Nonlinear Processes in Geophysics Discussions

Nonlin. Processes Geophys. Discuss., author comment AC3
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Reply on RC4

Clara Deser and Adam S. Phillips

Author comment on "A range of outcomes: the combined effects of internal variability and anthropogenic forcing on regional climate trends over Europe" by Clara Deser and Adam S. Phillips, Nonlin. Processes Geophys. Discuss., https://doi.org/10.5194/npg-2022-15-AC3, 2022

The original comments are included in plain black text, and our response is given in *italicized text* beneath each one.

RC4 Referee #2
REVIEW FOR "The Role of Internal Variability in Regional Climate Change", by Clara Deser and Adam S. Phillips, submitted to Nonlinear Processes in Geophysics

Summary:

The authors analyse historical and future European temperature and precipitation trends in the CESM2 Large Ensemble, observations, and an "Observational Large Ensemble". The analysis is based on an analogue-based dynamical adjustment method, which is used to disentangle dynamical (based on SLP analogues) and thermodynamical (residual) trends in the climate model and observational ensemble. As a result, the authors show that internal climate variability is a crucial source of uncertainty in future European climate, the level of which is broadly comparable in magnitude between the Obs-LE and CESM2-LE, and that the thermodynamical component of observations agrees well with the forced CESM2-LE component for temperature, and less well for precipitation.
Overall, the paper provides a very useful illustration of internal variability in present and future European climate, and a constructive discussion of current and outstanding issues in dynamical adjustment. The paper is also very well written and logically structured. I still have a few concerns that are outlined below, however, and I would therefore recommend moderate revisions.

Thank you for your favorable assessment and constructive comments and suggestions.

Major issues:
(1) Abstract

The Abstract is well-written, however it is somewhat disconnected from the actual analysis conducted in the paper. At present the Abstract reads a bit like that from a Perspective paper, while the (by far) largest part of the paper presents actually a specific analysis of European climate. Hence, I would recommend to adjust the Abstract such that it (also) reflects the analysis conducted in the paper.

Agreed! We shall adjust the Abstract to better reflect the content of the paper as suggested.

(2) Implications of high climate sensitivity in CESM2 for interpretation of thermodynamical trends

The authors interpret "the good agreement between the observed thermodynamic-residual trend component and the model's forced thermodynamic trend" (l. 604) as "further underscoring the realism of CESM2" (l. 605), and that "the model's forced temperature trend is realistic" as a powerful conclusion (l. 521). This conclusion is based on the temperature dynamical adjustment discussed on p. 30, where the authors argue that "observed thermodynamic trend is much closer in amplitude (and arguably pattern) to the model's forced response".

While I agree that these results are in general really encouraging, I do think that some caution is warranted: CESM2 is known for high climate sensitivity, so (I believe) we *should* expect some discrepancy in the amplitude of the pattern, and -contrariwise- a higher similarity in the pattern itself. Hence, why is the observed thermodynamical pattern's amplitude over Europe so high as to even match that of a high climate sensitivity model?

Although CESM2 is a "high climate sensitivity" model, its global mean temperature rise over the historical period (at least since 1920 or 1950) is quite similar to observations as shown in the attached plot (produced by the Climate Variability Diagnostics Package for Large Ensembles https://www.cesm.ucar.edu/projects/cvdp-le). In this plot, the gray line is HadCRUTv5 and the blue line is the CESM2 LE ensemble mean; the light blue (dark blue) shading is the 25th-75th (5th-95th) percentile range across the LE ensemble members. This graph shows that the "high climate sensitivity" of CESM2 LE is not a feature of the historical period that we analyse (1972-2021). We note that the moniker "high climate sensitivity" refers specifically to the Equilibrium Climate Sensitivity (ECS) of the model as diagnosed by means of CESM2 simulations in a slab-ocean configuration in response to an instantaneous doubling of CO2 (Gettelman et al. 2019). Evidently, a high ECS does not necessarily translate to a high transient climate sensitivity over the historical record.

Gettelman, A., Hannay, C., Bacmeister, J. T., Neale, R. B., Pendergrass, A. G., Danabasoglu, G., et al. (2019). High climate sensitivity in the Community Earth System Model Version 2 (CESM2). Geophys. Res. Lett., 46, 8329– 8337. https://doi.org/10.1029/2019GL083978

Moreover, for precipitation more careful conclusions would be warranted, as the residual component does not closely resemble the model's forced response. For example, the authors attribute the (large) pattern disagreement in Central Europe to "lower signal-to-noise" found in this region compared to other areas (l. 544-546), and further pattern disagreement over large areas in South Europe, such as the Balkans, Turkey, and Italy is only briefly mentioned. Here, I believe it would benefit the discussions if the authors would discuss this a bit more in-depth, and explain where the "lower signal-to-noise" explanation in this region comes from (because this is a transitional region between southern drying and northern wettening?).

We agree that there is also disagreement in areas in South Europe. We shall add some more discussion about the sources of disagreement between the observed dynamically-adjusted precipitation trends and the model's forced precipitation trend, chief among them is the fact that this is a transitional region between forced southern drying and northern wettening (and hence low "signal") as you have surmised.

(3) The authors assume that the forced CESM2 trend (i.e., ensemble average) reflects the thermodynamical response to climate change. This is consistent with literature, but reflects some simplification, which the authors acknowledge in their discussion. But, the authors also say that "future trends in SLP also contain a modest forced component indicative of enhanced westerlies over the continent" (l. 479), and I believe there may be in addition nuanced forced dynamical components with only a modest SLP signature.

In earlier literature (Deser et al. 2016), the authors actually use their dynamical adjustment method to show in their Fig. 7 that the average across the dynamical contribution is rather small. I believe it may possibly benefit the present paper and argument to include and discuss a similar figure for Europe?

Good idea. We will use our dynamical adjustment method to quantify the contribution of the forced dynamical component to forced precipitation trends and include the results in the revised manuscript.

Minor issues:

l. 150. McKinnon et al. 2017 is missing in the references section

Thank you; we will correct this.

l. 631. The connection to predictability studies and the "signal-to-noise paradox" is interesting, but the short discussion is hard to follow. Maybe the implications could be made a bit more explicit here.

Thank you; we will expand our discussion of the implications of the "signal-to-noise paradox" in the revised manuscript.

References:

Deser, C., Terray, L. and Phillips, A.S., 2016. Forced and internal components of winter air temperature trends over North America during the past 50 years: Mechanisms and implications. Journal of Climate, 29(6), pp.2237-2258.

Please also note the supplement to this comment:
https://npg.copernicus.org/preprints/npg-2022-15/npg-2022-15-AC3-supplement.pdf