A dynamical adjustment perspective on extreme event attribution
Response to reviewers

Reply to reviewer 2 (T. Woollings):

In the following the reviewer comments appear in black with the author responses in blue.
All the references mentioned in the text are given at the end.

General Comments

This is a nice paper which applies the existing method of dynamical adjustment to investigate some aspects of European climate change and extreme weather. The paper is sound and well written, and I am supportive of acceptance after the points below have been considered. My main comment is that the paper could be improved by the addition of some uncertainty analysis.

I thank the reviewer for his detailed lecture, helpful comments and suggestions, and appreciation of the manuscript.

1. The paper is aimed at the event attribution problem, ie in quantifying the role of climate change in extreme weather events. This is very clear in the abstract and the introduction. However, this specific method instead quantifies the circulation contribution, and any climate change effects are left in the residual. This is still very useful, but it could be made clearer early on that this method alone cannot make statements about the role of any particular external forcing, such as greenhouse gases or aerosols.

Thank you for the comment. I think that the scientific field of extreme event attribution encompasses both approaches, the risk-based and the process-based. As mentioned in the text, the two methods can (and perhaps should always …) be combined to enhance the robustness of the results. The referee rightly points out that the dynamical adjustment method as used in the paper cannot make statements about the respective role of different external forcings. First, I would add that this is also true of many risk-based studies that most of the time do not use single-forcing model experiments. Note also that this inability to make single-forcing attribution statements does not come from the dynamical adjustment but rather from the fact that the paper approach only relies on observations. Indeed, dynamical adjustment could be used on large ensembles of single-forcing simulations such as those presented in Deser et al. 2020 or performed in the framework of DAMIP. The use of large ensembles with all- and single-forcing jointly with dynamical adjustment would allow a model-based quantification of the true total (thermodynamic and dynamic) model response to different forcings as well as more robust statements about any potential forced dynamical response to combined and individual forcings. To this regard, a multimodel community resource similar to the MMLEA (https://www.cesm.ucar.edu/projects/community-projects/MMLEA/) would be extremely helpful. I have added a short discussion in the introduction pointing out the limitation of using only observations and therefore not being able to make single-forcing attribution statements.

2. Given that the main aim of the paper is to quantify the role of atmospheric circulation, it seems the method could be improved by adding an estimate of the uncertainty in this quantification. Even very close analogues in surface pressure will likely have differences in temperature due to large-scale effects not captured by the surface pressure. Hence, to complement the mean effect of circulation as used here, an uncertainty range could be given. More rigorous statements could then be made about the residual terms, in particular as to whether they are within the uncertainty range of the dynamical contribution or not.

Thank you for the comment. I agree with the reviewer that adding an uncertainty analysis for the two extreme events (specifically for Figures 1 and 3) is a very good idea (note that the daily uncertainty analysis is already performed in Figures 2a and 4a. In the case of the Russian heat wave, it suggests that the total residual – as well as the RES_INT term (not shown) – is most of the time much greater than the dynamical uncertainty range).
Therefore, I have implemented a method similar as the one used in O'Reilly et al. 2017. I have then modified Figures 1 and 3 by adding stippling to panels 1d and 3d when the RES_INT term values are within the dynamical uncertainty range.

3. One term is labelled RES_ADV and frequently discussed as representing advection, but this is never tested. Changes in advection will indeed contribute to this term but it is not clear that other processes do not contribute. This seems especially likely in summer, when advection is relatively less important for temperature variability and other factors such as radiation or adiabatic heating anomalies may play an important role (eg Pfahl and Wernli, Quinting and Reeder). Could this term be re-named, or the role of advection tested (eg fig 1f does look consistent with the changing nature of advection from the warming Arctic...).

This term is estimated by running the dynamical adjustment twice, once with TX detrended and once with the raw TX. By construction, it necessarily includes thermal advection changes related to externally forced changes in zonal and meridional TX gradients (as pointed out by the reviewer in Figure 1f). However, I agree with the reviewer that it does also include other contributions such as those mentioned in the above comment. Therefore, I have changed the naming of the two terms RES_FRC and RES_ADV. RES_FRC has been changed to RES_TRD (indeed it is the fraction of the anomaly which is due to the long-term trend). RES_ADV has been changed to RES_FRC. I have modified in the text the definition of the new RES_FRC to make clear that it is not only related to advection as just discussed.

4. Around Figure 1 there is speculation that the RES_INT anomalies reflect the role of regional SST features. This discussion could be informed by the use of uncertainty analysis as in point 2 above. In particular, are these anomalies outside the range of uncertainty in the dynamic term?

I agree, please see the response to comment 2. I have added a few sentences about the significance (or lack of) of the RES_INT term for both events.

5. Has the author tested for trends affecting the estimated dynamical contribution? It seems possible that for a given case the selected analogues may not span the period evenly. Eg, if the analogues happened to sample the most recent decades only, they would then not give a representative sample of the temperatures over the whole period. Hopefully this is unlikely, but could perhaps happen when there is a lack of SLP variance in the early, data-poor, period of the reanalysis?

Thank you for pointing this out. I have checked whether the analogue selection is biased to a specific period or spans evenly the full period.

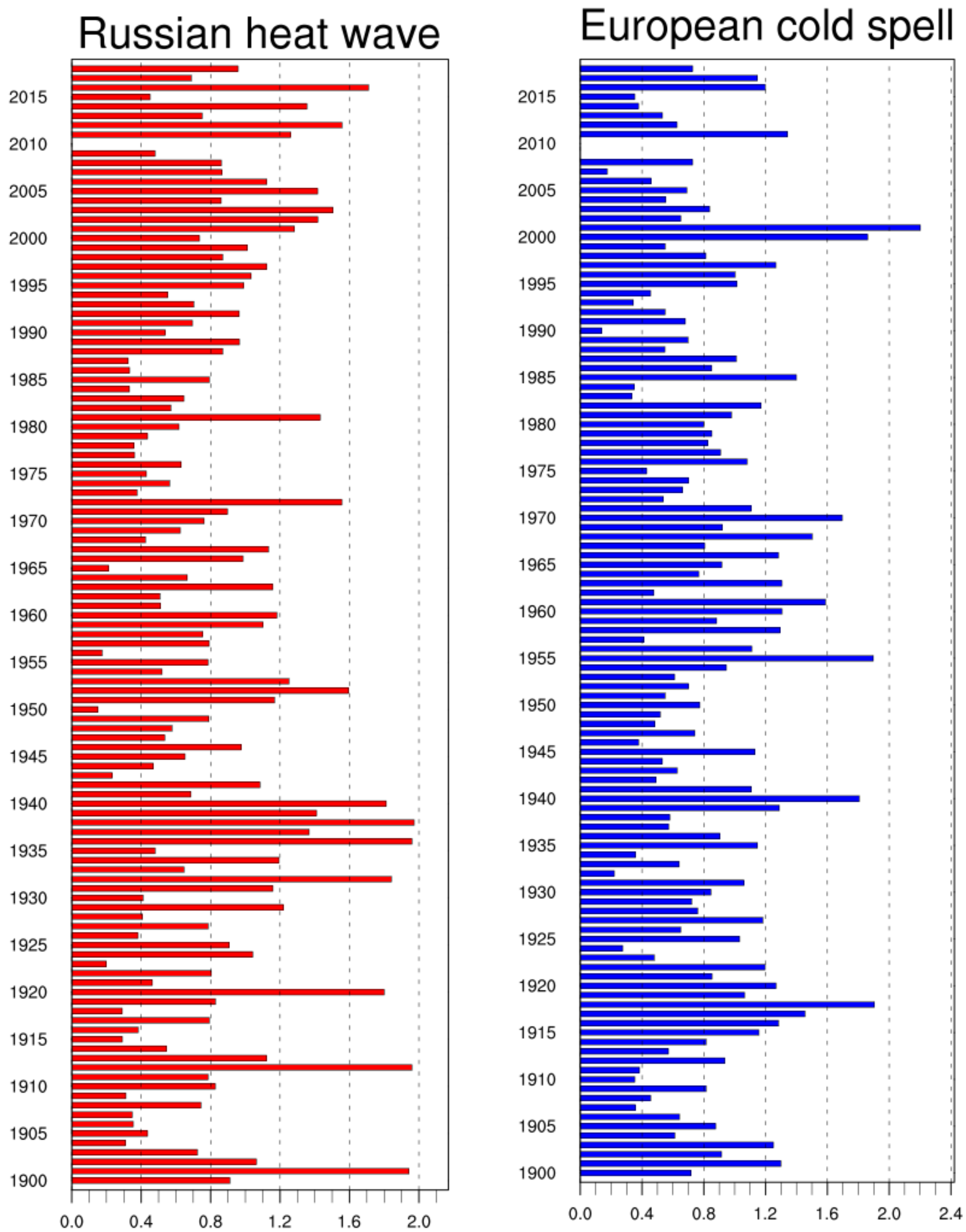

Figure R1: distribution of analogues (X-axis, unit in percent of the total number of used analogues given above) versus their year of occurrence (Y-axis) for the two extreme events

Figure R1 shows the distribution of analogues with respect to the years for the two extreme events (for the entire event, the total number of analogues used is equal to N_r x N_s x N_d, with N_r and N_s defined as in the paper and N_d the number of days of the event). It clearly shows that the selected sample of analogues does not exhibit any particular trend and that specific years with a large number of analogues can be found throughout the entire period.

6. Some of the results in section 4 are interesting and some further discussion might be helpful. eg why is there a 'thermodynamic' cooling trend in fig 5e, and what mechanisms underlie the strong dynamical trends shown here. Could these be consistent with internal variability associated with AMV, as described for example by Sutton and Dong (2012)?

Thank you for the question. Following a similar comment from reviewer 1, I have first investigated whether the cooling trend seen in Figure 5e is robust to observational uncertainty. Therefore, I have used the HadEX3 dataset (gridded values of both TXx and TNx, Dunn et al. 2020) to check the BERK results. Figure R2 below shows the TXx and TNx 1979-2018 trends based on HadEX3 (***note that I have slightly extended the domain eastward in Figure R1 compared with Figure 5 in order to make the point below***). Figure R2 does show a reasonably similar pattern to that of BERK for the TXx trend pattern. However, the HadEX3 pattern is different from the BERK one for TNx, with weak warming in the center and eastern part of the domain shown in Figure 5. A slight cooling is observed east of the region shown in Figure 5. In addition, the HadEX3 TXx and TNx trend patterns both show this cooling east of 60°E in the western Siberia plains (with a larger amplitude for TXx than TNx). This is in good agreement with recent findings about observed mean temperature changes in this region that have resulted from increased winter and spring precipitation in recent decades, delayed snowmelt and increased soil moisture in summer as well as land-atmosphere interaction (see Sato and Nakamura 2019, Bulygina et al. 2009, Bulygina et al. 2011, Guo et al. 2019).

Furthermore, I do not think that this cooling is strongly related to the AMO/AMV. It is a bit difficult to conclude as the *Sutton and Dong (2012)* temperature figure (their figure 2) stops at around 30°E. However, their SLP figure (which goes to 60°E) does not show any significant signal for the western Siberia region. In addition, Figure 6 from *O'Reilly et al. 2017* does show surface air temperature anomaly differences between warm and cold periods of the AMO/AMV index that extends to 60°E. Given the recent time evolution of the AMO/AMV index and assuming that maximum (TX) and minimum (TN) temperature show a pattern similar to mean temperature, this figure provides a nice fingerprint that can be used to interpret both thermodynamical and dynamical TXx and TNx 1979–2018 trends. While it shows that the AMO/AMV significant influence on temperature does not extend east and north of the Black Sea, it also suggests that a fraction of the TXx and TNx trends in the western part of the Figure 5 domain (in particular south of the Black Sea) could be due to a combined dynamic and thermodynamic AMV influence.
Finally, I speculate that the westward-shifted cooling seen in the BERK TNx trend pattern might result from the scarcity of stations in this region for the BERK TN dataset and the use of a large influence ratio in the infilling procedure. I have added a discussion in section 4 about the TNx trend results and observational uncertainty, alluding to the HadEX3 figure that has been added

to the appendix. I have also added some discussion of the possible role of the AMO/AMV influence on both TXx and TNx 1979–2018 trends.

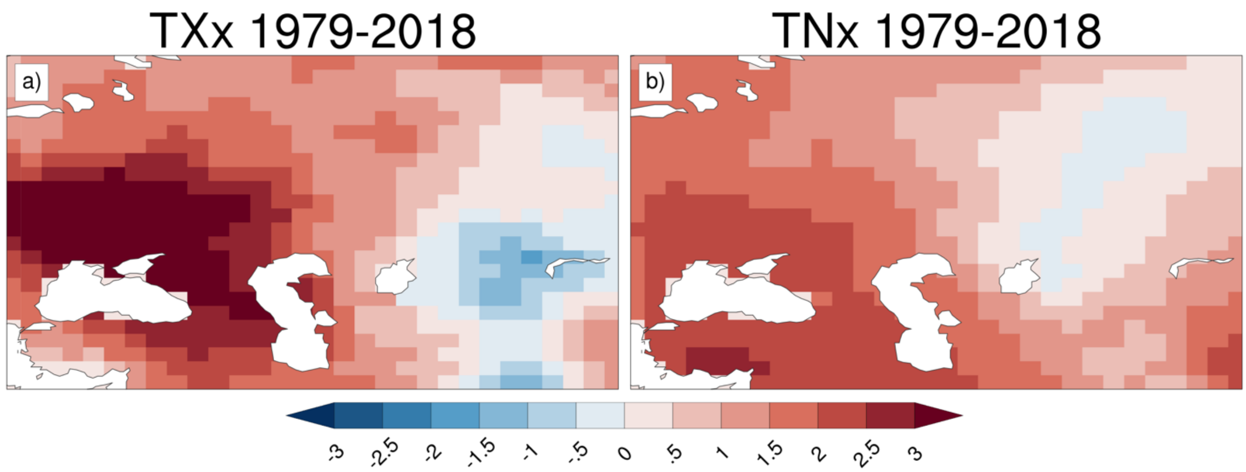

Figure R2: TXx and TNx 1979–2018 trend patterns derived from the HadEX3 dataset

7. On the Russian heatwave: the text suggests this has been 'mainly linked' to La Nina. I agree this is very likely a factor (see also Drouard and Woollings 2018, GRL), but even so this statement feels a bit strong.

Agreed, I have removed "mainly" and added the suggested reference.

References:

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