

Earth Syst. Dynam. Discuss., referee comment RC2
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Comment on esd-2021-52

Anonymous Referee #2

Referee comment on "A methodology for the spatiotemporal identification of compound hazards: wind and precipitation extremes in Great Britain (1979–2019)" by Aloïs Tilloy et al., Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2021-52-RC2>, 2022

This manuscript provides a clear and effective contribution to discerning and evaluating compound natural hazards, basically taking into consideration spatiotemporal clustering procedures to detect and classify the aggregation of such hazards with well-explicated metrics (though naturally there are a lot of nuances and details enriching the work). Overall, the work is very transparent, well explained and operationable, especially bearing into account the provided supplements with data and code that can be aptly worked on in an understandable manner. As such, this provides a valuable contribution not only at scholarly level but also in operational services.

This being said, and having seen the previous reviewer report, I will not repeat what is already there and to which I naturally concur.

My minor remarks thus come down to the following aspects:

1) To those who might wonder why using the metrics and assumptions sustaining the methodological development and deployment in the manuscript, rather than other alternative ways to detect and potentially attribute the diagnostics made in the paper?

2) Several methodologies for spatiotemporal compound event identification fall prey to the self-fulfilling prophecy of detecting what we want to detect through tuning the methodologies. Fortunately in this paper the procedure is sufficiently objective to minimise such risk. Could the authors elaborate in brief terms how their methodological choices fare better in this regard than the panoply of traditional process-blind statistical methods?

3) In keeping the interdisciplinary inter-domain philosophy of the ESD journal in mind,

could the authors elaborate a little further on the physical interpretation of the results, namely linking to ocean-atmospheric and land-atmospheric aspects that might help explain the results?

Overall, this is a solid contribution, clearly one of the rare occasions in which the preprint itself would already be a worthy final paper. By addressing the concerns of the other reviewer and minor ones remaining here, this review report intends essentially to slightly raise the bar of the work from very good to excellent. Especially in further stressing the differentiating assets of the methodological developments and most importantly beefing up the geophysical reasoning to further help the more physically minded readers make further sense of the results and potentialities of the study, besides what was already made clear in that regard. Again, very good work, a short notch away from excellence.

Looking forward to reading the revised version.