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Reply on RC1

Davide Faranda et al.

Author comment on "A climate-change attribution retrospective of some impactful weather extremes of 2021" by Davide Faranda et al., Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2022-9-AC1, 2022

Replies to Reviewer 1 (Authors' answers are displayed in bold)

Overall comments

This paper uses atmospheric circulation analogues to study eight extreme weather events in 2021 which occurred mainly over Europe (with one over the eastern USA), asking how they may have been affected by anthropogenic climate change, and in particular, by forced changes in the properties of the circulation regimes --- which is to say, changes in dynamics. The question is topical, and interesting. The attribution of changes in extreme weather, and predictions of future changes in extremes, are invariably based on thermodynamics, because that is the aspect of climate change in which there is high confidence. Yet extreme weather events invariably involve particular dynamical conditions, implying that changes in those dynamical conditions could potentially be part of the response to anthropogenic climate change. The difficulty is that any such changes are far more uncertain than the thermodynamic changes, as has been widely discussed in the literature. The contrast in levels of confidence is most apparent in the statements issued by the IPCC on this matter.

What this paper does, then, is to assess the relative contribution of changes in thermodynamics (by which I mean the difference in hazard conditional on the circulation analogue) and changes in dynamics (by which I mean the changes in properties of the circulation analogues), partly empirically (by identifying changes in the ERA5 reanalysis between 1950-1979 and 1992-2021), and partly theoretically (by drawing on existing literature, especially the AR6 report). It does this for a wide range of different kinds of extreme weather events. The synoptic descriptions of the events are very nice, and the format of the paper is potentially an interesting one. The problem is that the methodology is highly problematical, and the conclusions drawn are in many cases far too definitive. I am also absolutely shocked by how the conclusions of the AR6 have been twisted by the authors in a highly misleading fashion. Thus, the paper is completely unpublishable in its present form.

I briefly summarize the conclusions of the eight case studies (the justification for my statements is provided in my detailed comments):

Winter storm Filomena: unsubstantiated claim

French Spring cold spell: the expected result from thermodynamics

Westphalia Floods: unsubstantiated claim

Mediterranean summer heatwave: the expected result from thermodynamics

Hurricane Ida: the expected result from thermodynamics

Po Valley tornadoes outbreak: inconclusive (as in the IPCC for tornadoes in general)

Medicane Apollo: unsubstantiated claim

Late autumn Scandinavian cold-spell: such cold extremes do not warm

With the exception of the last one, which seems potentially interesting, the attribution statements are either what one would expect from the IPCC reports, or they involve unsubstantiated claims. That would not seem to provide much basis for a publication.

However, I can imagine a reframing of the results which could be publishable. For cases where there are good analogues, no apparent changes in those analogues between 1950-1979 and 1992-2021, and a thermodynamic effect which is consistent with physical understanding, the analysis supports a purely thermodynamic attribution of the event, allowing a strong statement to be made. That is interesting in itself. For cases where there are good analogues and apparent changes in those analogues between 1950-1979 and 1992-2021, the analysis raises questions about potential changes in dynamical conditions in response to climate change. One cannot simply assume, as is done here, that any such changes are anthropogenic; but one can articulate various hypotheses, based on the literature. That would improve the quality of extreme event attribution and risk assessment. And for cases where there are no good analogues, as is found here for Filomena and Apollo, the identification of the events as 'black swans' is very important as it suggests that a standard statistical attribution would be completely unjustified. This would also improve the quality of extreme event attribution, as well as stimulating research into how to treat such singular events.

Thus, I would suggest building more explicitly from the synoptic descriptions of the events, which I found to be much along the lines of the wonderful study of Black et al. (2004 Weather) on the 2003 European heat wave, and then articulate the contribution of thermodynamic and of plausible dynamical responses to climate change, being careful to avoid categorical language in the latter case (see detailed comments below). This could be done without a drastic overhaul of the manuscript, hence my recommendation of major revision rather than rejection. However, I need to emphasize quite firmly that many of the problems I identify below are fatal, in my view, if not addressed.

We thank the reviewer for reading our manuscript in detail. We appreciate that the reviewer recognizes the importance of attribution studies focusing on the dynamics and that they enjoyed the synoptic description of the events. We do understand the criticisms expressed and we are willing to review the manuscript following their suggestions. We believe that many of the comments can be addressed by a substantial reformulation of the paper in the next revision, both when it comes to the discussion of the existing literature and the interpretation

of the results. In particular we will rephrase the results as suggested, dividing the events analyzed in three categories: those for which our results suggest a pure thermodynamic role, those for which the results suggest also a dynamical component and the ones that are in practice unattributable because they lack analogs (black swans). This will imply a rewriting of the interpretation part of these events. Furthermore, to avoid confusion and misinterpretation of the IPCC AR6 report, in the next version of the paper, we will make a clear separation between IPCC statements (that will be reported within quotes) and other results available in the scientific literature. We will also relax the assumption that the changes we identify are necessarily anthropogenically driven, better describing our hypothesis about taking 30 years of factual and counterfactual worlds, as is done in many attribution studies (see, e.g. Vautard et al. 2016, Paciorek et al. 2018, Van Oldenborgh et al. 2019) .

Vautard, R., Yiou, P., Otto, F., Stott, P., Christidis, N., Van Oldenborgh, G. J., & Schaller, N. (2016). Attribution of human-induced dynamical and thermodynamical contributions in extreme weather events. *Environmental Research Letters*, *11*(11), 114009.

Paciorek, C. J., Stone, D. A., & Wehner, M. F. (2018). Quantifying statistical uncertainty in the attribution of human influence on severe weather. *Weather and climate extremes*, *20*, 69-80.

Van Oldenborgh, G. J., Philip, S., Kew, S., Vautard, R., Boucher, O., Otto, F., ... & van Aalst, M. (2019). Human contribution to the record-breaking June 2019 heat wave in France. *World Weather Attribution*.

Detailed comments

My jaw dropped when reading the very first paragraph of the paper (lines 19-29), which I regard as a complete misrepresentation of the conclusions of the AR6 WGI report. The SPM is cited in support of the statement that "anthropogenic climate change is critically affecting the dynamics of weather extremes" (line 20). When I read the SPM for statements about extremes, I can find no statement that could remotely be construed as supporting any conclusion about attribution of changes in the dynamics of extremes to anthropogenic climate change. (If I missed something, I would be happy to be corrected on this point.) On the contrary, all SPM statements about changes in extremes would appear to be anchored in thermodynamics. Even if the length of heat waves is increasing, this does not imply a change in dynamics; it is simply that if the mean temperature increases, then the mean time of exceedance above a fixed temperature threshold will necessarily increase (all else being equal). I found two statements in the SPM concerning potential changes in dynamics:

"B.3.2 A warmer climate will intensify very wet and very dry weather and climate events and seasons, with implications for flooding or drought (high confidence), but the location and frequency of these events depend on projected changes in regional atmospheric circulation, including monsoons and mid-latitude storm tracks."

"C.1.3 Internal variability has largely been responsible for the amplification and attenuation of the observed human-caused decadal-to-multi-decadal mean precipitation changes in many land regions (high confidence)."

The first statement says that future changes could depend on dynamics, which is a truism, but is certainly not implying any kind of attribution or definitive knowledge. The second statement is suggesting that dynamical modulation of the thermodynamic changes seen so far can be mainly attributed to internal variability, not anthropogenic climate change. Thus, the AR6 SPM is telling us that any forced changes in the dynamics of extremes are highly uncertain, and that any observed changes are dominated by internal variability.

I also could find no statement in the Executive Summary of AR6 Chapter 11 (on extremes) that could remotely be construed as supporting any conclusion about attribution of changes in the dynamics of extremes to anthropogenic climate change.

We accept the criticism that we have been not precise in quoting the AR6 report here, and we should have also referred to the full report or the technical summary rather than the SPM only. However, we want to stress to the reviewer, the editorial board and the readers of WCD that our intention was not to "twist" the statements of the IPCC Report. We will revise the paper according to the reviewer's suggestion and tone down our statements. In particular, the sentence : "anthropogenic climate change is critically affecting the dynamics of weather extremes" will be substituted by "anthropogenic climate change is critically affecting weather extremes". Indeed, the nature of these changes is mostly thermodynamics, and the IPCC report provides limited evidence that potential changes in extreme events dynamics could be due to anthropogenic changes. As quoted by the Reviewer, the SPM report states that: "B.3.2 A warmer climate will intensify very wet and very dry weather and climate events and seasons, with implications for flooding or drought (high confidence), but the location and frequency of these events depend on projected changes in regional atmospheric circulation, including monsoons and mid-latitude storm tracks." The technical summary further states that: "TS 2.3 It is likely that the mid-latitude jet will shift poleward and strengthen, accompanied by a strengthening of the storm track in the Southern Hemisphere by 2100 under the high CO2 emissions scenarios." These quotes support the idea that future changes in specific extreme events, including hydrological extremes and mid-latitude cyclones, could also depend on the dynamics, and that for the storm-track dynamics human influence may be playing a role.

In lines 20-23, this paper states "For summer, the AR6 report states that we are already observing prolonged periods of extremely warm conditions (Horton et al., 2016) with increased droughts leading to forest fires (Flannigan et al., 2000), species extinctions (Román-Palacios and Wiens, 2020) and health issues for vulnerable populations (Mitchell et al., 2016)."

As an elaboration of the preceding statement about changes in the dynamics of extremes, and as a characterization of the AR6, this is completely misleading. Horton et al. (2016) discusses the potential for changes in dynamics to affect the nature of heat extremes, but makes clear that any such changes are highly uncertain and controversial, and in any case this paper is not cited by AR6 Chapter 11. Perhaps the authors meant Horton et al. (2015), which is cited by AR6 Chapter 11, but that paper is about observed trends and makes no attribution, as would be implied by the word "already". As noted above, prolonged periods of warm conditions are a straightforward consequence of mean warming and do not require a dynamical explanation. The statements about impacts are similarly anchored in thermodynamic mechanisms.

We will follow the suggestion of the reviewer by rephrasing accordingly.

In lines 23-24, this paper states "In winter, increased persistence of cyclonic and anticyclonic structures leads to extremely wet and dry periods (Ogawa et al., 2018)"

The wording suggests that such a conclusion can be found in the AR6, but I could find no reference to Ogawa et al. (2018) in Chapter 11, and as noted earlier, no attribution of changes in the dynamics of extremes in the SPM or the Executive Summary of Chapter 11.

We were unclear in this passage, and indeed did not intend to refer to the AR6 any longer. In the revised text we will clarify that we are citing other relevant scientific literature, not necessarily linked to AR6. We will rephrase this sentence as: "Besides the IPCC AR6 report, a large body of recent scientific literature points to the need of understanding the role of dynamical drivers in extremes: in winter, increased persistence of cyclonic and anticyclonic structures leads to extremely wet and dry periods (Ogawa et al., 2018)"

In lines 25-27, this paper states "Finally, the IPCC also warns that, in the shoulder seasons, we observe a large variability of rains associated with both tropical and extratropical storms and convective events, leading to an alteration of the hydrological cycle (Gordon et al., 2005; Bala et al., 2010; Pendergrass et al., 2017)."I could find no support for this statement either in Chapter 11 or in Chapter 8 (on the hydrological cycle). There is an argument made for an increasing number of dry days in many regions, but again the argument is based on thermodynamics/energetics.

Here, we will remove the reference to the IPCC: "Gordon et al., 2005; Bala et al., 2010 and Pendergrass et al., 2017 suggest that, in the shoulder seasons, we observe a large variability of rains associated with both tropical and extratropical storms and convective events, leading to an alteration of the hydrological cycle.

Finally, in lines 27-29, this paper states "These trends are expected to accelerate in the coming years, if the global efforts to reduce carbon emissions are not implemented swiftly (Trisos et al., 2020)." Trisos et al. (2020) is about biodiversity loss and while I haven't read the paper, I would be surprised if it was not based on the sort of thermodynamic arguments for increased hazard represented in the AR6. It seems highly misleading to use that reference the way it is used here.

We will remove the quoted sentence

I must admit that I am at something of a loss when I read the first paragraph of a paper and find that every single sentence is highly misleading and a severe distortion of the literature. In fact, I don't believe that I have ever had that experience before. However, this paragraph is only intended to be motivational, and I have no disagreement with what is said in the rest of p.2, or the subsequent motivation for this study. Thus, while the opening paragraph definitely needs a complete overhaul, I will press on with my review.

We hope that the previous answers fully address the concerns of the reviewer, provide a truer account of the IPCC AR6 statements and clearly separate these from the rest of the literature discussed in the introduction

Lines 53-57: This wording appears to be suggesting that the authors consider any change in any atmospheric statistic between 1950-1979 and 1992-2021 to be the forced response to anthropogenic climate change. Quite apart from the potential inhomogeneity issue arising from comparing reanalyses prior to 1980 (before the satellite era) and after 1980 – which is always a serious concern, and needs to be addressed here – this approach seems far too liberal, and out of step with both the scientific understanding of multi-decadal variability (which is reflected, e.g., in erratic multi-decadal trends in the NAO), and the IPCC D&A framework which requires agreement between the spatial fingerprints of an observed trend and a well-accepted prediction from climate models in order to make any such attribution.

We understand the concern of the reviewer. We can argue that internal variability is the same in two distinct 30 year periods. The homogeneity issue is well known to affect essentially high latitudes and tropical regions (e.g., G. Sturaro, A closer look at the climatological discontinuities present in the NCEP/NCAR reanalysis temperature due to the introduction of satellite data, Climate dynamics 21 (2003) 309–316.). Indeed, a check of our assumption is that the probability density function of analog distances of any day is uniformly distributed between 1950 and 2021. Therefore our approach is not sensitive to this homogeneity problem (we compare atmospheric patterns, not pointwise values). In the text we will use this paragraph "as is", but provide a justification later in the data section for the problems raised by the reviewer. In addition, our approach to extreme events attribution is in line with the National Academy of Sciences "Attribution of Extreme Weather Events in the Context of Climate Change" rather than the detection and attribution approach outlined in the IPCC reports. We will be more clear about this in the new version of the manuscript .

National Academies of Sciences, Division on Earth and Life Studies, Board on Atmospheric Sciences and Climate, & Committee on Extreme Weather Events and Climate Change Attribution. (2016). ***Attribution of extreme weather events in the***

***context of climate change*. National Academies Press. DOI: 10.17226/21852**

Lines 64-65: How do you account for the large-scale changes in slp associated with the thermodynamic effects of climate change, which presumably don't affect the circulation (since for circulation it is the horizontal gradients that matter), but would affect the Euclidean distances? Note that Chapter 10 of the IPCC AR4 report had a strong statement about increases in the strength of extratropical cyclones from climate change, largely based on the single study of Lambert & Fyfe (2006); it was subsequently recognized that taking minimum slp as a metric for extratropical cyclone intensity was fallacious as it was subject to the confounding influence of large-scale slp changes, and the AR5 had to row back on this statement. How can you convince the reader that you are not prone to the same problem? This might particularly affect the persistence metric.

The rationale for using the sea-level pressure (and we note here that we use the entire map and not only the sea-level pressure minima) is that this observable is less subject to long term trends induced by the thermodynamic warming than, for example, Z500. Although the Lambert & Fyfe paper is interesting, it is largely outdated (AR4: horizontal resolution of 400km) and only discusses model simulations. We could not find any more recent study that mentions what the reviewer states, namely that " taking minimum slp as a metric for extratropical cyclone intensity was fallacious as it was subject to the confounding influence of large-scale slp changes". In the ERA5 data the horizontal resolution is 0.25°~ 30 km, an order of magnitude higher than in the AR4 simulations. This yields to well identifiable cyclones cores. Furthermore, we base our choice on the recent review of Walker (2020) who clearly states that " The most frequent choice is to use either local minima in MSLP or maxima in vorticity at a single geopotential height or pressure level (in the mid–lower troposphere) to identify an ETC and track that feature through time and space". Regarding the possibility that the SLP patterns are affected by changes in persistence, we outline that , in Faranda et al. (2019 Nature Communications), we have analyzed several sea-level pressure maps issued from a large sample of Reanalyses, CMIP5 historical simulations and future emission scenarios and found substantially no trend in the persistence metric and modest trend in the dimension *d* in the period considered in the present study. We will add to the new version of the manuscript this consideration, that is the basis for our study.

Faranda, D., Alvarez-Castro, M. C., Messori, G., Rodrigues, D., & Yiou, P. (2019). The hammam effect or how a warm ocean enhances large scale atmospheric predictability. Nature communications, 10(1), 1-7.

Walker, E., Mitchell, D., & Seviour, W. (2020). The numerous approaches to tracking extratropical cyclones and the challenges they present. Weather, 75(11), 336-341.

Lines 146-148: This very strong statement about changes in the wintertime North Atlantic storm track, attributed to Chapter 4 of AR6, again seems highly misleading. What the

Executive Summary of Chapter 4 actually says on this subject is this: "Substantial uncertainty and thus low confidence remain in projecting regional changes in Northern Hemisphere jet streams and storm tracks, especially for the North Atlantic basin in winter; this is due to large natural internal variability, the competing effects of projected upper- and lower-tropospheric temperature gradient changes, and new evidence of weaknesses in simulating past variations in North Atlantic atmospheric circulation on seasonal-to-decadal timescales." There is a big difference between what the CMIP models might show, and what there is confidence in.

We agree and will follow the suggestion of the reviewer by rephrasing as: "In the NH boreal winter, CMIP6 models show a northward shift of the ETC density in the North Pacific, a tripolar pattern in the North Atlantic, and a weakening of the Mediterranean storm track. CMIP6 models show overall low agreement on changes in ETC density in the North Atlantic in boreal winter. Nonetheless, substantial uncertainty remains in projecting regional changes in Northern Hemisphere jet streams and storm tracks, especially for the North Atlantic basin in winter (Lee et al., 2021)".

Lines 150-151 say "According to Seneviratne et al. (2021), the number of ECT (sic) associated with strong winds over the North Atlantic and Europe will decrease." But what Chapter 11 of AR6 actually says is this: "There is low confidence in past changes of maximum wind speeds and other measures of dynamical intensity of extratropical cyclones. Future wind speed changes are expected to be small, although poleward shifts in the storm tracks could lead to substantial changes in extreme wind speeds in some regions (medium confidence)." How can this text from Chapter 11 of AR6 possibly be twisted into the highly misleading and very categorical statement made by the authors? The AR6 surely appreciates that the most intense ETCs could potentially strengthen because of more latent heat release, and the current generation of CMIP models are far from being able to give a definitive answer on this.

We once again agree that our phrasing was misleading, but would like to underscore that Seneviratne et al. (2021) state that there is evidence for "fewer [intense wintertime cyclone]in the mid-latitude Atlantic" over the last 60 years and that "Post-AR4 single model studies support the projection of a reduction in extratropical cyclones averaged over the Northern Hemisphere during future warming". We will follow the suggestion of the reviewer by rephrasing as: "According to Seneviratne et al. (2021), "changes in the location of storm tracks could lead to substantial changes in local extreme wind speeds due to extratropical cyclones, although this is accompanied by considerable uncertainty in model projections".

As a result, the statement made on lines 152-153, "Hence, Filomena-like storms would be less probable in a future climate and would be less likely to produce such amounts of snowfall and strong winds" is without foundation and far too absolute.

In view of our above reply, we will replace the sentence by the following: "Hence, we can not assess the anthropogenic contribution to dynamical changes in Filomena-like storms. This could be a reflection of the substantial

uncertainties in the North Atlantic jet streams and storm tracks in winter discussed in Chapter 4 of AR6 (Lee et al., 2021)".

Lines 166-167: The histograms in Figure 2(p) are so ragged that I find it entirely plausible that the changes shown might simply reflect sampling uncertainty. The sample size of 33 analogues does seem very small. How can you convince the reader that you have captured a real difference here?

In view of the revised interpretation we will provide for Filomena and other events (see also our reply to the Reviewer's first general comment), we will replace the sentence by the following: "The seasonal analysis (p) shows that the analogues are distributed across all the seasons. This, coupled with the poor quality of the analogues, does not allow us to draw any conclusions about seasonal shifts."

Lines 274-276 say "the persistence index Θ (o) is higher in the recent period, indicating that recent cut-offs are more likely to stay stationary in Western Europe, leading to longer lasting precipitation events and potentially more intense floods." This is a remarkable and completely unjustified jump from an observed trend in one particular index to an unqualified attribution to climate change. So far as I am aware, there is no consensus whatsoever on how persistent summertime circulation regimes will respond to climate change, let alone the sort of regime that was conducive to the Westphalia floods.

What we mean here is that when we have a cut-off such as the one observed for Westphalia floods, then it is more likely to stay stationary and therefore more likely to produce long lasting precipitation (hence floods). This statement is supported by the change in the Θ distribution that we show and therefore is a result of our analysis. We are not claiming that climate change will produce more stationary precipitation events in general. We will make sure to rephrase the sentence to avoid any misunderstandings. We also note that there are studies supporting specific responses of persistent summertime circulation regimes to climate change. For example, Kornhuber et al. (2021) state that: "models project an increase in weather persistence across the midlatitudes [...], with strongest signals over land-area", and argue that although model projections show a weak agreement one can use the models' performance against observational data to draw more robust conclusions.

Lines 376-377 say "As we have discussed in Section 4.5.1, very intense hurricanes will become more frequent with climate change, and they will be more likely to undergo post-tropical transition." The language has considerably strengthened from that in Section 4.5.1, which was based on literature, and here, where it is unconditional. That increase in level of confidence is unjustified.

We agree and will tone down this sentence as: ""As we have discussed in Section 4.5.1, it is likely that very intense hurricanes will become more frequent with climate change, and they will probably be more likely to undergo post-tropical

transition."

Lines 460-461 say "there is a general consensus that the jet stream will shift northward and therefore cut-off low will become slightly less probable on the Mediterranean sea". No reference is given for this statement, and it seems inconsistent with the AR6 Chapter 4 statement that "Substantial uncertainty and thus low confidence remain in projecting regional changes in Northern Hemisphere jet streams and storm tracks". Any such statement would need to be substantiated, especially for the autumn season in question.

We will revise this statement as we agree that it does not reflect the level of confidence stated in the AR6 report.

Lines 511-514: This seems highly speculative. I found the result for this case study very surprising, and certainly worthy of further discussion if the implication is that certain dynamical situations can prevent the expected warming from anthropogenic climate change. (I have to say that I am finding it difficult to come up with a plausible physical mechanism.)

This is a hypothesis that we make, partly based on the rich literature (and heated discussions) concerning the Warm Arctic - Cold Eurasia pattern and its dynamical drivers. The part that is indeed highly speculative is that the Cold Eurasia part of the pattern may then reflect on the Scandinavian cold spells occurring under easterly advection - something which we find plausible but that we are not aware has been systematically studied in the literature. In the original text, we only grazed this aspect by referring to the well-known study by Cohen and colleagues, but we agree that it would be interesting to extend our reasoning further. In the revised text, we will both phrase more clearly that this is a hypothesis rather than a robust result of our analysis and provide a broader context for this statement. We will anchor the extended discussion in the Warm Arctic - Cold Eurasia literature, including studies that have linked this pattern to the occurrence of extreme cold spells (e.g. Ye and Messori, 2020).

Ye, K., & Messori, G. (2020). Two Leading Modes of Wintertime Atmospheric Circulation Drive the Recent Warm Arctic–Cold Eurasia Temperature Pattern, *J. Clim.*, 33(13), 5565-5587

Typos: you sometimes say ECT when you mean ETC

Thank you for spotting this. We will proof-read with greater care our revised text.