***Response to Referee Comment 2 on* "Time of Emergence of compound events: contribution of univariate and dependence properties" *by* Bastien François et al.**

Anonymous Referee #2

General comments:

Comment:

The authors present a nice framework for estimating the time of emergence (TOE) of compound events (CEs). To demonstrate the framework, the authors analyse the TOE for two types of CEs in France. While I recommend publication of the framework in general, I have two major concerns regarding the application which should be considered prior to acceptance.

Response:

We would like to thank the anonymous referee for her/his very positive comments and the detailed questions. All the comments and our point-by-point responses are given below.

Comment:

Major concerns:

1. The authors apply their framework to CMIP6 model data. While this choice may be justified for the temperature related CE in central France, it is likely not for the wind-precipitation CE in Brittanny. CMIP6 models have a rather coarse resolution (mostly coarser than the chosen grid of 0.5° x 0.5°). Their representation of regional wind and precipitation is therefore likely rather bad (see e.g. IPCC AR6 WG1 Chapter 10), and more representative of large scale averages which might not be relevant for regional impacts. Thus the value of the study is more on the development (and demonstration) of the framework rather than on providing relevant results for the two considered regions (maybe one of them).

Response:

We agree with this comment: the coarse resolution of CMIP6 models can affect the representation of regional compound events such as compounding wind and precipitation extremes over the region of Brittany, France. However, and as the referee noted, the aim of the paper is more on the development of the framework than on providing precise results for Time of Emergence of specific compound events. With the aim of clarifying better this point, we propose to add the following sentence (in blue) in the paragraph starting at L70 of the initial submitted article (Section Introduction):

> "*In this paper, we propose a new methodology to assess the time of emergence of compound events probabilities. We also develop a*

copula-based multivariate framework, which allows for an adequate description of the contribution of the marginal and dependence properties changes to the evolutions of multivariate hazard probabilities. This compound event analysis is applied to two case studies. ***Please note that the goal of the paper here is not to provide precise results of ToE on these two case studies, but rather to introduce the conceptual framework and raise awareness among climate scientists on the potential emergence of CE probabilities, as well as the contributions of statistical properties to probability changes.*** *We first analyse compound wind and precipitation extremes over the coastal region of Brittany (France). This bivariate compound event, i.e., composed of co-occurring climate hazards over the same region and time, has been analysed in several studies (e.g., Martius et al., 2016; Bevacqua et al., 2019; De Luca et al., 2020a; Reinert et al., 2021; Messmer and Simmonds, 2021) as it can have severe impacts such as important economic losses, massive damages to infrastructure and loss of human life (e.g., Fink et al., 2009; Liberato, 2014; Wahl et al., 2015; Raveh-Rubin and Wernli, 2015).*"

Comment:

Alternatively, the authors could have chosen CORDEX simulations. I guess they did not because they wanted to choose a preindustrial baseline. But is this really useful? People are (well, should be) adapted to present climate, so one could well estimate the TOE relative to, e.g., 1971-2000 or even a later period. This would also increase the relevance of the results for the chosen journal by essentially asking "when will we feel climate change in these hazards?". I therefore request to replace the chosen models by CORDEX models to provide results of practical relevance. I explicitly leave the decision on this choice to the editor as the paper anyway makes a useful conceptual contribution.

Response:

It is true that the choice of using simulations from CMIP6 models instead of CORDEX is in part due to our willingness to investigate probability changes with respect to a preindustrial baseline. This choice is also made in order to complement the research on the uncertainty of GCMs in representing the characteristics of CEs and their evolution over time (e.g., Ridder et al., 2021, 2022; Vogel et al., 2020, among others). Although we agree with the anonymous referee that analyzing outputs from RCMs with higher resolution (e.g., CORDEX simulations) could provide results of practical relevance at regional scale, we believe that our study using GCM simulations still provides interesting results that illustrate our methodology well and highlight general key points when analyzing changes of CE probabilities. However, in order to clarify that our methodology can be used and/or adapted to analyse simulations with higher spatial resolution, we propose to add the following paragraph (in blue) in the sub-section 6.2 (Conclusion, discussion and future work / Discussion and perspectives) at L585 of the initial submitted article:

> "***In this study, we demonstrated our conceptual framework using simulations from an ensemble of 13 GCMs. While using GCMs permitted to illustrate our methodology and draw general conclusions when analyzing changes of CE probabilities, the resolution of such climate models is often considered too coarse for a realistic representation of climate variables at a regional scale, such as for precipitation and wind (e.g., IPCC, 2021). Consequently, the ToE results obtained in this study for Brittany and Central France regions may not be accurate enough to be used for adaptation planning. Applying our methodology to analyse simulations from Regional Climate Models (RCMs) that simulate physical processes at a higher spatial resolution would permit us to provide more relevant information on regional CEs that could be used to design realistic regional adaptation strategies. An appropriate future step could be for example to apply the presented methodology to analyse CEs using simulations from RCMs forced by CMIP5 data (CORDEX) or CMIP6 data."***

Regarding the baseline period, we believe that the choice of a pre-industrial baseline period in our study permits us to provide interesting results on probabilities associated with natural variability and the influence of anthropogenic climate change on CEs. However, it is true that a more recent baseline period such as 1971-2000 could have been chosen to provide information adapted to the present (or say recent) climate. This is already mentioned in sub-section 3.1 (Statistical method/Time of emergence of climate hazards) at L160 of the initial submitted article:

> "*In this study, we consider the reference period (1871-1900) as baseline to assess the emergence of hazard probabilities. However, there is no agreement on the choice of the baseline period for ToE studies. While most of the studies choose a pre-industrial period as baseline to attribute emergence to anthropogenic greenhouse gas forcing (e.g., 1850-1900, Hawkins et al., 2020), other studies choose a more recent baseline period (e.g., 1951-1983, Ossó et al., 2022), which can provide relevant information for adaptation planning. We further discuss the choice of the reference period for emergence in Sect. 6.*"

and in sub-section 6.2 (Conclusion, discussion and future work / Discussion and perspectives) at L557:

> "*However, other baseline periods could have been chosen, such as more recent ones which would provide useful results for adaptation planning (e.g., Ossó et al., 2022).*"

We however suggest to provide more information on the motivation for choosing more recent baseline periods by adding new sentences (in blue) in sub-section 6.2 (Conclusion, discussion and future work / Discussion and perspectives) at L557:

*“However, other baseline periods could have been chosen, such as more recent ones which would provide useful results for adaptation planning (e.g., Ossó et al., 2022). **In practice, despite the climate changes with respect to its natural variability, societies are (or should be) adapted to the present or recent climate. Hence, estimating the ToE relative to a more recent period, e.g. 1971-2000, would permit providing results of more practical relevance for adaptation.** Of course, depending on the chosen baseline period, the estimated natural variability that serves as reference for assessing changes would be different, and thus would affect the ToE results.”*

Comment:

2. The authors make use of the model ensemble in two different ways (Indiv vs. Full).

The TOE compares noise and signal and is thus a property of the climate system. The noise should really just be internal variability, and the signal the forced signal, which differs from model to model. Pooling all models together mixes signal and noise - the difference in model signals is mixed into the noise. This makes no sense in particular because the uncertainties in precipitation and wind changes are so uncertain that calculating a mean change as signal may create something physically implausible (see e.g. Shepherd, Nat. Geosci, 2014). Please remove the "Full Ensemble" version!

Response:

We agree that pooling may lead to a loss of signal. The “Full-Ensemble” version proposes a methodology to extract a CE probability signal among the different models by assembling the variables contributing to the CE using pooling. The underlying philosophy of this procedure is similar to multi-model means (MMM, see, e.g., Tebaldi and Knutti 2007; Knutti et al. 2010) that consist in assembling the different models and averaging them to derive multi-model means. Pooling of model data has already been performed in the climate science literature and assumes that each climate model is one valid representation of the physical processes at play in the climate system, and climate simulations being samples from the observed distribution (e.g., Srivastava et al., 2021). Under this assumption, it permits us to assess the global uncertainty inherent in climate modelling. Note also that a bias correction method has been applied to each climate simulation (with respect to CNRM-CM6) in order to make the simulations consistent with each other. However, it is true that, as for any MMM or pooling, in practice, model’ signals are different and thus mixed together. Even if the application of the Full-version has some drawbacks that can be criticized, in particular for its application on wind and precipitation subject to large uncertainties as mentioned by the anonymous referee, we believe that the methodology and the results obtained are still interesting and can be of interest to readers investigating compound events probability using multi-model ensembles.

Hence, instead of completely removing the “Full-ensemble version” from the article, we propose to remove all explanations, results, figures and appendices related to the Full-version from the main body of the article, and put them as Supplementary material. In addition, we propose to also mention its limitations for variables with large uncertainty by adding a sentence (in blue) at the end of the paragraph starting at L273 of the initial submitted article for the “Full-Ensemble version” in the Supplement:

> “*Depending on the versions, the objectives are not the same: whereas the Indiv-Ensemble version permits to analyse the modelling of hazards separately and assess the uncertainty in ToE arising from the inter-model differences, the Full-Ensemble version permits to derive unique ToE estimates and contribution values accounting for the global uncertainty in climate modelling. This Full-Ensemble version assumes that the variables of interest are drawn from the same distribution.* ***Please note that pooling multiple models together may lead to a loss of signal: by assembling the contributing variables together to estimate the CE probability, the pooling procedure can result in mixing the different model signals and noise together. This is particularly true for precipitation and wind changes which are subject to large uncertainties (Shepherd, 2014). However, pooling of model data assumes that each climate model is one valid representation of the physical processes at play in the climate system, and climate simulations being samples from the observed distribution (Srivastava et al., 2021). Thus, even if the climate models have different signals, pooling them and analysing them using our methodology with the Full-version permits to provide useful information on ToE and contribution values by taking into account the complete uncertainty in climate modelling.***”

Please note that, in agreement with the relocation of the results related to the Full-version in the Supplement, some of the figures in the article will be modified/reorganized. We here detail some of the main changes below. We stress that those changes are just a reorganization of the presentation of the results. It does not involve new figures and results.

- Figure 3: the right side of the flowchart concerning the Full-version will be deleted. A new figure for the flowchart concerning the Full-version will be added in the Supplement.
- Figure 7: Figs. 7a-c will be combined with Fig. S5a-c. A few sentences will be added to describe Fig. S5a-c in the main article..
- Figs. 7d-f will be combined with Figs. S5d-f in the Supplement.
- Figure 8: Bars corresponding to the Full-Ensemble version will be deleted. The resulting plot will be added in a panel in Fig.7 (panel Fig. 7g).
- Figures 9 and 10: Figs. 9d-f and Figs. 10d-f will be relocated in the Supplement into one figure. Figs. 9a-c and Figs. 10a-c will be combined into one new figure in the main article.
- Figure 11: Figure 11 will be deleted.
- Figure 12: Figs. 12a-c will be combined with Figs. S13a-c. A few sentences will be added to describe Figs. S13a-c in the main article.

- Figs. 12d-f will be combined with Figs. S13d-f in the Supplement.
- Figure 13: Bars corresponding to the Full-Ensemble version will be deleted. The resulting plot will be added in a panel in Fig. 12 (panel Fig. 12g)
- Figure S5: As explained, combine 7d-f with S5d-f.
- Figure S7 will be deleted.

Comment:

Minor issues:

line 46 and following: More relevant in this context is detection rather than attribution. This should also be mentioned.

Response:

Following this comment, we propose the following corrections (in red and blue), L46 (Section Introduction):

> "*Evaluating the ToE of compound hazards probabilities with respect to a baseline period — from which the natural variability is estimated — is valuable to analyse evolutions of compound events and attribute those to a specific cause, such as anthropogenic greenhouse gas emissions. **Detection and a**~~**A**~~ttribution is an important research field in climate science that aims at determining the mechanisms responsible for recent global warming and related climate changes **(Bindoff et al., 2013)**. For example, it can be done by comparing probabilities of an event between two worlds with different forcings (the "risk-based" approach, Stott et al., 2004; Shepherd, 2016).*"

Comment:

line 61: please refer to meteorological drought (no rain). You would likely find a change in soil moisture drought (low soil moisture).

Response:

We agree. We propose the following corrections (in blue), L61 (Section Introduction):

> "*For example, rising temperatures can naturally lead to more co-occurrences of hot temperatures and **meteorological** droughts, despite no significant trends in **meteorological** droughts are detected (Diffenbaugh et al., 2015; Mazdiyasni and AghaKouchak, 2015). However, in addition to warmer temperatures, the strengthening of the dependence between hot temperatures and **meteorological** droughts for future periods can also contribute to an increase in their co-occurrences (as highlighted in Zscheischler and Seneviratne, 2017).*"

Comment:

line 65 and following: This is not a general finding - it should be made clear for which type of CE this applies.

Response:

We propose to add the following corrections (in red and blue) starting at L64 of the initial article (Section Introduction):

> "*Several studies concluded about the importance of considering dependencies to assess CE properties and frequencies in a robust way**, e.g., for wind and precipitation extremes** (~~e.g.,~~ Hillier et al., 2020**), or temperature and precipitation (**~~;~~ Singh et al., 2021a; Vrac et al., 2021). Recently, Abatzoglou et al. (2020) even showed ~~, using reanalysis data, that changes in dependence properties have been more important than changes in univariate properties in the recent decades.~~**that, in the recent decades, changes in multivariate annual climatic conditions (water deficit, evapotranspiration, minimum and maximum temperature) with respect to a reference climate state have been more important than changes in univariate annual climatic conditions for a large portion of the Earth.***"

Comment:

line 161: It is not really an issue that there is no agreement. In fact, there cannot be a single baseline as there is no single baseline climate. The point is that you simply ask a slightly different question when choosing a different baseline (e.g. compared to preindustrial; 1950s, 1990s, or even little ice age or medieval warm period). Also, as argued above, the TOE wrt to present climate might even be more relevant. I would rephrase this sentence or just delete it and simply state "We choose...".

Response:

We propose to delete the sentence (in red) starting L161 of the initially submitted article:

> "*In this study, we consider the reference period (1871-1900) as baseline to assess the emergence of hazard probabilities. ~~However, there is no agreement on the choice of the baseline period for ToE studies.~~ While most of the studies choose a pre-industrial period as baseline to attribute emergence to anthropogenic greenhouse gas forcing (e.g., 1850-1900, Hawkins et al., 2020), other studies choose a more recent baseline period (e.g., 1951-1983, Ossó et al., 2022), which can provide relevant information for adaptation planning.*"

Comment:

Beginning of Section 3.3: you should clarify that the attribution of changes to changes in margins and dependence has already been introduced by Bevacqua et al., Sci. Adv. 2019. This also helps you to clarify the novel aspects of your contribution.

Response:

We agree and propose to add the following sentences (in blue) in the paragraph starting at L207 of the initial submitted article (Section 3.3 - Statistical method/Change of probabilities: contribution of the marginal and dependence properties):

> "*Then, do exceedance probability values change significantly between reference and future periods? And if so, how much of this change is due to changing marginal properties? To changing dependence structure?* ***Attributing probability changes to changes of marginal and dependence properties has already been introduced by Bevacqua et al. (2019) to analyse compound flooding from precipitation and storm surge in Europe. However, to our knowledge, assessing those changes relative to a reference natural variability in a ToE context has never been done yet****. In order to isolate the effects of these potentially changing statistical properties, we propose to calculate two additional exceedance probability values.*"

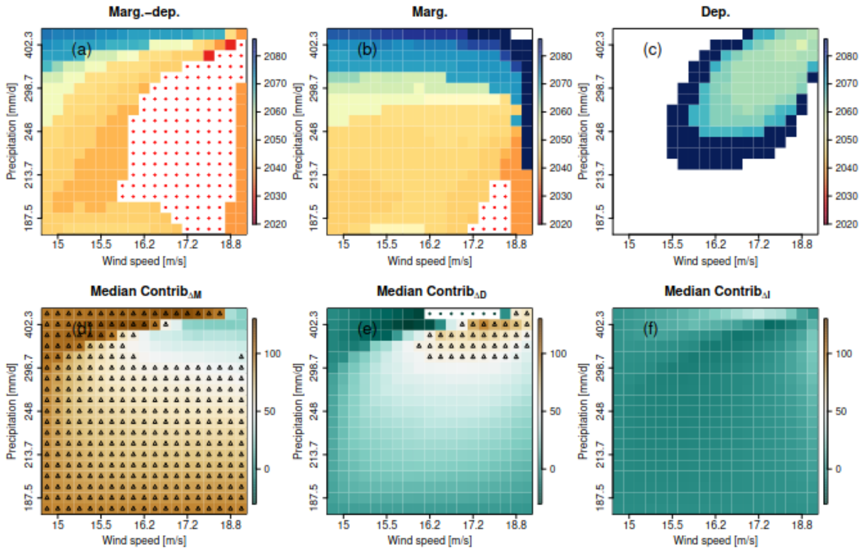
Comment:

Figure 6: the color bar is hardly visible. Please plot just one for each row and then broaden!

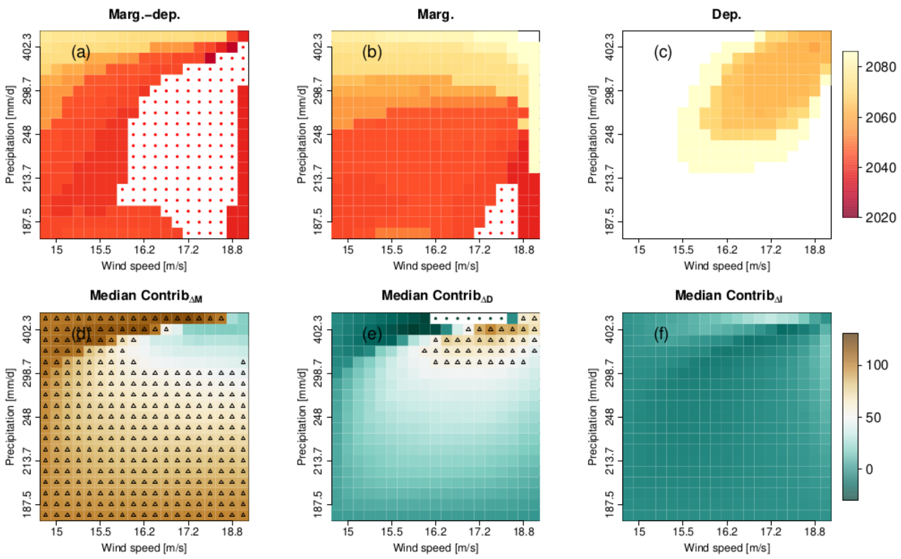
Response:

We agree on this comment not only for Fig. 6 but also for all the other figures, when applicable. As illustration, we show below the changes made for Fig. 6. Please note that we also considered the modification of the colour scale for ToE values.

Previous Fig. 6:



Proposed new Fig. 6:



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