

Weather Clim. Dynam. Discuss., referee comment RC2
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Comment on wcd-2021-40

Tim Woollings (Referee)

Referee comment on "A dynamical adjustment perspective on extreme event attribution"
by Laurent Terray, Weather Clim. Dynam. Discuss.,
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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.

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.

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.

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...).

<https://doi.org/10.1029/2012GL052261>
<https://doi.org/10.1175/MWR-D-17-0165.1>

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?

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?

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)?

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.