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Interactive comment on "A Methodology for Attributing the Role of Climate Change in Extreme Events: A Global Spectrally Nudged Storyline" *by* Linda van Garderen et al.

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This paper applies the "storyline" approach to the analysis of the 2003 European and 2010 Russian heatwaves. The storyline approach to the analysis of extreme events is typically performed by constraining the circulation features in the factual and counter-factual representations of such events to be similar to features that were observed at the time of the event. In this case, the approach is to constrain the circulation within a global climate model to be similar to analysed circulation from a reanalysis via a spectral nudging technique.

General comments:

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Discussion paper

Even tighter constraints could, presumably, be obtained if a similar analysis were performed with a forecasting system that assimilated all observed data on the one hand (the factual case), and the same observed data except with “bogussed” temperatures on the other hand (the counterfactual case). While clearly not feasible (or expected) for this study, a similar study with a forecasting system might provide some additional useful insights into the application of storyline methods since the data that are presented to the model in the counterfactual case would then have to satisfy the thermal and dynamical balance constraints that would be imposed by the assimilation system. While this might make the counterfactual more difficult to implement, the use of an ensemble analysis and forecasting system would, in particular, provide some interesting possibilities for the quantification of uncertainties. Such an approach would also provide a “seamless” connection to probabilistic event attribution approaches (see next comment) that could draw on probabilistic weather forecasting techniques. Some discussion along these lines might be merited.

The introduction and the concluding discussion both try to make the case that the storyline approach is distinct from the probabilistic event attribution approach. I think, however, that the distinction is actually not very sharp. Rather, this is a question of conditional distributions and the degree of conditioning. The Stott et al., 2004, paper that started all of this off estimated distributions conditional on external forcing only (i.e., using a free running coupled model). Many subsequent papers estimated distributions conditional on external forcing and the pattern of sea-surface temperature anomalies that prevailed at the time of the event, largely because this enabled the production of very large ensembles of simulations with atmosphere-only models. In the storyline approach, conditioning is on external forcing, SST anomaly patterns, and circulation. In the case of this paper, a large-scale circulation constraint is applied globally via a spectral nudging approach. Even with this additional third constraint, the authors still, ultimately, end up trying to interpret the outcome in the context of uncertainty (e.g., by referencing estimates of climatological quantiles). Thus, even though they do not specifically estimate the factual and counterfactual distributions – interpretation becomes a statistical exercise. The fact that these distributions are not estimated reflects, I think, only a computational limitation (using an ensemble forecasting system in a parallel approach to the one taken in this paper would produce distributions that are conditional on the observed circulation). So, in my mind, this is not a matter of probabilistic vs non-probabilistic (or in medicine, epidemiological versus pathological) approaches to the interpretation of evidence, but rather simply a question of the degree of conditioning.

Some additional specific comments:

8: "are associated" –> "are often associated"

20-21: I suggest deleting this last sentence of the abstract. It isn't obvious how it follows from the preceding sentence, and also, there doesn't seem to be anything in the paper that discusses or explores this kind of application of the storyline methodology that is proposed.

43-47: I'm not sure that this view is as common as stated. I think what is understood is that large-scale internal variability is a feature of the dynamics (thermal and non-thermal) of the coupled Earth System, and that the dynamical changes tend not to be secular in the way that thermal changes are secular under external forcing (although there are a few exceptions – e.g., projections that storm tracks will shift a few degrees poleward, and the Southern Annular Mode response to stratospheric ozone forcing). Further, changes in vertical velocity are really hard to separate from purely thermal changes (despite some formalisms such as that of Bony et al., 2013) because of the feedbacks from latent heat release that are associated with a change in vertical motion.

77-78: I think it would be appropriate to mention Scinocca et al., 2016 (doi: 10.1175/JCLI-D-15-0161.1), who I think implemented a spectral nudging approach not dissimilar from the method used in this paper.

93: In this study the model is nudged towards reanalysis data, but in general, it could be nudged to other types of data as well. For example, one might want to "dynamically downscale" a transient global climate change simulation with a much higher resolution global atmospheric model, nudging some aspects of the circulation of the high-resolution atmospheric model to that of the driving earth system model.

106-109: Notwithstanding the fact that there is probably not a lot of sensitivity to the choice of driving data (circulation is understood to be well-constrained by observations in reanalyses) it would still be useful to include some discussion of how the choice of driving data was made. Later, the paper makes some comparison between the nudged ECHAM6 output and ERA-Interim, so an immediate question might be, why not also use ERA-Interim (or perhaps better yet, ERA-5) as driving data. To the extent that ECHAM6 and ECMWF models still share common physics, there might also be an argument for using an ECMWF reanalysis product for driving ECHAM6 from a commonality of physics perspective.

160-162: I've always found the choice of counterfactual climate that is typically used in event attribution studies to be a bit unsatisfying. In effect, we need to trust that we can reliably adjust boundary conditions (such as SSTs) and reliably simulate a climate for which we have only very few observations. This choice allows a larger potential signal-to-noise ratio since it encompasses a relatively large amount of warming, but to the extent that it is important to have confidence that the counterfactual is well simulated, it might be preferable to use a period in the modern instrumental era when forcing was not as large.

171-172: I think it would be useful to say something about how well the large-scale circulation is constrained by the available observations. You've used NCEP1, but one could, for example, use an ensemble product such as the 20th century reanalysis (https://www.psl.noaa.gov/data/20thC_Rean/) to obtain an estimate of the strength of the observational constraint, at least in that product. The spread between ensemble members will be small for variables, periods and regions where the available observations provide effective constraints.

176: Formally at least, the quantity in brackets should also be a function of t rather than simply being fixed to a single number at each location (if nothing else, perhaps there is some seasonal variation in the pattern that would be relevant for the kind of short-term simulations used in this paper).

221: I would have thought that the IPCC AR5 Working Group I report would have been the best reference to cite to support a statement about how much warming has taken place.

235: "the hottest in the past. . ." –> "the hottest summer in the past . . ."

236-238: As an aside, while these impacts, and those of the Russian heat wave described later, are large, they pale in comparison with the impacts that we are currently experiencing in the global pandemic.

241: "blocking" –> "block"

242: "by the atmospheric blocking" –> "by atmospheric blocking"

248: I think it is imperative to cite Stott et al., 2004, in this context as well.

254-264: It would be useful to compare the frequency of exceedance above the 95th percentile with what would be expected climatologically. We would expect exceedance to occur, on average, on 5% of days (that is, 4.5 days per season). Because of serial dependence, however, the expected interannual variability about that 4.5 day per season number is a bit difficult to calculate. Nevertheless, the counterfactual exceedance frequency would appear to be consistent with, or perhaps less than, the climatologically expected 4.5 days, whereas factual exceedance is clearly much higher than the expected frequency.

254-264 (Figure 5): Please include a curve for observed temperatures as well as the various simulated temperatures.

381: I think it would actually be useful to say a bit more about the noise level (there isn't a lot on this aspect in the paper). In particular, the “noise level” reflects the variance of the temperature distribution after conditioning on the large scale circulation in the particular way that the conditioning has been done (the statistical interpretation is, ultimately, unavoidable, I think). If you change the constraint – for example, by changing aspects of the nudging strategy – then that “noise level” (aka, conditional variance) will change. I think readers should be made aware of those links and the impact that the study design choices could ultimately have on the attribution results that are obtained.

I hope you find these comments useful, and that everyone remains well in these unusual times.

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