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Discussions

Interactive comment on "Wave climate and storm activity in the Kara sea" *by* Stanislav Myslenkov et al.

Anonymous Referee #2

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Review comments for "Wave climate and storm activity in the Kara sea" by Stanislav Myslenkov, Vladimir Platonov, Alexander Kislov, Ksenia Silvestrova, Igor Medvedev submitted to Natural Hazards and Earth System Sciences (nhess-2020-198)

The manuscript presents a long-term modeling data set for the Kara Sea that has been generated on an unstructured grid. The data set is used to study the mean and extreme values of the wave parameters, their statistical change in time, and their relation to the wind regime and changing ice conditions. The authors find that the peak-over-threshold data of the storms follow a Pareto distribution closely for the milder events, but the highest events deviate strongly from this theoretical distribution.

The content of the paper is interesting and suitable for the journal. Nonetheless, the methodology, and the treatment and presentation of the results are somewhat lacking.

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Discussion paper

I therefore have to recommend that major revisions are made to the manuscript before it's suitable for publication.

Major Comment #1:

You use an old physics package and have very little measurement data to validate the model. The ST1 in essentially the physics that was used in WAM cycle 3, and it's from the 1980s. WAM cycle 4 physics were introduced already in the 1990s. Typically WW3 users use either ST4 or ST6, which are more modern and developed (as I understand it) specifically for WW3 (although they have seen implemented in other models also). I'm not an expert on WW3, but you seem to use an old version of the model code. It's a real shame that you have gone through all this work to produce a long data set with an unstructured grid, but used outdated physics.

I'm not quite sure what to do here, since rerunning everything with an more appropriate model setup seems somewhat unreasonable. I guess that, ultimately, this is not a fatal flaw, but the shortcomings and reasons for the choices should be openly and thoroughly presented. The ST1 dissipation for example dissipates swell through the whitecapping formulation in mixes sea-swell conditions. This is probable relevant for your case. The possible shortcomings in the model setup brings us to the next point.

The validation of the model is very light. I can understand that good observations are hard to come by, but is there no other measurements available? No remote sensing data? Please provide some references that the satellite data are "not accurate" (L390). Satellite data are routinely used, so is there some special circumstances here? The validation you have shows that the model overestimates the highest values, but this is completely brushed over. The entire model validation needs to be reworked here.

Major Comment #2:

With POT data one usually start with the Generalized Pareto Distribution (GPD), which has a variable shape parameter (see e.g. Coles, 2001 or the wide body of original articles available). This is the standard methodology, and while some other distribution can definitely be chosen (by for example fixing the shape parameter in the GPD), this needs to be explored since it could better account for the highest values that doesn't seem to follow the distribution you chose.

Major Comment #3:

The references and general treatment of the topic is not as rigorous as one could hope, and the language also need to be modified to make aspects clearer. Grammatical issues can be fixed in a language check. One of my biggest objection is that you use the reference to Taleb (2010) with respect to your extreme value analysis. This is a popular book. Please cite actual scientific literature. Also in the wave modelling part there are several citations missing and it is just brushed over as being "standard". Also the reference to the "F-test" leaves the reader a bit unsure of what actual test is being used etc.

What is lacking more than anything is a critical view of the approach and how the shortcomings might influence the results, and a stronger connection between your results (and methods) and other existing scientific studies.

Minor Comment #1: L11, I believe the correct name is "WAVEWATCH III", not "Wave-WatchIII"

Minor Comment #2: L17, I don't understand what a "double growth" is?

Minor Comment #3: L27, To me it's not clear what it means that "99 % of the points are described by a distribution".

Minor Comment #4: L29, "twice as less" is not proper English. Please rephrase.

Minor Comment #5: The introduction is very "choppy" with short paragraphs listing different studies. A more proper way would be to summarize the themes in those studies in a way that put the current paper in perspective and support those points with references. The difference is obviously not clear cut, but in this case the readability suffers.

Minor Comment #6: L100-101, WAVEWATCH solves the action balance equation, not the energy balance equation. Also, you are missing a dot product in Equation 1.

Minor Comment #7: L110, mention the references for the ST1 physics package (it is essentially WAM cycle 3 physics). This will make is readable also for modellers not familiar with WW3 specifically.

Minor Comment #8: 111-112. The correct reference for DIA is Hasselmann et al. (1985).

Minor Comment #9: L118 “Standard JONSWAP scheme”. Please provide a reference even though it is “standard”.

Minor Comment #10: L131, As a result of what? The wind information was every hour, so you I guess this is just as a result of you choosing to output every three hours (which is totally fine)?

Minor Comment #11: L132, Significant wave height in models is not H1/3.

Minor Comment #12: L 133-134. Does the 1% probability of exeedance mean that 1% of the single waves are higher than this threshold during the 3 hour period, or that the threshold for a single wave is exceeded, on average, once in every 100 three hour block. The language is a bit ambiguous.

Minor Comment #13: I don’t see a panel b) with a histogram in Figure 2. Also, can’t the location of the wave buoy just be marked in Figure 1? You are referring to stations No. 3 and No.5, but only show one station. The scatter plot also says “from all points”. Do out have data from several stations?

Minor Comment #14: L169-170, It looks like the highest wave heights are being overestimated, and this should be addressed since you are explicitly studying high events. Add a 1-1 line on the scatter plot to guide the eye.

Minor Comment #15: L186, The 50 year return period where? Entire Kara Sea?

Minor Comment #16: L225-228, So are you calculating the mean period (or actually the spectral zero-crossing period), but then using the peak period to calculate the wavelength. Why use different parameters? And the calculation of the wavelength should be made clear in the methods section.

Minor Comment #17: L243, Is there only one “F-test”? I have a feeling that this term encompasses many different specific tests. Please elaborate.

Minor Comment #18: L248, “for two points”. Please mark the points on some map.

Minor Comment #19: L264. You say that you need to consider fetch length, but then you don’t incorporate it into your analysis. I’m not saying you have to include it, but then word the sentence a bit differently, since now the reader is expecting it to show up.

Minor Comment #20: L269, Why is a wind speed exceeding a threshold for two whole days relevant when you have a minimum imposed distance of only 9 hours between storm events?

Minor Comment #21: L307, “About 99% of the points are described by the Pareto distribution”. What does this mean?

Minor Comment #22: L332, “The probability of “dragons” doesn’t match the base distribution.” This might be because your base distribution is not suitable. You need to motivate your choice here and show how other distributions fit the data.

Minor Comment #23: L343-345, “The basic distribution ends in the range of SWH values 6.5–8 m in different sectors. Such differences are associated with the definition of freak waves in the article (Buhler, 2007).” I don’t understand this. “Freak waves” (usually) refer to individual waves that are unusually high with respect to the underlying significant wave height. It has nothing to do with high significant wave heights. The phrase “are associated with the definition” is very vague. The author is Bühler, not

Buhler.

Minor Comment #24: Discussion and Conclusions. This is more of a long summary. What I would have liked to see would have been a critical discussion about the restrictions and the applicability of the results, an a better comparison to other similar results in the scientific literature.

References:

Hasselmann et al. (1985): "Computations and Parameterizations of the Nonlinear Energy Transfer in a Gravity-Wave Specturm. Part II: Parameterizations of the Nonlinear Energy Transfer for Application in Wave Models", JPO, Vol 15. Issue 11. pp. 1378-1391.

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