Reviewer 1

In this paper, the authors tackle the problem of attributing extreme events to climate change. They take the angle of conditioning extreme events on the regional circulation, as captured by flow analogues, over Europe in the considered cases. This has the advantage of allowing a more ‘apples-to-apples’ comparison of dynamically similar events, in contrast to the more standard GEV approach based on annual block maxima.

The paper is methodologically focused, and aims to demonstrate the validity and usefulness of this flow-dependent approach on three high profile European extreme events from recent years, and compares the findings to both an alternate detrending procedure and a version of the GEV/ETV approach. They also explore the sensitivity of their method to parameter variations. I think the approach is very interesting; it is definitely sensible and insightful to integrate circulation information into the attribution process.

However the conclusions the authors’ analysis provides for the given cases are rather unusual, and after a few careful reads I think the authors either need to better evidence the plausibility of their findings or revise their method. Therefore I am suggesting major revisions. I detail my objection immediately below, followed by a number of minor and typographical comments.

We thank the reviewer for considering our work positively. We share part of the concerns expressed by the reviewer with regards to the results displayed. In the following we address their comments.

To quickly summarise what the analogue-based attribution shows for each case study:

- The heatwave was made ~4 degrees warmer by climate change, and ~100,000x more likely. The EVT approach claims a similar intensity change (>3 degrees), but only deems the event 10-100x more likely.
- No significant change in probability ratio or intensity of the wind extreme. The EVT approach seems to indicate reduced probability/intensity, but not significantly.
- The precipitation event was made ~100x less likely and was made 8-20 mm/day weaker (or 20-50mm/day weaker; there is an inconsistency between text and figure 3i). The EVT approach suggests a slight, not very significant, increase in probability and intensity. The precipitation case gives me the most cause for concern. Looking at how the logarithmic fit in 3c,f squeezes the distribution so drastically (transforming a >25mm/day event into a <10mm/day event in the current climate), it seems very hard to believe this is a sensible transformation to apply.

Under the EVT approach in fig 6f meanwhile, the change in distribution is small. To me this makes sense given the fairly weak apparent correlation between RMST and total precip in 3c.
Coming from another perspective, and thinking about the meteorology, the 4 Oct 2021 event was a result of an extratropical cyclone tracking across the Mediterranean, with the anticyclonic anomaly to the east helping to stall its eastward movement. Do we really expect the probability of extreme rainfall when a cyclone hits the coast to have decreased by 90-99% relative to 1950? Or indeed, to have had its intensity ~halved? This runs counter to basic theory (Clausius-Clapeyron relation) and previous work that find little observational

change in cyclone properties. It seems to me that either the logarithmic method of trend fitting is producing spurious results, and/or the similarity metric used to define analogues is not faithful to the driving meteorology (e.g. the analogues don't include cyclones when the true flow does). I suspect that defining analogues based solely on Z500 over a large area is too coarse an approach.

We agree with the reviewer that the results obtained for the precipitation event are implausible with respect to the current knowledge of the change in the dynamics of cyclones and high precipitations in the Mediterranean. We investigated the two points raised by the reviewer:

(i) after inspection of the analogues values for precipitation for several grid points, we saw no evidence that the logarithmic method of trend fitting would be incompatible with the structure of the data (not shown)

(ii) however, when looking at the synoptic composite maps of both the Z500 and precipitation for the analogues found with our initial choices, it was clear that the structure of the precipitation event was not recovered by the analogues (see Figure 1 showing this structure for the new analogues).

As suggested by the reviewer, this is indeed likely because the area chosen for finding analogues for this event was too large. In the updated version of the manuscript we reduced this area to be closer to the region where the event actually occurred. This led to a much reduced estimation of the negative intensity changes (see Figure 3 and Figure 4), which is more coherent with the results obtained with the GEV. Despite the fact that the analogues quality metric for this event is good compared to the two other events (Figure 2), the synoptic field is not as similar as the event itself compared to the two other events (Figure 1f). We therefore agree that the results obtained for this event should be taken with care - similarly to those obtained with the GEV method though. We discuss those points more extensively in the result and discussion section (see below).

One may wonder why the results obtained here do not seem to follow the expected increase of precipitation with increasing temperature following the Clausius-Clapeyron law. To explore this discrepancy, we show below (Figure R1) the intensity change of the 2m air temperature obtained using the analogues of the precipitation event. In most of the area concerned with the high precipitations (south of the Alps), there is no significant change in the temperature field. This is coherent with no significant change in the precipitation field for these analogues. Why the analogues of this event are not associated with an increasing temperature is not clear though. We checked whether the analogues in the more recent years are found in a climatologically colder part of the year (shift in the seasonality of the analogues), but found no evidence for such a shift (not shown). It suggests that the warming signal depends on the atmospheric circulation, which also highlights the interest of conditioning on large scale circulation. These elements are now mentioned in the text: L379-382.

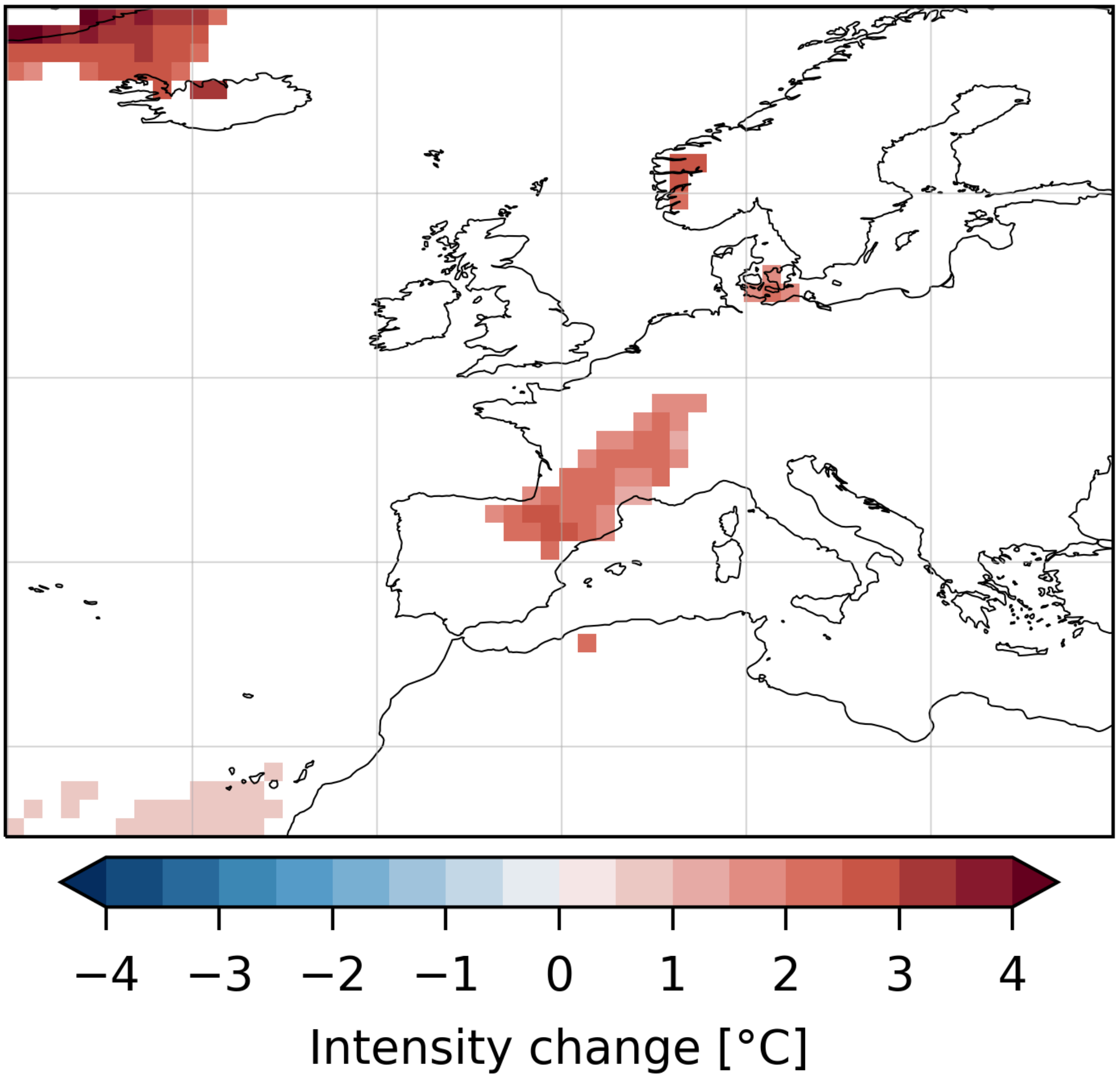

Figure R1: Intensity change of the 2m air temperature for the analogues of the precipitation event

I am also a bit worried about the high probability ratios estimated for the heat case. You mention later that probability ratios have many issues, but the fact remains that your method produces PR estimates 1000x that of the EVT approach. I don't think this is unrelated from the fact that the EVT distribution is positive skewed. While you've fitted a skew normal for the analogues, the distribution has ended up very gaussian so the estimation of tail probabilities is going to be very unreliable. Again, there could be a possible issue with the analogues: it wouldn't take a large distortion of the flow field to put central France under a trough.

We agree with the reviewer that the large difference in the PR in our approach compared to the EVT approach is due to the positively skewed temperature distribution fitted using a GEV. We first note that this difference in the skewness reflects the difference in the skewness of the original data, which is largely due to one data point only: the event itself in 2019. If we remove this data point from the fit, the

skewness is not as pronounced and the median PR goes from around 5 to around 200, denoting the sensitivity to just one data point of tail probabilities estimated using a GEV distribution (see Figure R2). Therefore, the estimation of these tail probabilities both with our method and the GEV method is problematic for such very intense extremes. Nevertheless, as we state in the discussion, even though the absolute values of the IC and the PR are uncertain, there is no doubt in this case that the event is attributable. Also, the direct comparison of the PR between the analogues and the EVT methods is not straightforward a priori and it is not clear how they should relate one to each other, because one is conditional and the other is not. We do not think that there is an issue in the quality of the analogues found for this event, as their composite is very close to the actual event in this case (see Figure 1d).

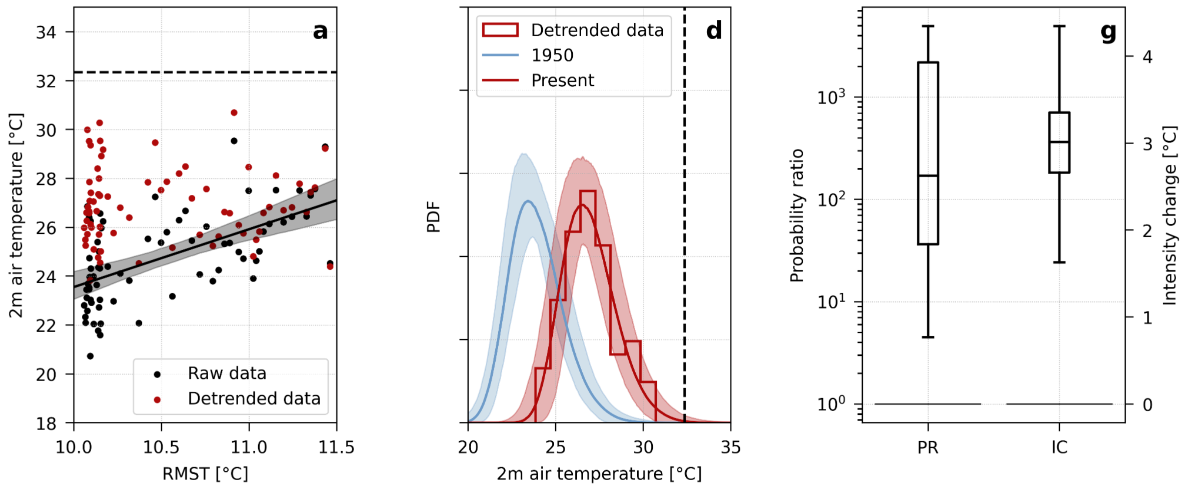

Figure R2: as Figure 6 first column while removing the event itself from the fit.

I ask the authors to revise or more fully justify these more implausible findings I've mentioned. It might aid interpretation to actually plot ~4 analogues (say the 15th, 30th, 45th and 60th closest analogues) for each case in the appendix, to see if a mismatch of meteorology between the event and analogues does indeed explain this.

We thank the reviewer for this suggestion. However, showing just a few analogues may not be very informative of their full distribution. In the updated version of the article we rather added a figure (Figure 1def and L197-203) showing the average of the Z500 field and the metric field (temperature, wind and precipitation) for the analogues found with our method. Except maybe for the precipitation event, the synoptic mean field is close to the one of the event itself.

Minor/typographical comments

1. Line 12: what is a 'climatological event'? A little unclear to me. Also 'negatively affect' reads more naturally than 'affect negatively'

The 2021 IPCC report defines a climatological extreme event as "a pattern of extreme weather persisting for some time". We agree with the reviewer that this definition is

somewhat arbitrary but we prefer to keep it here to emphasize the possible different time scales involved. The rest of the sentence was corrected as suggested.

2. Line 15: ‘which aim is to’

This has been corrected.

3. Line 31: Given ‘very low’ doesn’t have a well defined quantitative meaning, I’d just say ‘estimating low probabilities’

It could be possible to define a difference between low probability events (events for which few previous cases exist) and very low probability events (unseen events), but we agree with the reviewer that this may be confusing and we have modified the text as suggested.

4. Line 48: ‘These methods both condition the ...’

This has been corrected as suggested.

5. Line 57: I think by this point in the introduction it would be good to explicitly state the assumption of your work that changes in the synoptic dynamics themselves due to climate change are negligible/ should be treated separately. The conditional attribution is informative but by definition only a partial attribution to thermodynamic changes. As far as I can see this is not written in the introduction, and should be clearly emphasised.

The following sentence has been added in the introduction as suggested: L55 “The conditional attribution thus separates the thermodynamical and dynamical changes due to climate change and addresses only the former.”

6. Line 62: ‘This method is used, for example, by the...’

This has been corrected.

7. Line 68: ‘Which lead to exceptional heat in...’

This has been corrected.

8. Line 69: ‘Western of Europe’

This has been corrected.

9. Line 71: ‘precipitations’ (and several other times in the document)

This has been corrected.

10. Line 81: Can you motivate why you take a 5 day mean for precip? I presume to focus on the synoptic driving.

The use of 5 day mean for precipitations is indeed intended to focus on the synoptic driving while daily precipitations risk being too variable to identify a signal using imperfect analogs.
The following sentence was added in the text: L81 "Using a 5-day rolling mean for precipitation allows us to focus on the synoptic driving rather than day-to-day variability."

11. Line 86: Even 1 degree is a fairly strong constraint on shifts. It might be interesting to see results for a 2.5/3 deg box average centred on the three points examined in figure 3.

The results for a 3 degrees box centered on the three grid points in Figure 3 are presented in Figure R3 below. They are very similar to the ones of Figure 3.

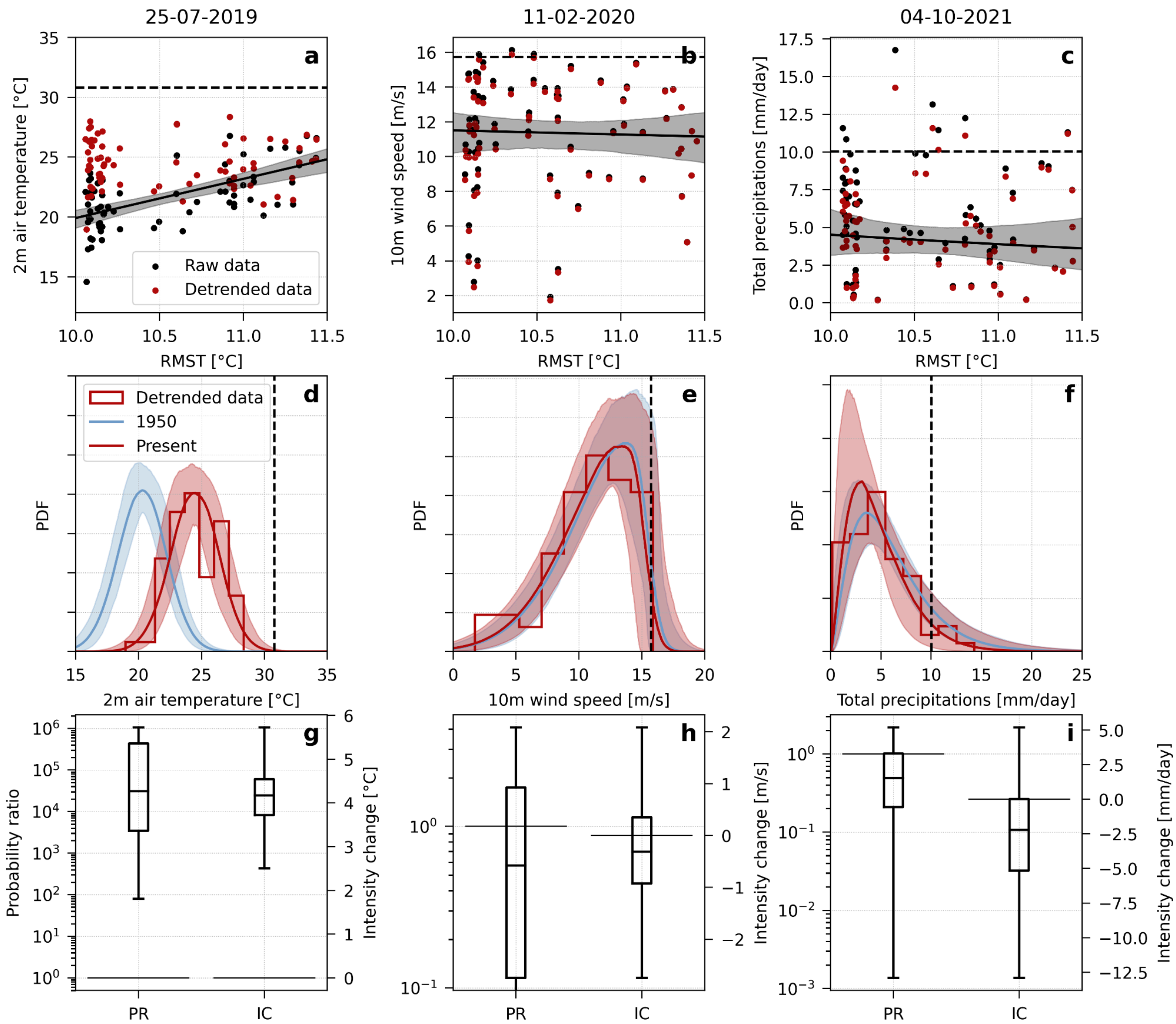

Figure R3: as in Figure 3 but using a 3° by 3° box average centered on the grid points rather than the value at the grid point.

12. Line 103: Clearer to say "thickening due to warming of the atmosphere".

This has been changed as suggested.

13. Line 111: Its not totally clear in what space you compute the analogues. The full high-dimensional space of gridpoint values within each region? That's giving you a O(1000) dimensional space. Given that distance vectors in high-dimensional space tend towards equal length (https://link.springer.com/chapter/10.1007/3-540-44503-X_27) which may lead to poor ability to detect analogues, did you consider computing analogues in an O(10-100) dimensional PCA space?

We indeed compute analogues in the full O(1000) dimensional space of the Z500 field over the region considered, as in many other papers using analogues (e.g. Faranda et al. 2024). We understand the concern of the reviewer with regards to the ability of the method to detect good analogues in this high dimensional space, however in this space the actual number of degrees of freedom is much smaller than O(1000) because a lot of grid points are highly correlated. Tests made using a projection in a lower dimensional space, such as one obtained using a PCA, did not lead to major changes in the analogues found (see also Yiou (2014) and Jezequel et al. (2018)). For simplicity we therefore preferred using the original space.

14. Line 127: impossible from the thermodynamic perspective, but this could be tackled by considering how synoptic conditions are being impacted by climate change.

The conditional approach presented here - and the other methods cited in introduction also - is indeed close to a “thermodynamic” perspective for which we assume that the synoptic conditions have not changed and we project the event in the world with or without climate change. From the unconditional perspective, whether or not the dynamics of the yearly maximum events are different from the event studied is irrelevant as one only compares the intensity levels reached. We are not sure to understand what the reviewer refers to when they mentions “considering how synoptic conditions are being impacted by climate change”, which is likely something difficult to assess with past data only (see also L57-58).

15. Line 136: What is the motivation for using surface temperature for something like precip, given condensation happens aloft? If RMST is used just for simplicity then maybe make a comment about this somewhere.

Here we do not really use surface temperature per se as a covariate - at least not local surface temperature - but RMST as a measure of the non stationarity of the climate system. This choice is standard in the attribution literature and we follow this practice here.

16. Line 199: Worst-> worse

This has been corrected.

17. Fig 3abc: You should state in the figure caption that the ‘detrended’ data is shifted to the 2020 RMST value. It makes sense for your application, but isn’t a standard detrending, where you’d expect to end up with zero mean displacement of the data.

This has been added as suggested.

18. Line 263: The intensity change your report does not correspond to what you show in fig 3i.

This has been corrected.

19. The captions for figures B5 and B6 are the same.

This has been corrected.

20. Line 345: missing reference.

This has been corrected.

References

Jézéquel, A., Yiou, P., & Radanovics, S. (2018). Role of circulation in European heatwaves using flow analogues. *Climate dynamics*, *50*(3), 1145-1159.

Yiou, P. (2014). AnaWEGE: a weather generator based on analogues of atmospheric circulation. *Geoscientific Model Development*, *7*(2), 531-543.

Faranda, D., Vrac, M., Yiou, P., Jézéquel, A., & Thao, S. (2020). Changes in future synoptic circulation patterns: consequences for extreme event attribution. *Geophysical Research Letters*, *47*(15), e2020GL088002.

Platzer, P., Yiou, P., Naveau, P., Filipot, J. F., Thiébaut, M., & Tandeo, P. (2021). Probability distributions for analog-to-target distances. *Journal of the Atmospheric Sciences*, *78*(10), 3317-3335.