH and the slight underestimation of the SWH occurring in WEW. The error statistics are significant at the 95% confidence level. Although WEW increases the wind and the SWH underestimation, it overall improves the SWH RMS error by approximately 7% against buoys data and by 11% against remote sensed data. In contrast to the bias scores, RMSE penalizes the variance between in-situ or remote sensed data and the simulations implying a deterioration of the RMS error in CTRL run (Chai and Draxler, 2014). Similar RMSE improvements by the coupled systems have been also confirmed in the relevant literature (e.g. Lionello et al., 2003 and Renault et al., 2012). Moreover, in a parallel to WEW research effort within the MyWave project the Italian team consisting of the Institute of Marine Sciences (ISMAR) and the Italian Meteorological Service (CNMCA) coupled WAM with the COSMO atmospheric model over the Mediterranean Sea (at a lower horizontal resolution though) showing similar results especially in terms of winds and significant wave height RMSE reduction (Torrisi et al., 2014). Overall, WEW offers a more realistic representation of the air-sea interaction processes although it is not reflected in an exceptional improvement of the statistical scores. This is attributed to the fact that the application of the two-way fully coupled system can generate and support a more realistic near sea surface atmospheric circulation pattern by fully resolving air-sea interaction mechanisms at the relevant interface, including the wind speed regime and wave patterns.