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Interactive comment on "Modelling deep-water formation in the North-West Mediterranean Sea with a new air-sea coupled model: sensitivity to turbulent flux parameterizations" *by* Léo Seyfried et al.

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Response to Referee #2

The paper has been revised according to the comments from the reviewers and we thank both reviewers for their very helpful comments and suggestions. Our point-by-point response is inserted in the reviewer's comments.

Reviewer's Comment: This paper presents a new modelling system consisting in a coupled ocean-atmosphere model and show some results regarding deep-water for-

mation events in the North Western Mediterranean. Additionally the authors run some sensitivity experiments to show the impact of the choice of flux bulk formulas. I think the paper addresses an interesting topic, is well written and the results are interesting. Therefore I recommend it for publication after some issues are addressed. I have recommended a major review because there are many small issues to address, even if none of them are critical.

In general I think that some more details should be provided in what regards the modelling system description and the different bulk formula that are used in the paper, as these are key aspects to understand the results.

Authors' Answer: Additional information on the coupling platform is now given in the introduction (see detailed comments below). In section 2, the differences between the 3 parameterisations are better presented and discussed (see detailed comments below).

RC: Another issue is that I think the results are not discussed in depth. For instance, an important question that is now present in the modelling community is what is the role of high resolution on the modelling of these type of processes. In this sense, it is not clear to me what part of the improvement brought in this modelling system is due to the high resolution and which part due to the air-sea coupling. Some discussion on this aspect would be appreciated.

AA: We agree that these issues are really important for the modelling community. However, the present study was not designed to study to role of the coupling and can not provide very relevant answers with that respect. A preliminary uncoupled experiment (not included in the paper) suggests that in absence of coupling the heat fluxes could be strongly overestimated during the preconditioning of deep ocean convection. These results are consistent with previous findings (eg. Lebeaupin Brossier and Drobinski, 2009; Small et al., 2012; Renault et al., 2012). However, it is not straightforward to distinguish the improvement resulting from the coupling itself (ie from its different feedbacks) from the one resulting from a more accurate atmospheric forcing or a more accurate flux parameterisation. This would require a series of carefully-designed experiments in which the current coupled system would be step-by-step downgraded into an uncoupled system till it would exactly mimic the behaviour of the atmospheric and ocean models in their stand-alone configuration. Currently, this type of work is hampered by the fact the surface fluxes are computed on the atmospheric grid and not on the oceanic grid.

Most of these arguments are now given in the conclusion: “However, this conclusion regarding the good performance of the MOON flux parameterization needs to be further consolidated.

First the present results were obtained with a coupled system. They could probably be different with uncoupled simulations. In air-sea coupled simulations, the interactive evolution of ocean and atmosphere influences the turbulent heat fluxes, which themselves modify the atmospheric and oceanic surface fields involved in the flux calculation. In statically unstable Mistral and Tramontane conditions, if the sensible (or latent) heat flux increases, the vertical temperature (or humidity) gradient is reduced, which in turn limits the increase in the sensible (latent) heat flux. It is likely that these feedback loop effects tend to limit the discrepancies induced differences between by the different parameterizations. The results of partial and preliminary uncoupled simulations (not shown) suggest that these discrepancies could be larger than in the coupled simulations. It would be therefore of great interest to disentangle the effect of the flux formulation from the effect of the air-sea coupling and to check whether the MOON parameterization still improves the results in uncoupled conditions. However, it is not straightforward to isolate the coupling effect in a clean and rigorous way. This requires a series of carefully-designed experiments in which the current coupled system is step-by-step downgraded into an uncoupled system till it exactly mimics the behavior of the atmospheric and ocean models in their stand-alone configuration. In our current system, this type of study is hampered by the fact that the surface fluxes are computed on

the atmospheric grid, ie at a coarser resolution that the one used by the ocean model.

The differences in resolution between the atmospheric and ocean models (10 and 1 km, respectively), though partly justified by scale considerations, is also a debatable question. A further development will thus investigate the sensitivity to the resolution of the atmospheric model. In the present configuration, the atmospheric model does not have the possibility of representing scales fully adjusted to that of the oceanic model. In particular, with a 10 km resolution, the local maxima and horizontal gradients of the surface parameters are probably too smooth, which may affect the air-sea interactions especially in the vicinity of the oceanic front (Small et al., 2008) and could also modify the response of the coupled system to the different parameterizations.

In addition, the role of the waves necessitates further investigation. In our study, the waves are not considered in COARE and MOON and only indirectly accounted for in ANDREAS. In ANDREAS, the depth of the spray layer is computed as a function of the significant wave height (Andreas et al., 1995). The latter is rather roughly estimated from a simplified parameterization based on wind speed (Andreas and Wang, 2007). Similar crude relationships are used in COARE3.0 for the wave height and wave period. Another envisaged development will couple the current system with a wave model (Michaud et al, 2012) and revisit the results obtained with the ANDREAS and COARE3.0 parameterizations.

RC: Also, the atmospheric domain looks relatively small so I wonder if the good results of the atmospheric parameters aren’t induced by the lateral boundary conditions. Again, what is the role of the coupling in the good quality of the results? Could one obtain similar quality using uncoupled models?

AA: The size of the atmospheric domain is typical of the one used in the field of Numerical Weather Prediction. The fact that the model is forced at its lateral boundaries with an analysis (as opposed to a larger-scale forecast) certainly contributes to improve the results. However, most of the improvement (as compared to regional climate results) is likely due to a better resolution of the topography and thus of the regional winds such as tramontana and mistral which are strongly controlled by the terrain. Further improvement is even expected from a higher resolution of the atmospheric model (on going work) since a 10 km resolution is still insufficient to accurately represent the atmospheric deep convective systems.

RC: Finally, you have shown that the choice of bulk formula have small impacts on the evolution of each parameter but a huge impact on the dense water volume formed (for instance). In your opinion, what should be done to improve the parameterizations? What kind of observations would help to improve them?

AA: The development of accurate flux parameterization in strong wind conditions is an area of research in itself. First, measurements in severe weather are difficult, often inaccurate and/or incomplete (e.g. simultaneous sea state observations are missing). Additional dedicated field campaigns together with wind-water tunnel experiments (Andreas et al. 2016) would certainly help to go one step further. Second, the physical processes taking place in the diphasic surface layer are complex and may be not fully understood yet. Third, there is still a large gap between our understanding of theses processes and our ability to represent them in numerical weather predictions models. This is why, as atmosphere-ocean modelers, we should remain particularly attentive to the most recent developments and multiply the experiments to test and evaluate new propositions, would they be fairly pragmatic and model-based (as Moon's) or more sophisticated and physically-based (as Andreas').

RC: Page3 L4-7. As the paper has an important technical component it would be good to provide more details on the platform.

AA: Added/modified text in the introduction “These issues, among others, have motivated the recent development of a new coupling platform (SURFEX OASIS3-MCT) providing better numerical tools to address the scientific and technical questions related to ocean-wave-atmosphere coupling (Voldoire et al., 2017). This coupling platform is

based on a external multi-surface model SURFEX (Masson et al. 2012) and on the OASIS3-MCT (Valcke et al., 2015) coupling interface. SURFEX computes the surface-atmosphere fluxes over four surface types (land, town, ocean and inland waters) and can be used in a stand-alone version with prescribed atmospheric forcing or embedded in an atmospheric models. The use of OASIS3-MCT allows SURFEX to be linked to various other models including ocean, atmosphere, hydrology, waves and sea-ice models. This generic coupling strategy based upon an externalized surface model ensures that the surface flux computations are done in a consistent way, independently of the models to be coupled. As illustrated in Voldoire in 2017, this strategy has greatly facilitated the coupling of the different models developed in the French community, including the coupling of the MESONH atmospheric model (Lafore et al., 1997) and the SYMPHONIE ocean model (Marsaleix et al., 2008,2009, 2012)"

RC: P3 L7-9. Please, provide more details on what are the conclusions of those studies. Why the air-sea coupling is beneficial? What is it providing? Introduction. I think that the interest of using a coupled system to analyse DWF should be better presented.

AA: Added text in the introduction: “Regarding the atmospheric forcing, the benefit of using a fully coupled system to study air-sea interactions in the numerical weather prediction models was already illustrated in previous studies based upon different air-sea coupled systems (eg Lebeaupin Brossier and Drobinski, 2009; Small et al., 2012; Renault et al., 2012). These studies have shown that coupled simulations provide a better representation of atmospheric and oceanic surface parameters compared to uncoupled simulations. In particular during strong wind events coupled simulations capture the rapid SST cooling more accurately, which makes the atmospheric boundary layer more stable and reduces the heat and moistures exchanges. It is likely that this improved representation of the atmospheric forcing could also lead to an improved representation of the deep water formation.

Besides the question related to coupling, there is still significant uncertainty as to the choice of a relevant parameterization to compute the turbulent fluxes for strong wind conditions such as the Mistral and Tramontane. Current parameterizations have been carefully assessed and validated against large data sets. However, due to the limited number of available observations in strong wind conditions, they are known to be inaccurate for wind speeds exceeding 20 $m\ s^{-1}$ (e.g. Hauser et al, 2003). The sensitivity tests performed by Estournel et al., 2016b, suggest that the uncertainty associated with the turbulent flux computations could have a strong impact on the deep water formation process in the NWMS."

RC: P3 L30. How many levels are close to the surface?.

AA: 52 terrain-following vertical levels stretched from 15 m to 15000m, with 16 of them in the first km. This information is now given in the text.

RC: P3 L32. The modelling of convection is of paramount importance in this paper. Thus, more details on how this is parameterized should be included.

AA: Added text “In the case of the ocean, the vertical diffusion is parameterized following Gaspar et al. (1990) with a prognostic equation for the turbulent kinetic energy and a diagnostic relation for the mixing and dissipation lengths. A 1-km resolution is still too coarse to explicitly resolve convective plumes, which thus need to be parameterized. Different parameterizations have been proposed (e.g., Marsland et al., 2003). The most common and basic one consists in artificially increasing the vertical diffusion coefficient in statically unstable layers (eg Waldman et al., 2017). In our case, the heat and water fluxes are linearly distributed over the whole mixed layer, the depth of which is given by the depth at which the vertical density gradient becomes negative. By doing so the first level under the surface does not support the entire amount of heat loss by itself, which prevents the development of static instabilities at the surface. Furthermore, this parameterization is consistent with the nearly linear vertical variation of the buoyancy flux in the convective layer (Deardorff et al., 1969).

RC: Section 2.1. Please, give more details about SURFEX. For non-expert readers its role in the modelling system is confusing.

AA: More details about the role of SURFEX are given in the introduction (see above)

RC: P4 L5. A "3" is missed in OASIS-MCT

AA: Corrected

RC: Section 2.2. P4. L23. The different parameterizations used are for Cd, Ch and Ce? Please, be more clear in the description of the parameterizations and include more details. This is also a very relevant part of the paper and the reader needs to know what are the differences between the different options.

AA: Modified text in section 2. Turbulent air fluxes at the air/sea interface are computed from bulk type parameterizations based on the Monin-Obukhov similarity theory (Foken, 2006). These parameterizations compute the turbulent fluxes as

$|\tau| = \rho_a u^{*2}$

$H = -\rho_a C_p u^* \theta^*$

$LE = -\rho_a L_e u^* q^*$

where τ is the momentum flux, H the sensible heat flux, LE the latent heat flux; ρ_a the air surface density and where u^*, θ^*, q^* are scaling parameters for momentum, potential temperature, and humidity, respectively. The momentum scale, u^*, is referred to as friction velocity.

Classically, the scale parameters are expressed as a function of the vertical gradients of the mean fields at the air-sea interface, the surface roughness and the atmospheric stability. Although based upon the same formalism, the turbulent flux parameterizations differ in the way they specify the different roughness lengths and the so-called stability functions. In particular, the validity of the Charnok’s formulation (Charnock, 1955) which is generally used to relate u^* to the dynamic roughness length has often been questioned for strong wind conditions.

In this study, three well-established parameterizations have been used:

- The COARE3.0 parameterization (Fairall et al., 2003) is one of the most widely used in the modelling community. This parameterization derives from the COARE 2.6 algorithm (Fairall et al., 1996) originally developed from the observations performed during the TOGA-COARE experiment (Webster et al., 1992) in the North Pacific. An important upgrade in COARE3.0 is a new formulation of the surface (dynamic and scalar) roughness lengths which slightly increases the fluxes for wind speeds exceeding 10 $m\ s^{-1}$. Although COARE3.0 has been validated against a much larger data set (~ 7000 observations) than the one used for COARE2.6 , COARE3.0 remains mostly reliable for wind speeds below 20 $m\ s^{-1}$ due to the limited number of observations available in strong wind conditions. It is worth noting that the influence of waves (available as two possible options in COARE3.0 but not extensively validated) was not activated in our study.

- The ANDREAS parameterization (Andreas et al., 2015) is a novel and more physically-based approach which distinguishes two different contributions to the turbulent heat fluxes: the standard air-sea interfacial fluxes controlled by molecular processes right at thee air-sea interface on the one hand, and the sea spray fluxes controlled by microphysical processes around sea spray droplets on the other hand. As opposed to the COARE-type algorithm, the friction velocity used to compute the interfacial fluxes is parameterised as a function of the 10 m wind speed at neutral stability, eliminating thereby the uncertainty associated with the definition of the dynamic roughness length and the use of the (Charnock, 1955) expression. The sea-spray contribution becomes notable only for wind speeds exceeding 13 $m\ s^{-1}$. Small droplets are then ejected by surface waves into the atmospheric surface layer. They cool, evaporate and can significantly contribute to the air-sea exchanges of heat and water. The sea spray fluxes are computed using the fast microphysical algorithm described in (Andreas et al., 2005). ANDREAS parameterization has been established with a data set of ~ 4000 observations with wind speeds up to almost 25 $m\ s^{-1}$.

- As opposed to COARE and ANDREAS, the MOON parameterization (Moon et al., 2007) mainly relies upon model results. It has been developed based upon the results of a coupled wave-wind model. The simulations of 10 idealized tropical cyclones have been used to derive a new expression of the dynamic roughness length, which limits the increase of friction velocity with wind for wind speeds exceeding 12.5 $m\ s^{-1}$. This new formulation was indirectly validated using the Geophysical Fluid Dynamics Laboratory coupled hurricane–ocean prediction model (Kurihara et al., 1998). For 5 hurricanes observed in the Atlantic Ocean, the new formulation lead to better results than the former one (based upon the Charnock's formulation) with a clear improvement of the cyclone intensity and no degradation of its track and central pressure.

RC: P5 L8 "They also allow the impact of the sea spray in ANDREAS to be distinguished". I don't understand this sentence. Could you please clarify the text here?

AA: Text replaced by “Although not used further in the following, the results of ANDREAS without sea spray effect (ANDREAS no-spray) have been added to assess its impact”

RC: P7.L6. Please, summarize the conclusions of Estournel et al. (2016a).

AA: This comment was referring to the following sentence : “Sensitivity to the initial state is not discussed here as it has been the subject of a thorough study by Estournel et al. (2016a).” which has been now removed as the conclusion of Estournel et al 2016a regarding the sensitivity of model results to the oceanic initial state was already summarized P7.L1 “. Estournel et al. (2016a) show that this correction is necessary to properly simulate the preconditioning phase and the triggering of the convective phase.”.

RC: P7.L28-32. I think this paragraph is too pessimistic. The agreement between different time series is very high and differences are not so large .

AA: Rephrased sentences: “For the other surface atmospheric parameters (2 m air temperature and relative humidity), slightly larger discrepancies are found from one simulation to another. Air temperature and humidity remain relatively close to observations in terms of correlation (respectively 0.98 and 0.85, Table 2). Bias and root mean square error exhibit larger but still weak differences between simulations. The largest difference is found for humidity. In particular, it is clear from Fig. 4 c that the moisture drops associated with the strong wind episodes are more pronounced in COARE and ANDREAS than in MOON”

RC: P8 L1-2. Conversely I think that the extremely high correlations in the SST are over optimistic and due to the seasonal cycle.

AA: Rephrased sentences: “This correlation is mainly due to the representation of the seasonal cycle and to the weak variability of the SST during the winter period when the SST ceases to evolve. The drops of SST associated with the events of Tramontane and Mistral in autumn are well captured by the three simulations.”

RC: P8.L5-L7. Can you do a rough estimate of what is the relative importance of each mechanism (local process vs advection) ?

AA: A rough estimate is provided by Estournel et al 2016a. Integrated during the autumn period the advection process in mass budget represent about 40% compared to local process. This information is now given in the text.

RC: P8.L9-10. I think ANDREAS shows at least comparable skills with respect to MOON.

AA: Rephrased sentences: “Nevertheless, it can be concluded from Figs 4 and 5 and Table 2 that in general the results of the MOON and ANDREAS appear to agree with the Lion buoy better than the results of the COARE do and that MOON slightly outperforms ANDREAS .”

RC: P8. L23-25. I don’t understand this. It looks from the figures that differences be-

tween simulations are larger during the peaks. How can you deduce that the feedback mechanism is playing a significant role?

AA: This discussion has been moved to the conclusion where the potential impact of coupling is now discussed in more details.

RC: P9. L5. I agree MOON provides the best agreement, but it is just slightly better. Considering the simulation period is relatively small I think you should moderate that statement.

AA: Rephrased sentence: “Although the differences remain fairly weak, as reflected by the statistical analysis, in our coupled system, the MOON parameterization gives the best agreement with the available observations”

RC: P13. L13. ".. demonstrates that XXX are strongly ..." . XXX - Something is missed.

AA: Replaced sentence: “In addition to air surface temperature and moisture, sea surface temperature is also strongly sensitive to the turbulent flux parameterizations.”

RC: Conclusions. I don’t see that MOON is really outperforming the other parameterizations. For instance, for the SI on Leg-2 COADS seems to produce better results.

AA: It is true that the MOON bias are not the best ones for DEWEX-Leg2. The conclusion regarding the good performance of MOON has been soften and is also now supported by the analysis of the root mean square errors (which have been added in Table 3).

RC: Figure 1. Define in the caption what is DWF and NBF.

AA: Done

RC: Figure 2 . What is each subplot? What is the x-axis?

AA: This figure has been redrawn with axis labels.

RC: Figure 3. What are the colours in (e) ? Isn’t it redundant to use them in a time-depth plot?

AA: The colours in Fig. 3(e) are similar to the ones used in Figs 3a-d and are defined with the colorbar, This color information is redundant in a time-SI plot, but in our opinion helphelp the visualization.

RC: Figure 4. What are the grey bars in the plots?

AA: The grey bars correspond to the strong wind periods (hourly wind speed $> 15\ m\ s^{-}1$). This information is now given in the caption.

RC: Table 1 "sigMa"

AA: Corrected

RC: Table 3. Include the averaged SI index obtained from observations, so the biases can be better interpreted.

AA: Done

Please also note the supplement to this comment:
https://www.ocean-sci-discuss.net/os-2017-43/os-2017-43-AC2-supplement.pdf

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