

Nat. Hazards Earth Syst. Sci. Discuss., referee comment RC2  
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## Review report for nhess-2022-127

Anonymous Referee #2

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Referee comment on "Time of emergence of compound events: contribution of univariate and dependence properties" by Bastien François and Mathieu Vrac, Nat. Hazards Earth Syst. Sci. Discuss., <https://doi.org/10.5194/nhess-2022-127-RC2>, 2022

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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.

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 Brittany. CMIP6 models have a rather coarse resolution (mostly coarser than the chosen grid of  $0.5^\circ \times 0.5^\circ$ ). 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).

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.

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!

Minor issues:

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

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

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

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...".

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.

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