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Discussions

# *Interactive comment on* "Storm-wave trends in Mexican waters of the Gulf of Mexico and Caribbean Sea" *by* Elena Ojeda et al.

**Anonymous Referee #1**

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This manuscripts presents a trend analysis of the nearshore extreme wave climate conditions at Mexican waters based on hindcast data. They focus on the number of events, the mean/maxima wave height and storm energy content, respectively, for two type of events: tropical cyclones and Nortes. A methodology that accounts for the nature of the data is being used and the probability of occurrence involved. I think this study provides relevant results for the study area, where studies as such are scarce and it falls within the Natural Hazards and Earth System Sciences scope. It is also well written and structured. Therefore, I recommend its publication after considering the issues highlighted below. I have three main comments/concerns. The authors use a rather sophisticated method to compute the trend of the storm energy content, involving the probability of occurrence but when showing the results they only show the evolution of the probability of occurrence and a linear-derived trend by calculating the



mean annual rate of increase/decrease using E(t=initial) and E(t=end). I understand the simplification to show the results in a table format but I would suggest to plot the trend evolution of E as directly calculated as well (like in Fig 7 but showing evolution of E). Even if it is qualitatively, I think it is good to know how the trend might deviate from the linear trend, according to this method, especially if extrapolations or qualitative comparisons with future projections are made. A similar methodology could have certainly being used not just for E, but also for # events, since the premises of why using such methodology (rather than a more standard technique) are the same (positive data, many years with zeros). Specially for type of events that are not frequent (ie many years without events), using the linear regressing might lead to meaningless results, for example negative number of events, not just for past extrapolations but also for the period of analysis, as it seems to happen for Cancun (fig 6). An option could be to apply Eq 4 but instead of E(t| storm), having # events (t| storm) In the last paragraph of Section 5.4 it is mentioned the results of Perez et al 2014 regarding the projection under A1B scenario. Then it is roughly compared to the study's results and given the disagreement, it is said that this might be related to the different definition of storm used. This is certainly possible but I believe this is not the only factor (probably not even the most important). Two quite different datasets are compared here: (i) past hindcast (in which greenhouse is not explicitly included) and (ii) a future projection (in which a certain greenhouse scenario is explicitly included). Also the time frames are different. I am not saying this cannot be mentioned but given the existing differences, such discrepancies are reasonable. I would suggest commenting on that by adding other factors that interfere in such comparison. In addition, as a more technical note, I think there is an error at Eq. 3. If E is log-transformed, ie lnE(t|storm) = at + b. Then E(t|storm) =exp(at+b)

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