Reply on first review

Changes over time in the 100-year return value of climate model variables

by Leach et al.

Please address all correspondence to Philip Jonathan (*p.jonathan@lancaster.ac.uk*)

Summary

XXX:PhJ: Make sure we use short-cuts with correct fonts for variable, GCM, climate scenario and NA/CS.

We thank four reviewers (referenced Reviewer 2-5) sincerely for their time and expertise in reviewing the manuscript. We note that both reviewers find merit in the work.

Reviewer 2 XXX. Reviewer 3 XXX. Reviewer 4 XXX. Reviewer 5 XXX.

More generally, both reviewers make suggestions for improvement of the manuscript, which we reply to below, entailing some additions and modification of the manuscript. Where we feel the manuscript is already adequate on a point, we provide a note of explanation.

In the following sections, reviewer comments on the manuscript are given verbatim in italics and referenced by review section number 'S', review reference number 'R', comment number 'C'. Our author replies (suffixed 'A') are given in normal font per review comment. Larger changes to the manuscript are given in red in both rejoinder and revised manuscript. References to locations in the article are made by us per section (e.g Section 4, S3.5), to avoid confusion with page- and line-number differences between manuscripts.

We hope that the revised manuscript is acceptable for publication, but would be happy of course to revise further in light of additional review comments.

Review section 1. Are the objectives and the rationale of the study clearly stated?

S1.R2.C0 General comment. This study provides a detailed analysis of CMIP6 global coupled models, focusing on the effects of climate change at site-specific, regional, and global scales. The examination of changes in extreme quantiles, particularly the 100-year return value of climate variables, is highly relevant and enhances understanding of long-term trends. The methodology, combining extreme value analysis (GEV) and non-homogeneous Gaussian regression (NHGR), is well-suited for quantifying extreme events and spatial trends.The structure of the paper is clearly laid out, with a logical progression from data description and methodology to the presentation of results. The separation of global, climate zone, and specific location analyses provides a thorough exploration of the different scales at which climate change impacts may manifest. The inclusion of supplementary material further enhances the accessibility and understanding of the data and results. Only minor revisions are required to further enhance the clarity and impact of the paper. These include making a few refinements to the text for improved flow and precision, correcting typographical errors, and expanding the discussion of the results. Once these minor adjustments have been made, I believe the paper will be ready for publication. Overall, this work represents a significant contribution to the field, with well-defined objectives and a robust methodology. It offers valuable insights into the future projections of key climate variables and highlights the importance of considering both extremes and spatial averages when assessing climate change impacts.

S1.R2.C0.A1 We thank the reviewer for the positive comments. We have accommodated the majority of reviewer suggestions in the revised article, details per section given in this document.

S1.R2.C1 Remark #1 The introduction is thoughtfully organised, drawing on a range of references, including recent ones. The objectives are clearly articulated, and the article's structure is comprehensively outlined. Furthermore, supplementary material is included to reinforce

and support the content of the article. However, the data referenced in Leach (2024) is not available, as the provided link is not working. Please review and update the link in the reference "Leach, C., 2024. Changes over time in the 100-year return value of climate model variables: data. https://github.com/Callum-Leach/Climate-Change-100-Year-Return-Value-Offshore-Engineering-Data", accordingly.

S1.R2.C1.A1 The hyperlink is correct, but without the double quotation at the end of the string. We have ensured that the double quotation mark is clearly separated from the link text, and have confirmed that the hyperlink works.

S1.R2.C2 Remark #2 In the first sentence of the 3rd paragraph of Section "2. Global coupled model output" (page 3 of the manuscript), it would be helpful to explicitly include the corresponding letters for initialisation, physics, and forcing of the model when describing the ensemble members. For improved clarity, you might consider revising the sentence as follows: "(...) we examine output for five climate model ensemble members (rX) where available; these correspond to a common initialisation (iX), physics (pX) and forcing (fX) per GCM, (...)". This would provide readers with a clearer understanding of the specific configuration of each ensemble member, particularly when these details are referenced in Table 1.

S1.R2.C2.A1 We agree, and have added the description as suggested. We have revised the text in S1 to read we examine output for five climate model ensemble members (rX) where available; these correspond to a common initialisation (iX), physics (pX) and forcing (fX) per GCM,.

S1.R2.C3 Remark #3 It would be beneficial to explicitly highlight why the 100-year return value is a critical metric for understanding climate change impacts. This could be incorporated into the introduction to ensure better alignment with the conclusions.

S1.R2.C3.A1 We agree, and have added a description as suggested. XXX:PhJ:Write something.

S1.R2.C4 Remark #4 It would be helpful to clarify how this study contributes to bridging gaps in the existing literature, thereby strengthening the rationale for the research. This could be highlighted in the introduction or the discussion section for better context.

S1.R2.C4.A1 We agree, and have added a description as suggested. XXX:PhJ:Write something.

S1.R3.C1 yes

S1.R4.C1 yes

S1.R5.C1 Generally yes, however, the choices of variables and models needs to be put into context. Please see the Reviewer Comments to the Author Section.

S1.R5.C1.A1 Refer to S10.R5.

Review section 2. If applicable, is the application/theory/method/study reported in sufficient detail to allow for its replicability and/or reproducibility?

S2.R2.C1 Yes [X] *No* [] *N/A* []

S2.R2.C2 Remark #5 It would be constructive to clarify in Section "3. Methodology" the rationale or criteria used to select and present the results from the UKESM1-0-LL GCM in Section "SM5 Diagnostic plots for GEVR model fitting to global annual data, and NHGR modelling fitting to global means, for UKESM1-0-LL" of the Supplementary Material, as opposed to results from other models.

S2.R2.C2.A1 Thanks. We chose UKESM1-0-LL as a typical example. We have added text to this effect in S4.1 of the revised manuscript. The text reads (Note that diagnostic plots were produced for all analyses undertaken. The UKESM1-0-LL GCM is used as a convenient exemplar here.)

S2.R2.C3 Remark #6 It would be insightful to provide additional information about the specific parameters and settings used in the non-stationary GEV and NHGR models, as this would enable other researchers to replicate the statistical analysis.

S2.R2.C3.A1 Details of the MCMC analysis (for GEV and NHGR models) is already provided in the Appendix. We have improved the descriptions of priors there to read Prior distributions were specified as follows for GEV: $\xi \sim U(-1, 0.2)$; $\sigma \sim U(0, \infty)$; $\mu \sim U(-\infty, \infty)$ and for NHGR: α, $\beta \sim U(0, \infty)$.

S2.R3.C1 Yes [X] *No* [] *N/A* []

S2.R4.C1 Yes [X] *No* [] *N/A* []

S2.R5.C1 Yes [X] *No* [] *N/A* []

Review section 3. If applicable, are statistical analyses, controls, sampling mechanism, and statistical reporting (e.g., P-values, CIs, effect sizes) appropriate and well described?

S3.R2.C1 Yes [X] *No* [ ] *N/A* [ ]

S3.R2.C2 Remark #7 The paper discusses the choice of block maxima for the analysis of extreme values, but does not provide a clear justification for this selection over the peaks-over-threshold method. It would strengthen the manuscript to explicitly explain the reasoning behind selecting block maxima for the analysis, providing a more robust rationale for this approach.

S3.R2.C2.A1 Thanks. This issue is already addressed directly in S5.3.

S3.R2.C3 Remark #8 The paper would benefit from providing additional details regarding the criteria used to compare trends across models, particularly for parameters such as location ($\mu 0$), scale ($\sigma 0$), and shape ($\epsilon 0$). A clearer explanation of these criteria would enhance the transparency and robustness of the analysis.

S3.R2.C3.A1 XXX:PhJ:Write some waffle, say how we did it. Mention current work on coupled model, Taylor expansion of arbitrary function etc.

S3.R3.C1 Yes [ ] *No* [X] *N/A* [ ]

S3.R4.C1 Yes [X] *No* [ ] *N/A* [ ]

S3.R5.C1 Yes [X] *No* [ ] *N/A* [ ]

Review section 4. Could the manuscript benefit from additional tables or figures, or from improving or removing (some of the) existing ones?

S4.R2.C1 Remark #9 The paper provides an adequate number of figures for the reader to fully comprehend the study. The Supplementary Material includes additional figures for further reference, and relevant results from the analysis of the graphs in the supplementary document are also discussed within the manuscript. The supplementary document is well-structured, with an introductory section that clearly links the different sections of the supplementary material to the figures in the main manuscript. The figures are well-organised and effectively present the corresponding results. However, for consistency, it would be beneficial if all the graphs in the figures within the Supplementary Material included the x-axis labels. For instance, the graphs in Sections SM1 (Figures SM1 and SM2) and SM4 (Figures SM19 and SM20) do not display the x-axis values, whereas all other graphs in the remaining sections do. Furthermore, in the captions of figures that include empty panels (Figures SM19 and SM20), it would be more coherent to add the note "Empty panels indicate that data for the specific combination of GCM and climate variable was not available for analysis," as seen in the caption of Figures SM1 and SM2.

S4.R2.C1.A1 x-axis labels in figures: In figures consisting of multiple panels with common x-axis, we have ensured that the x-axis tick values and labels are only given on the bottom subset of panels. This entailed minor changes to figures SM1, SM2, SM19 and SM20.

S4.R2.C1.A2 We note that the text "Empty panels indicate that data for the specific combination of GCM and climate variable was not available for analysis" is already included in the caption of Figure SM1 and Figure SM2 (and by implication applies to other figures) where appropriate. For clarity we have added Empty panels indicate that data for the specific combination of GCM and climate variable was not available for analysis to the captions of Figures SM19 and SM20.

S4.R2.C2 Remark #10 In the fourth sentence of the 6th paragraph of Section "2.1. Compilation of spatial summaries: global and climate zone data" (page 5 of the manuscript), where it states "(...) Note also a suspect value in r1i1p1r1 for NorESM2-0-LL tas minimum for scenario SSP245 in the Antarctic (and hence Global), see Figures SM1 and SM8 for illustration: (...)", it might be helpful to rephrase this as: "(...) see Figures SM8 and SM1, respectively, for illustration: (...)" in order to provide the reader with clearer guidance.

S4.R2.C2.A1 We have reordered the figure references as suggested.

S4.R2.C3 Remark #11 In the first sentence of the 9th paragraph of Section "2.1. Compilation of spatial summaries: global and climate zone data" (page 6 of the manuscript), where it is stated, "Figures SM10-SM16 show corresponding global annual mean time-series for the seven GCMs in turn, for further comparison," there appears to be a slight mistake. Figures SM10-SM16 actually pertain to "climate zone annual mean time-series," rather than "global annual mean time-series." I would suggest reviewing the entire paragraph and verifying the accuracy of the information presented.

S4.R2.C3.A1 Thanks. There were a few typos in paragraph! The corrected text reads Figures SM10-SM16 show corresponding

climate zone annual mean time-series for the seven GCMs in turn, for further comparison. Again, there is good general consistency across GCMs. However, trends across climate zones for annual means are quite different to those for annual maxima; e.g. compare `rsds` across climate zones in Figures SM9 and SM16. This suggests that the shape of the distribution of climate zone annual `rsds` in particular changes with climate zone.

S4.R2.C4 Remark #12 In the Supplementary Material, I suggest revising the caption for Figure SM25 (tas for UKESM1-0-LL) to clearly indicate that it represents global annual minima of tas for UKESM1-0-LL. The current caption is quite similar to that of Figure SM24 (UKESM1-0-LL, tas), which represents global annual maxima (a) and mean (b) values, potentially causing confusion for the reader.

S4.R2.C4.A1 Thanks. We note that both the figure title and subcaption of Figure SM25 state that the figure refers to global annual minima.

S4.R2.C5 Remark #13 In the Supplementary Material, the captions for Figures SM17 and SM18 should include the clarification: "The first two characters of the GCM name are used for concise labelling" consistent with the explanation provided in Figure SM26.

S4.R2.C5.A1 Thanks. We note that Table 1 of the main text explains to acronyms used for GCM referencing. However, we have revised the caption of Figure SM17 as a compromise as suggested, adding the text Two letter acronyms are used to refer to GCMs, as detailed in Table 1 of the main text. XXX:PhJ:AddIt.

S4.R2.C6 Remark #14 In the caption of Figure 2, it would be beneficial to include the explanation: ""Mxm" and "Mnm" in titles represent maximum and minimum, respectively" as was done in Figure 5 of the manuscript. This clarification is particularly important since these abbreviations appear here for the first time in a figure.

S4.R2.C6.A1 Thanks. We have revised the caption as suggested to include "Mxm" and "Mnm" in titles refer to maximum and minimum respectively. XXX:PhJ:AddIt.

S4.R2.C7 Remark #15 Interpreting the means in the box-whisker plots presented in the Supplementary Material, Section SM6.1 (Figures SM26–29), is somewhat difficult; however, the presentation in the manuscript provides a clearer and more accessible format for interpretation.

S4.R2.C7.A1 Thanks. We have added a pointer in the caption of Figure SM26 to S4.2 and F9 of the main manuscript, stating See Section 4.2 and Figure 9 of the main text for discussion. XXX:PhJ:AddIt. We have included the following text in S4.2, referring the reader to the supporting information in Figures SM26-29 Figures SM26-29 provide supporting findings for Δ^Q per ensemble member, plotted seperately per climate scenario. XXX:PhJ:AddIt

S4.R2.C8 Remark #16 In the Supplementary Material, the caption for Table SM1 contains a discrepancy. Where it states "(...) thus we estimate an increase of 7.02K in the 100-year minimum tas in the Temperate North under scenario SSP585 over the next 100 years" the value for SSP585 should be 17.42K, not 7.02K. The value of 7.02K corresponds to SSP245. I kindly request that you revise it.

S4.R2.C8.A1 Thanks. We have revised the text to read under scenario `SSP245` over the next hundred years as suggested. XXX:PhJ:AddIt.

S4.R2.C9 Remark #17 In the caption of Table 4, it is stated that "(...) thus we estimate a reduction of 3.16 ms^{-1} in the return value for sfcWind under scenario SSP585." However, the climate zone and the scenario difference effect are not specified. I suggest providing this information for clarity.

S4.R2.C9.A1 Thanks. We have modified the caption to read in the Arctic. XXX:PhJ:AddIt

S4.R2.C10 Remark #18 In the Supplementary Material, the caption for Table SM4 states that "(...) thus we estimate a reduction of 3.16 ms^{-1} in the return value for sfcWind under scenario SSP585." However, the value of "-3.16" does not appear in the table. I recommend reviewing and specifying the region to which this reduction applies.

S4.R2.C10.A1 Thanks. This is an error, from copying captions between figures! We have modified the caption to refer to `tas` globally, which shows an increase of 6.23K under scenario `SSP585`. thus we estimate an increase in $6.23K$ in the return value for tas under scenario SSP585. XXX:PhJ:AddIt

S4.R3.C1 nothing

S4.R4.C1 NO

S4.R5.C1 I believe this paper contains a sufficient number of figures and statistical analyses.

S4.R5.C1.A1 Thanks.

Review section 5. If applicable, are the interpretation of results and study conclusions supported by the data?

S5.R2.C1 Yes [X] *No* [ ] *N/A* [ ]

S5.R2.C2 Remark #19 The conclusions are generally well-supported by the data presented in the study. The interpretation of the results is consistent with the findings, and the study's conclusions align with the key trends and patterns identified through the analysis. The authors effectively highlight the significance of their findings in relation to the broader context of climate change. No major revisions are needed in this section, as the conclusions accurately reflect the results. Overall, the study presents a clear and coherent interpretation that is well-supported by the data. However, to further strengthen the Section "5. Discussion and Conclusions", the authors may wish to consider the remarks provided throughout this review, which suggest ways to enhance the clarity and robustness of the study's interpretations and conclusions.

S5.R2.C2.A1 We are grateful to the reviewer for a thorough review! Details of changes made in light of R2 review comments are given throughout this document.

S5.R3.C1 Yes [X] *No* [ ] *N/A* [ ]

S5.R4.C1 Yes [X] *No* [ ] *N/A* [ ]

S5.R5.C1 Yes [X] *No* [ ] *N/A* [ ]

Review section 6. Have the authors clearly emphasized the strengths of their study/theory

S6.R2.C0 The strengths of this study are well communicated, but the following suggestions could further emphasize them:

S6.R2.C1 Remark #20 It would be beneficial to highlight the novel aspect of considering both extremes and spatial means across multiple models and scenarios, as this approach offers a more comprehensive view of climate variability.

S6.R2.C1.A1 Thanks. We have added the following text at the end of the first paragraph of S2.1 of the revised manuscript: Analysis of spatial extremes and of spatial means across multiple models and scenarios provides a more comprehensive assessment of future GCM-predicted climate variability that typically considered for offshore design. XXX:PhJ:AddIt.

S6.R2.C2 Remark #21 I suggest emphasising the contribution of using CMIP6 data to assess long-term trends in extreme values, highlighting how this study differs from those that focus on short-term calibration.

S6.R2.C2.A1 Thanks. We believe that we have addressed this point sufficiently already. For example, the first sentence of S5 states why examination of GCM output is interesting for long-term, and S1 contains a discussion of GCM calibration for short-term work. Nevertheless, we have added the text Analysis of GCM output provides indications of likely long-term climate effects which cannot be achieved for example from calibration of typical hindcast output for short-term purposes. XXX:PhJ:AddIt.

S6.R3.C1 yes

S6.R4.C1 YES

S6.R5.C1 Yes, but please refer to the Reviewer Comments to the Author Section for further comments on this.

S6.R5.C1.A1 Refer to S10.R5 below.

Review section 7. Have the authors clearly stated the limitations of their study/theory/me

S7.R2.C0 The authors acknowledge several limitations of the study, but the following additions could enhance transparency:

S7.R2.C1 Remark #22 It would be helpful to clearly state that the absence of calibration for future extremes may lead to systematic biases, especially for variables with high spatio-temporal variability.

S7.R2.C1.A1 Thanks. We believe that this point has already been made in S1. Nevertheless, we have added the following text in S5.3 for emphasis: As noted in S1, the current study considers the application of uncalibrated CMIP6 output. By focussing on differences in return values over a period of 100 years, we are sure that caibration offsets will not affect our inferences.

However, linear and higher order "gain" terms in the calibration will typically result in systematic departures from the results given here. However, calibration of GCM outputs to as-yet unseen future conditions is problematic. XXX:PJ:AddIt.

S7.R2.C2 Remark #23 It would be useful to discuss the potential impact of using annual maxima, as opposed to alternative methods such as peaks-over-threshold, on the robustness of the findings.

S7.R2.C2.A1 Thanks. We refer the reviewer to our response S3.R2.C2.A1.

S7.R3.C1 yes

S7.R4.C1 YES

S7.R5.C1 There are some limitations of the study that the author could mention in the conclusion section. See the Reviewer Comments to the Author Section.

S7.R5.C1.A1 Thanks. We refer the reviewer to our response S10.R5 below.

Review section 8. Does the manuscript structure, flow or writing need improving (e.g., the addition of subheadings, shortening of text, reorganization of sections, or moving details from one section to another)?

S8.R2.C1 The manuscript is well-organized, and the structure and flow are generally clear and logical. Each section is appropriately defined, and the progression of ideas from one to another is smooth. The use of subheadings aids in navigation, and the presentation of content is cohesive. No significant changes or improvements are needed in terms of structure. Overall, the manuscript is well-structured and effectively communicates the research.

S8.R2.C1.A1 Thanks.

S8.R3.C1 no

S8.R4.C1 NO

S8.R5.C1 The manuscript is well written.

S8.R5.C1.A1 Thanks.

Review section 9. Could the manuscript benefit from language editing?

S9.R2.C1 No

S9.R3.C1 Yes

S9.R3.C1.A1 Thanks. We assume that this response may be a typo, seeing that (a) no details of the language improvement are given, and (b) none of the other reviewers have concerns with language.

S9.R4.C1 No

S9.R5.C1 No

Review section 10. This field is optional. If you have any additional suggestions beyond those relevant to the questions above, please number and list them here.

S10.R2.C1 Remark #25 The current submission contains six highlights, whereas the journal specifies a maximum of five bullet points. I recommend combining or removing some of these points to comply with this requirement. Additionally, three of the highlights (1st, 4th and 6th) exceed the 85-character limit, including spaces. I suggest rephrasing them to make them more concise while maintaining their key messages. Finally, in the first highlight, the phrase "We quantify (…)" is written in the first person. It would be more appropriate to use an impersonal structure, such as "Quantification of (...)", for consistency.

S10.R2.C1.A1 Highlights. We have reduced the number of highlights to five, and reduced the length of each highlight. We have rephrased the first highlight (although in general we prefer a more "story-telling" style). The new highlights are: XXX:PhJ:DoIt.

S10.R2.C2 Remark #26 The abstract provides a clear overview of the purpose of the research, the principal results, and the major conclusions, as required by the journal. It outlines the aim of assessing changes in tail characteristics for wind, solar irradiance, and temperature variables using CMIP6 model outputs. The key methods employed, such as non-stationary extreme value models and non-homogeneous Gaussian regression, are well described. Additionally, the abstract highlights the main findings, including the weak evidence for changes in wind extremes and the stronger evidence for changes in solar irradiance and temperature. However, the current abstract exceeds the journal's word limit of 200 words. I recommend revising it to ensure conciseness while retaining the essential information on the study's purpose, methods, key results, and conclusions.

S10.R2.C2.A1 We have reduced the length of the abstract as requested. The revised abstract reads: XXX:PhJ:DoIt.

S10.R2.C3 Remark #27 The manuscript includes nine keywords, which exceeds the journal's specified limit of seven. I recommend reducing the number of keywords to comply with the journal's guidelines. Additionally, the final keyword ends with a semicolon (";"), which should be removed to align with proper formatting. Please revise the keywords accordingly.

S10.R2.C3.A1 We have reduced the number of keywords to seven as requested. The revised keywords are: XXX:PhJ:DoIt.

S10.R2.C4 Remark #28 I have identified a typo in Section "1. Introduction". In the second sentence of the 1st paragraph (page 1 of the manuscript), the phrase "(...) that global mean sea level has increases (...)" should be corrected to "(...) that global mean sea level has increased (...)."

S10.R2.C4.A1 Thanks. We have changed "increases" to increased.

S10.R2.C5 Remark #29 I have identified a typo in Section "2.1. Compilation of spatial summaries: global and climate zone data". In the fifth sentence of the 3rd paragraph (page 4 of the manuscript), the phrase "(...) this figure also provides illustrations of generally reducing trends in global annual maximum rsds, and increasing trends in global annuam minimum of tas." should be corrected to "(...) and increasing trends in global annual minimum of tas."

S10.R2.C5.A1 Thanks. We have changed "annuam" to annual.

S10.R2.C6 Remark #30 I have identified a typo in Section "2.2. North Atlantic and Celtic Sea data". In the first sentence of the 2nd paragraph (page 7 of the manuscript), where it states "(...) and given in Figures SM19-20." it should be "(...) are given in Figures SM19-20." Kindly confirm.

S10.R2.C6.A1 Thanks. We have changed "and" to are.

S10.R3.C0 In this paper, the effects of climate change on extreme climate variables are studied, focusing on the changing trends of short-wave radiation (rsds), near-surface wind speed (sfcWind), maximum daily wind speed (SFCwind Max) and near-surface air temperature (tas). The output of several global coupled climate models (GCMs) and a variety of different climate scenarios (SSP126, SSP245, SSP585) were statistically quantified. The non-stationary generalized extreme value regression model (GEVR) and non-homogeneous Gaussian regression model (NHGR) were used to evaluate the extreme value and annual mean change of climate variables, respectively. At the same time, Markov chain Monte Carlo method (MCMC) was used to quantify the model uncertainty. This paper is innovative in that it uses seven climate models and multiple climate variables to comprehensively assess the impact on extreme climate. This comprehensive multi-model and multi-variable analysis method improves the breadth and comparability of the research, especially in the quantification of extreme value distribution. In addition, non-stationary generalized extreme value regression (GEVR) and non-uniform Gaussian regression (NHGR) models are used to analyze the changes of extreme value and mean value, and the Bayesian method is combined to estimate the changes of extreme value in 100 years, which improves the reliability of the results to a certain extent. However, there are still limitations in the article, and it is suggested to be revised and published:

S10.R3.C1 (1) It has been repeatedly mentioned that there are significant differences in the forecast results between different models, especially in wind speed and extreme value changes. This shows that there are still significant uncertainties when different climate models simulate the same variables. It is recommended that the authors further explore ways to reduce model inconsistencies, such as through model calibration or higher-resolution simulations.

S10.R3.C1.A1 We thank the reviewer for these two points. Regarding model calibration, we note that it is not clear whether model calibration would reduce uncertainties in differences in return value; we refer the reviewer to S1 of the main manuscript, and our response S7.R2.C1.A1, where this point has been discussed. Regarding higher-resolution simulations, we have added the following text in the discussion S5: We note that current analysis is conditional on the quality of current GCM models; we might anticipate that current findings would change, were the analysis be repeated in future using improved (e.g. higher resolution) climate models. XXX:PhJ:AddIt

S10.R3.C2 (2) A more in-depth physical discussion of the weaker trends in some variables, such as MRI-ESM2-0 wind speed maxima, to

explain whether these phenomena are related to the limitations of climate models or the atmospheric dynamics of some specific regions.

S10.R3.C2.A1 Thanks. We have already speculated on these issues in the discussion S5. Nevertheless, we have added the following text in S5 to highlight the point: In general, is not clear whether the weak trends in wind speed variables are related to the limitations of climate models, or to real characteristics of future atmospheric dynamics. XXX:PhJ:Addit.

S10.R3.C3 (3) The conclusion of the paper does not mention whether the research problem in the introduction has been solved, so it is suggested to improve it.

S10.R3.C3.A1 Thanks. We have added the following text at the start of the second paragraph of the discussion S5: The objective of the current work was to examine key output from CMIP6 global coupled models at site-specific, regional and global scales, to form a view of what state-of-the-art science is telling us about climate change effects. To achieve this, we estimate XXX:PhJ:AddIt.

S10.R3.C4 (4) Although the colors currently used (such as green, orange and gray) distinguish different climate scenarios, the colors are not bright enough, and high contrast color schemes can be considered, such as red and blue.

S10.R3.C4.A1 Thanks. We find that different readers have preferences regarding colour schemes. The colour scheme used for the current work was selected based on current "recommended popular" colour schemes, it has been used by the authors in the past on multiple papers, and provides good contrast even when figures are viewed in greyscale.

S10.R3.C5 (5) The description of the model and data in the paper is rather redundant. For example, the model source and variable definition of CMIP6 are mentioned several times in different parts, and it is suggested to simplify.

S10.R3.C5.A1 Thanks. The authors' view is that it is sometimes important to re-emphasise certain features of the data and analysis. Indeed, in some of the descriptions, the authors feel that perhaps even more detail than currently provided is necessary for the reader to best understand and interpret the information presented. We have tried to reach a reasonable balance. We note that none of the other reviewers have concerns.

S10.R4.C0 In this paper, the changes of the tail characteristics of wind speed, solar radiation and temperature output of CMIP6 climate model due to climate forcing are studied with time. To assess changes in centennial reproducible values of wind speed, solar radiation, and temperature variables in the CMIP6 climate model output, as well as changes in annual mean data for these variables over the period 2015 to 2100. The authors used Bayesian inference to estimate the parameters of a non-stationary extreme value model (GEVR) and a heterogeneous Gaussian regression (NHGR) model. Using these models, the authors quantified centennial recurrence changes in annual extremes and changes in annual mean levels over the period 2025 to 2125. The study considered three different climate scenarios (SSP126, SSP245, SSP585) and multiple climate model ensemble members. It is found that for wind speed variables, the study results show that the centennial recurrence value changes with time and climate scenarios are weak. In contrast, solar radiation and temperature variables show more pronounced changes in centennial recurrence values. For annual averages, the evidence for changes in wind speed variables over time is stronger, especially in the Northern Hemisphere, but the magnitude of change is smaller. The suggestions are as follows:

S10.R4.C1 (1) Although Bayesian analysis does not rely on P-values, providing the median, mean, and confidence intervals (such as confidence intervals) of the posterior distribution will help readers understand the uncertainty of parameter estimates.

S10.R4.C1.A1 Thanks. We accept the reviewer's point, and note that 95% credible intervals (the Bayesian equivalent of frequentist "confidence intervals") are provided in many of the figures. In some cases, showing central characteristics (like the mean and median) alone is necessary to avoid too much clutter in figures. We hope we have reached a reasonable balance.

S10.R4.C2 (2) The specific application of non-stationary extreme value model and heterogeneous Gaussian regression model and the reasons for parameter selection can be further elaborated to help readers better understand the applicability and limitations of the model.

S10.R4.C2.A1 Thanks. On choice of model, we have added the following text in SXXX: XXX:PhJ:DoIt. On "parameter selection", we believe the reviewer is referring to "climate variable selection" (i.e. why look at wind speed, TAS and RSDS?). In this case, we have added the following text in SXXX to explain that these variables are all of interest in the design of marine and coastal structures. The text reads: XXX:PhJ:DoIt.

S10.R4.C3 (3) The limitations of their research should be more clearly stated in the discussion section, including limitations in model selection, data availability, analytical methods, and interpretation of results.

S10.R4.C3.A1 Thanks. We believe that we have emphasised many limitations of our work already in S5.3, including (a) limited spatio-temporal resolution of current climate models failing to capture local changes in atmospheric pressure fields driving winds adequately; (b) that non-linear terms in models for `tas` may be justifiable, (c) the possibility that a common starting distribution across climate scenarios at year 2015, (d) the possibility that block-maxima analysis instead of peaks-over-threshold may cause statistical inefficiencies, (e) that taking maxima over large spatial domains may complicate tail characteristics, (f) that increased uncertainty arises from considering multiple climate models and ensemble runs. See also our responses S10.R3.C1.A1 (no model calibration performed), S10.R5.C4.A1 (assumed linear temporal change of model parameters) and

S10.R5.C5.A1 (assumption of independent GCMs).

S10.R4.C4 (4) In the discussion section, the possible reasons for the uncertainty of the hundred-year recurrence value of the wind speed variable are discussed in detail, and the potential impact on the offshore engineering design is discussed.

S10.R4.C4.A1 Thanks.

S10.R4.C5 (5) It is recommended that the authors further explore in the discussion section how these findings can be applied to actual ocean engineering design, especially in consideration of the effects of climate change.

S10.R4.C5.A1 Thanks. We would note that this work is a research article on the effect of climate change on variables of interest to ocean engineers. We also note that various industrial bodies (e.g. IOGP Report 662 2024) have already provided recommendations for design subject to climate change. We would hope that the current work might provide insights to inform some of these recommendations, but it is absolutely not "the complete answer" in itself. We have added the following text in the discussion, to reflect this: The current work is academic research examining key outputs from CMIP6 global coupled models at site-specific, regional and global scales, to form a view of what state-of-the-art climate science is telling us about climate change effects in variables of interest to the offshore engineering community. Discussion of practical design steps, already provided in documents such as IOGP Report 662 2024, is far beyond the scope of this work. XXX:PhJ:AddIt

S10.R5.C0 The paper presents a statistical analysis of projected future changes in both extremes and means of four climate variables, based on outputs from seven CMIP6 GCMs, under three climate scenarios, and multiple ensemble runs. The primary objective is to explore the tail characteristics of the distributions for wind, solar irradiance, and temperature across different spatial scales: globally, within specific climate zones, and at select point locations in the North Atlantic and Celtic Sea, which I assume are of particular interest to Shell. I find the work to be thorough and of significant relevance to the scientific community. The authors' statistical approach is detailed and meticulous, demonstrating a clear mastery of the subject matter. Given the importance of the topic and the rigorous methodology employed, I recommend this paper for publication, subject to addressing a few minor points. My main comments are outlined below.

S10.R5.C1 (1) The authors do not provide any rationale for selecting the climate variables, but given that this is a scientific paper, it is valuable to provide context in the introduction. Please include some background information and perhaps specific examples explaining why wind, solar irradiance, and temperature were chosen. Are these variables relevant for floating solar installations? Are they crucial for offshore wind farms? What are the reasons behind these selections? Additionally, offer a brief explanation of why these specific seven GCMs were selected, even if it is simply for convenience because they were readily available in your database. Overall, both the choice of variables and the selection of models would benefit from more background information.

S10.R5.C1.A1 Thanks. A similar point was made by R2 abd R4. Please see our responses S1.R2.C3.A1 (why 100-year events), S1.R2.C4.A1 (research gaps), and S6.R2.C2.A1 (large extent of study over multiple GCMs etc)

S10.R5.C1.A2 On relevance of climate variables XXX:PhJ:DoIt.

S10.R5.C1.A3 On GCMs, the choice was pragmatic. The GCMs chosen were simply all those available on the UK CMIP6 site at the time of the analysis, which provided sufficiently comprehensive output (e.g. multiple ensemble runs, multiple scenarios, ...) on all of `rsds`, `sfcWind`, `sfcWindmax` and `tas`. We have added a sentence in SXXX to clarify this. XXX:PhJ:DoIt.

S10.R5.C2 (2) If I understand correctly, for sfcWind, rsds, and tas, you extracted daily averages. While I recognize that this choice simplifies the analysis, it raises questions about its impact on extreme statistics. This potential limitation is not addressed anywhere in the text. It would be beneficial to mention this as a limitation of the study in the conclusion. Is this the reason why you selected sfcWindmax for the wind?

S10.R5.C2.A1 Thanks. We have added the following text in S2. XXX:PhJ:Doit. XXX:CL:[I cannot find any text which describes the limitations of using daily averages etc. In section 2 of the main manuscript we describe what data we collect, but nothing to do with limitations.]

S10.R5.C3 (3) Figure 3 is unclear. It's not immediately apparent how the ensemble members are represented, as I only see a single point per model in most of the subplots and the colours for the scenarios. I believe this figure requires revision for better clarity.

S10.R5.C3.A1 Thanks. We agree in general that presenting a huge amount of analysis output succinctly is challenging! Nevertheless, on reflection, the authors think that F3 as it stands is informative. There are multiple points per model (these sometimes overlap to some extent in some cases, but clearly do not in others). We have added the following text to the caption to emphasise this: Discs indicate slopes for (multiple) ensemble members: disc colour indicates climate scenario (`SSP126`, green; `SSP245`, orange; `SSP585`, grey), and disc "alpha level" distinguishes individual ensemble members given GCM.

S10.R5.C4 (4) The study assumes that all three model parameters (location, scale, and shape) change linearly over time. Have you tested the impact of assuming changes in only the location parameter, for example? Additionally, do you have any plans to incorporate covariates, such as the North Atlantic Oscillation Index, for specific regions? Addressing these points could strengthen the robustness of the analysis.

S10.R5.C4.A1 Thanks! This is an interesting point, and the subject of further current analysis. Model choice for the manuscript under discussion was motivated by that used in Ewans and Jonathan (2023), which exploited linear growth of all model parameters (for GEV and NHGR). We agree completely that in some cases a simpler model might have been more appropriate, as the reviewer suggests. Nevertheless, for the sake of simpicity of exposition, we preferred to retain a common linear functional form the variation of GEV and NHGR parameters. In current work, on changes in extreme temperatures, we are attempting a more sophisticated analysis in which model selection across a hierarchy of models of different complexity (using a criterion such as the divergence information criterion, DIC) is exploited. We have added the following text in the discussion (SXXX) to emphasise this: XXX:PhJ:DoIt. XXX:PhJ:[Mention that we could also explore climate oscillation indices as covariates in future]

S10.R5.C5 (5) The final statistics tables provide a concise and engaging way to summarize the overall variability in the analysis and the confidence in the projected changes. However, the assumption that all models contribute equally is a bit of a stretch. There is no discussion in the text regarding the independence of GCMs, and treating ensemble runs as independent realisations when they essentially represent the same climate model with different initial conditions, model set up, or test runs, is also a questionable assumption. I recommend adding a dedicated discussion in the conclusion section to address model independence and explicitly state these assumptions as a limitation. Additionally, while it may be clear to the authors, the methodology for calculating the expected value of change and the probability of change should be explicitly described. For example, is the expected value simply the mean of all return values across the GCMs and ensemble runs? Is the probability of change calculated as the fraction of models where $\Delta > 0$ over the total number of models? Clarifying these details would greatly enhance the transparency of the analysis.

S10.R5.C5.A1 Thanks. On the assumption of independence of GCMs, we again agree with the reviewer's comment. We have added the following text to clarify the potential limitations of assuming independence: XXX:PhJ:DoIt[Access, UK common components; can you actually learn which GCM is working best and upweigh it?] XXX:CL:[In section 5.2 we have written "the climate model and ensemble output available to us are drawn randomly from large families of models and their ensemble member" but we have not explicitly stated any limitations to this, or anything to the effect of weighting models based on their performance.].

S10.R5.C5.A2 On estimating probabilities of change, and expected change, the procedure is straightforward and more-or-less what the reviewer outlines. We have added the following text to clarify: XXX:PhJ:DoIt.

S10.R5.C6 Minor comments: It is quite challenging to give detailed comments on the text without line numbers, but I will try my best. Next time I suggest submitting with line numbers.

S10.R5.C6.A1 Thanks. We are sorry about this. To be honest, it is never clear to the authors whether it is intended that the line-numbering be introduced at LaTeX compilation, or thereafter by "compilation" on the OE website. Apologies, we will try to do better next time.

S10.R5.C7 In the objective and outlines subsection " For the global and climate zone analyses, we think it IS interesting . . . "

S10.R5.C7.A1 Thanks. Actually, we judge the "is" optional here; the sentence reads well either way. In the spirit of compromise, we have added it is as requested. XXX:PhJ: Doit.

S10.R5.C8 Eq. 3: do you weigh your climate zone averages by latitude?

S10.R5.C8.A1 Thanks. This point is explained in the text. Within climate zone, we take the arithmetic mean of values (and acknowledge that this is an approximation). However, across climate zones, we calculate global means correcting for the area differences between the different zones.

S10.R5.C9 Figure 2: It might be actually more interesting to show here the ensemble averages from all your model runs, instead of just one model.

S10.R5.C9.A1 Thanks. We agree in general that presenting a huge amount of analysis output (from the original GCM and the subsequent statistical work) succinctly is challenging! Nevertheless, on reflection, the authors think that F2 as it stands is informative (because it is relatively uncluttered), particularly coupled with all the complementary supporting material in SM. We hope the reviewer can live with this. Including ensemble averages for 7 climate models and 3 scenarios would entail placing 21 lines in each panel, which is probably too much.

S10.R5.C10 Page 6 "In summary, climate scenario effects on wind speed variables are less pronounced than on rsds and tas, with the exception of the Arctic zone; physically, perhaps this is related to the occurrence of polar lows and cyclonic systems there, but it IS not clear. . . "

S10.R5.C10.A1 Thanks, is added.

S10.R5.C11 Page 6, just before Figure 3: "Section 3" remove the s.

S10.R5.C11.A1 Thanks, "s" removed. XXX:PhJ:DoIt

References

Ewans, K., Jonathan, P., 2023. Uncertainties in estimating the effect of climate change on 100-year return period significant wave heights. Ocean Eng. 272, 113840:1–17.

IOGP Report 662, 2024. Potential climate change effects of metocean design and operating criteria. IOGP, London.