I appreciate the effort of the authors in answering my comments / criticism and for implementing many of them in the new version of the manuscript. It think that the quality of the paper improved, and especially, the results are not just reported, but also explained and interpreted in the paper in a more critical way. I understand that it is difficult to perform attribution studies due to the limited amount of observations and different model biases, however this does not exclude a critical interpretation and a qualitatively high presentation of the results in the manuscript. I do not question that the message of the paper is very relevant and that action from authorities is urgently needed in order to prepare for the increasing frequency of heat events.

After reading the response of the authors and the new version of the manuscript, I still have a few comments / suggestions:

1) I think that the sentence "*it could either occur by chance or nonlinear effects have made such heatwave possible*" (L. 280-281) is an oversimplification leading to misunderstandings, thus it needs some clarification should be rephrased.
   We often model extreme events as random variables, but the climate is essentially a chaotic deterministic system. It behaves at certain scales as if it would be random (this is why the approximations using random variables often works), nonetheless extreme events do not happen by chance. What the authors actually mean, I suppose, is that we observe "by chance" a very low-probability event in a relatively short time series. This is explained at the beginning of Sec. 3, but this and similar sentences still appear in the manuscript.
   I am also not convinced that the only alternative to an event "by chance" are "nonlinear effects" or, as mentioned in the abstract, "new nonlinearities". It is possible that global warming makes some well-known processes more (or less) possible thus changing the distribution of extreme events. "New nonlinearities" sounds for me too specific considering that the authors did not study this issue directly.

2) L. 439-440 and L. 210-211"*Also further research is needed into the limitations of standard GEV analysis on annual maxima with short records and seemingly non-stationary behavior*."
   The GEV approach is formulated for independent, identically distributed, random variables. It can be still applied to correlated data, in case the correlations are weak enough and the block maxima are uncorrelated. However, it is not surprising that it does not work if these conditions are strongly violated due to the non-stationarity induced by global warming. This issue is often addressed by assuming a time dependence of GEV parameters, however there is no theoretical support for these kind of dependencies. It is well known as well that the method is quite data-hungry because it considers only the maximum of each block. Furthermore, it is an asymptotic method, thus it is valid only in case the block size is large enough, and the convergence to the asymptotic distribution can be extremely slow. I do not question the advantages of the method, but the above mentioned application issues are well known in case the data does not satisfy the necessary conditions. Thus, care is needed when applying this method to observational data sets and non-stationary model simulations. But, again, these problems are well-know, thus I do not see the usefulness of the mentioned future studies.

   These issues are thoroughly explained in:
   Coles, "Introduction to Statistical Modeling of Extreme Values", *Springer*, New York, NY, USA, 2001.

For convergence issues and the problem of limited data size see, for example:

Vannitsem, “Statistical properties of the temperature maxima in an intermediate order Quasi-Geostrophic model,” *Tellus, Series A: Dynamic Meteorology and Oceanography*, vol. 59, no. 1, pp. 80–95, 2007.

Felici, Lucarini, Speranza, and Vitolo, “Extreme value statistics of the total energy in an intermediate-complexity model of the midlatitude atmospheric jet. Part I: Stationary case”, *Journal of the Atmospheric Sciences*, vol. 64, no. 7, pp. 2137–2158, 2007.

Gálfi, Bódai and Lucarini, Convergence of extreme value statistics in a two-layer quasi-geostrophic atmospheric model, *Complexity*, 5340858, 2017

3) The vast number of data sets and methods used in attribution studies seem to lead to shortcomings in the presentation of these methods and the interpretation of the results. If this could be avoided, however, it would make attributions studies more accessible for a broader audience and would reduce the risk of misinterpretation.
   I am aware that there is a vast body of relevant scientific literature and the authors cannot explain every detail of their methods in the manuscript. However, I firmly believe that the relevant methods should be properly explained, even if very concisely. It is not reader friendly at all to give a list of papers and expect the reader to go through a whole body of literature just to understand one manuscript. Nonetheless, I think that also from this aspect the manuscript improved with respect to the previous version.

4) *Fig. 15* is too small, it is impossible to see the arrows illustrating the wind direction.

5) The language of the manuscript improved as well, but it still needs some revisions in terms of understandability and typos.