Referee #2: David Woolf

Q1. The study investigates the relationship of extreme wave heights to atmospheric modes based on a high-quality wave hindcast. The study is described concisely and clearly. There are not any great revelations, but a useful study is reported fairly and competently in an appropriate form and to a suitable journal. I have some detailed comments, which are given below, but I am content for a revised version to be published after reasonable attention to all reviewer comments.

> *Reply #1. We sincerely thank reviewer for his constructive comments and the effort that he made in reviewing carefully our work. We deeply think that thanks to this revision the new version of the manuscript (hereinafter Ms.) improved significantly.*

SPECIFIC COMMENTS

Q2. I do not have a problem with calculating the linear trends by a simple method and reporting these (Figure 4 and associated text), But I'd urge caution in interpretation. Firstly, there is likely to be some autocorrelation in the atmospheric forcing (and thus wave heights), which makes the independence of values assumed in simple regression doubtful. Secondly, the particular time period, 1979-2009 is pertinent; different periods would show different patterns.

> *Reply #2. We totally agree with the referee's comment. We consider relevant to write a new sentence in the Ms. in order to clarify this issue (lines #116-117):*
>
> > *"These calculations are restricted to the time period corresponding to the atmospheric forcing of the WAVEWATCH III and different patterns could be obtained for different periods".*

Q3. A similar weakness in estimation of statistical significance is apparent in the use of "t-values" for the relationship of PCs to climate indices (lines 130-136), but again that is a minor objection and should not discourage publication. It is possible to take the statistical analysis further, for example through a wavelet analysis of the wave height - climate index relationship (I have seen this for sea level, but I am not aware of such an analysis for wave heights), but that is for another paper.

> *Reply #3. We agree with the referee's comment again. The lines #151-152 of the Ms. have been rewritten in order to note this issue, as:*
>
> > *"These significance values are particular for this study since they depend on the data used and the analyzed time period."*
>
> *We agree that a wavelet analysis would complement these kind of studies. In order to keep the focus of the work we prefer to perform them in a new Ms. However, we pointed this comment in the Summary and conclusions section. Lines #290-292 of Ms. read:*
>
> > *"For future studies, a wavelet coherence analysis (Torrence and Compo, 1998) between the main climatic indices and the extreme waves will provide additional information on the dominant modes of variability and how they vary in time."*

Q4. My general impression is that, as for some previous studies, the relationship of extreme waves in the North Atlantic to NAO is very convincing, but (albeit with calculated significance) the other relationships are rather weak (possibly with the exception of SCAND).

> *Reply #4. Yes, the NAO index is the most relevant climatic mode of the North Atlantic Ocean, being noteworthy the effect of the SCAND index. However, we include the rest of climatic indices (EA and EA/WR) analysis because they have a significant influence on the extreme waves in the Mediterranean basin. And, as we mention in the Ms., we use the North Atlantic Ocean study as a "validation" of the methodology developed and then applying it in the Mediterranean Sea, since the North Atlantic wave climate has been largely analyzed in previews studies.*

Q5. It seems to me to be open to debate if we understand the extreme wave heights better from modest correlations to atmospheric indices. The relationship to composite was more interesting and in some respects was more convincing. For example, the relationship to EA is physically sensible and quite satisfying in this form.

Reply #5. We agree with the referee in this aspect, we think that the composite analysis is the most innovative and interesting study in this paper, being a more suitable way to relation the climatic modes with the extreme waves. However, the proposed methodology needs to compute the correlation in order to select the spatial point where the climatic patterns have a higher influence. In addition, it can be useful for the better understanding of the relation of the climatic indices and the extreme waves for the reader.

Q6. The sections from lines 193-217 and from 219-246 are rather monotonous and not very effective in communication. I suggest finding a more engaging method of communicating this information, perhaps a Table?

Reply #6. We are not sure if the referee talks about the description of the composites or about the relation between the composites and the EOFs. If it is the first case, we consider these sections as descriptive (we only describe the composite results; Fig. 9-12), being the composite figures the clearer and concise way to transmit the information. If, on the other hand, he refers to the relation with the EOFs and PCs, the table which show that is the Table 2 (in new Ms., old Table 1) where the correlation between the main climatic indices and the principal modes appear. We want to remark that we obtain the same results if we compare the signals of PCs with the different climatic indices and the EOFs maps with the composites.

Q7. I do not have any strong objections to the content of "Summary and conclusions" though I can be counted as a sceptic regarding simple projections of NAO behaviour and their utility in projections of extreme waves.

Reply #7. We think that the climatic indices signal is more stable than the extreme waves, and it can be a suitable way to know any probable behavior of them. However, we are aware that when we mention the future projections, we are talking about statistics, but if we improve somehow the extreme waves prognostic, this paper will have met its target.

Q8. The abstract adequately describes the topic and principal results, but gives no explanation of the methodology beyond "31-year wave model hindcast". I suggest adding another sentence.

Reply #8. The following sentence has been added in lines # 7-10 of the new Ms.,

> *"A new methodology for analyzing the atmospheric signature associated with extreme waves is proposed. The method obtains the composites of Significant Wave Height (SWH), mean sea level pressure (MSLP) and 10 m-height wind velocity (U10) using the instant when specific climatic indices have the stronger correlation with extreme waves."*

Q9. Line 64-65. "we assume that wave climate is constant for 3 hours". I interpret that phrase as an assumption of an autocorrelation period of 3 hours, is that correct? Was the data analysed to reach this conclusion? Does it have any implications beyond simply informing using 3-hour data?

Reply #9. For a limited period of time and in a particular geographical region, wave conditions vary in a stationary way. For this reason, it is commonly assumed that a sea state remains stationary for 3-6 hours. The objective of the sentence in the Ms. is only to explain that the time step is suitable in order to do the presented analysis in this paper, since we follow the common physical assumption of the wave state where the waves features are stationary during 3 hours (the time step of our dataset).

Q10. Line 76 and following: There are ~240 3-hour values in each calendar month. How exactly is the 99th percentile calculated? (An interpolation between the second and third highest values for each month?)

Reply #10. The 99th percentile is calculated as the value that only the 1% of the monthly data exceed it. Therefore, this value is interpolated between the second and third highest values for each month since we have 1 data each 3 hours (with ~240 data/month only 2.4 data exceed the 99th percentile value).

Q11. Line 82. How good is a fit of annual and semiannual sinusoidal to the seasonality? Was there any analysis for higher harmonics?

Reply #11. Fitting the seasonality of the monthly SWH_{99} series as a cosine function through a least squares adjustment, we remove the annual and semiannual frequencies in the both basins. We check this method with a fast Fourier transform analysis, comparing both signal (with and without seasonality) obtaining that the annual and semiannual periods are removed. In addition, we verify that there is any dominant higher frequency in time series. This is because when computing the extreme waves any periodicity inside a month is erased.

Q12. Line 93. spelling "cyclogenetic"

Reply #12. This has fixed.

Q13. Line 95. Perhaps change "adjusted though a first order polynomial : : :" to "fitted by a linear regression in time"?

Reply #13. This has been fixed.

Q14. Line 101. Change "northern" to "north".

Reply #14. This has been fixed.

Q15. Line 116. "periodicity : : : around 5 years" Not wrong, but perhaps risky? I would generally avoid talking about periodicity unless there is a very strong case.

Reply #15. We modified the Ms and we now explain that we computed it through a FFT analysis of the PC. For that, we add this information in #129-130 lines of Ms. as,

> *"The first EOF, which explains a 28.5% of the winter SWH99, presents a periodicity in its PC around 5 years (calculated through FFT analysis of Fig. 5-1)."*

Q16. Line 145, ": : : being the rest of correlations marginal". I could not make sense of this line!

Reply #16. We want to remark that the rest of the correlations are not significant. However, we agree with the referee's comment in that this line is misleading, and thus we remove this part of the sentence.

Q17. Line 196 ": : : leads a wind jet". I suggest "this composite is characterized by a strong westerly wind stream : : :" N.B. A similar relationship was demonstrated dynamically by Wolf and Woolf (2006; GRL 33(6)).

Reply #17. This has been changed.

Q18. Add gratitude to NCEP and NOAA CPC for data in Acknowledgments?

Reply #18. Thank for the referee's advice. We write the following sentence in lines #309-310 of Ms:

> *"In addition, authors thank NCEP and NOAA CPC for the free available data that have been used in this article."*