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Interactive comment on "Characterization of the air-sea exchanges mechanisms during a Mediterranean heavy precipitation event using realistic sea state modelling" *by* César Sauvage et al.

Anonymous Referee #1

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Review of: Characterization of the air-sea exchanges mechanisms during a Mediterranean heavy precipitation event using realistic sea state modelling César Sauvage 1 , Cindy Lebeaupin Brossier 1 , Marie-Noëlle Bouin 1,2, and Véronique Ducrocq 1

General comments

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This paper describes a case study assessment of the introduction of an interactive wave simulation to describe sea state as a lower boundary to an atmosphere model

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simulation of a heavy rainfall event over the Mediterranean. The Introduction and Model Description sections are in general very clear and efficiently set out. Results are compared with a comprehensive set of surface and satellite-based observations of atmosphere and wave variables. As set out below, aspects of the experimental design and dependence on a single case limit the extent to which the paper can add value to the existing literature, and the authors are encouraged to consider this further. Overall, the paper is generally written to a good standard, is relevant and has scientific merit.

I am content that the paper should be published following some minor, but significant(!), suggested corrections and considerations as set out below. Each in themselves are perhaps worthy of 'major' corrections and further work, as acknowledged by the authors in the concluding paragraph, but perhaps it is sufficient that the choices made are more directly addressed and justified within the current paper rather than recommending a more significant re-write and further simulation and analysis work.

Specific comments

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1. Wave model coupling approach

Section 2.3.2 – wave model coupling. The authors describe the use of the WASP parameterisation of the surface roughness and coupling via the wave model simulated peak period. This seems like a rather indirect approach, given that more typically the WAVEWATCHIII calculated Charnock parameter could be used directly into Eq. 7 (e.g. Varlas et al., 2018; Section 2). In fact, Wahle et al. 2017 pass the WAM wave model calculated roughness length directly (see their Section 2.3). The direct use of WAVEWATCHIII computed Charnock parameter was also described for example in coupling studies of the North West European shelf by Lewis et al (2018, 2019).

Another study cited, Renault et al., 2012, apply a similar wave-age dependent coupling (their Section 3.5), and here reference to Drennen et al., 2005 might be appropriate. Further, more detail of p5,l30 ("coefficients A and B being polynomial functions of the surface wind speed") would be useful.

The authors should set out their rationale for the WASP parameterisation in preference to the wave model computed Charnock or roughness. Indeed, a comparison between the WASP and Wavewatch computed roughness would have been a very enlightening addition to this discussion and of wider use for assessing potential modelling uncertainties for the community. In short, what is the sensitivity of results (roughness lengths) to this configuration choice?

2. A-W coupling experiments

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Section 2.4 – set of experiments. The authors set out the 4 (WY, AY, AWF and AWC) experiments. On the one hand, this is a justifiable and clean experimental design. However, given the increasing use of more fully coupled atmosphere-ocean-wave regional configurations for similar case study assessments (e.g. Renault et al, Ricci et al, etc), the authors should more directly justify the lack of ocean interactions within the current study. This is highlighted in the final paragraph of the paper, but should also be addressed directly in the choice of experiments described in Section 2.

Finally, please comment on expected sensitivity of results to the choice of coupling frequency (1h). Were any sensitivity tests conducted to assess this? Some studies (e.g. Renault et al., though many others exist), involve interactions at much higher coupling frequency, to capture interactions with fast moving systems for example.

3. Simulation lead time considerations

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The authors chose to validate only the first 24h of each ARMOME simulation in Section 3, though simulations covered T+0 to T+42. Why are data beyond the first day not

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considered? Similarly, the focus in Section 5 is on T+14 and T+24 snapshots only.

Converse to this, would you expect the impact of wave interactions to perhaps grow with time (some spin up effect) if all regional simulations were initialised from the same operational analysis? Please also comment on the time taken for 1.3 km scale high-resolution details to spin up within the model domain. This spin up effect may help explain the rather similar results shown in Fig. 2.

Wave results for AWF seem slightly degraded relative to WY in Fig 2., but not commented on. Is there some explanation for the different behaviour?

In Section 5, what is the sensitivity to model lead time? Presumably there are periods of overlapping data from different model start times for these periods of interest? Does the influence of wave interaction grow with lead time, or are results dominated by increasing errors? Authors state that "differences between three simulations were well established", but are you confident differences were spun up?

In general, all simulation results seem to be essentially similar, and it is difficult to assess how much this is a true reflection that the systems are not very sensitive to wave interactions (a null result, which should be more explicitly captured in the Abstract), or a symptom of the experimental design. Authors should be clearer in their discussion on this.

4. Discussion of precipitation differences

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The overall conclusion from Section 5.2 appears to be that simulations were "about the same". It is again difficult to judge the extent to which differences just reflected expected variability in the simulation (e.g. as might be reflected in an ensemble of simulations of the case), and how much any differences could be attributed more physically to changes in low-level flows and heat fluxes previously described. P13, l33 should therefore be expanded to provide a more qualified discussion of how "this dis-

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placement was directly linked to. . ..". I am otherwise left with an impression that the precipitation differences are somehow 'random' and could equally be produced with some other change in (e.g.) model parameters, initial condition etc. There is some attempt at this in the summary from l5, p14, but this could be more explicitly set out. For example, phrases like "due to differences in terms of heat fluxes. . ." is too vague here to help the reader follow the physical arguments being discussed.

It might be instructive to discuss the relative sensitivity of the system, e.g. with reference to any operational ensemble information available at the time of this particular study, to set some context.

5. Dependence on a single case study

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It is difficult to assess the significance of this paper to a general readership and to the community, given that it addresses only a single case study. However, it is equally not clear how many such cases would need to be considered before some robust statistics are achieved, and the key physical mechanisms are lost in the number of cases addressed – it would be a quite different paper in fact.

The authors should however be clearer, perhaps in both the methods and discussion sections, on the relevance of the single case to wider improvement of understanding and simulation quality for the region. What can operational centres learn (if anything) from the study for development of forecast model configurations for example? Suggesting that further cases are considered in a similar manner would fundamentally change the submitted manuscript, so is not recommended by this reviewer, but the limitations of the single study (particularly assessed in a deterministic framework) should be more openly acknowledged and discussed. Further, discussion of how the current paper adds value beyond some earlier work in the region (e.g. Renault et al, 2012 and later references) would be welcome.

Technical corrections

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P6,l23 – do you mean $\frac{1}{2}$ deg. or perhaps 1/12 deg. global WW3 resolution model? Could not work out if the boundary conditions were rather coarse scale (and if so, please comment on any boundary spin up issues into much higher resolution system), or if a typo.

P6, l27 – please comment if SST is updated daily (as implied) during the simulation, or a fixed SST is used throughout the 42h simulation? One might again expect precipitation fields to be rather sensitive to details (e.g. resolution, updating frequency) of the SST field in this case (e.g. Lebaupin Brossier et al, 2006; 2008) – authors should comment. Useful to also confirm if any surface currents information is used, or if assumed to have a stationary sea surface?

Fig. 9, 10, 11, 12 – would be clearer to plot impact of interactive coupling differences as (AWC – AWF) in panels b) and d), given panels a) and c) establish differences of AWF to AY. The additional impact of coupling here is the inverse of what is currently shown, so is a bit confusing to follow. For example, in Fig. 12, are the AWC differences just the inverse of AWF to AY (such that AWC is more similar to AY than AWF?), or is the main rain area further displaced again?

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