

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-719

Anonymous Referee #2

Referee comment on "Long Range Prediction and the Stratosphere" by Adam A. Scaife et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-719-RC2>, 2021

(FYI, the following was written before reading the other reviewer's comments. You will find some similarity between the concerns raised by both of us; I'd like to echo in particular the comments around "prediction" vs. "projection".)

A review of recent developments in our understanding of the role of the stratosphere in surface weather and climate variability is presented, focusing on those aspects that provide improvements in long range prediction. A prime predictor arising from stratospheric variability in mid-latitudes appears to be the polar vortex strength, which itself is influenced by other teleconnection patterns (e.g., QBO, ENSO, MJO). Time scales considered are monthly, seasonal, annual, decadal, multi-decadal, which all exhibit co-variability patterns between the stratospheric polar vortex and the tropospheric jet.

Overall, this review reads well and the selection of included phenomena and relationships looks appropriate to me. It's great to see a combined effort from an extensive core list of researchers in the field.

That the stratosphere matters at the time and spatial scales considered in this review is, as far as I can tell, meanwhile well established. From that perspective the review seems overly strong about trying to convince the reader that the stratosphere does indeed matter. Arguing too much from the perspective that someone still needs to be convinced runs the risk of overstating the role of the stratosphere and overselling the point.

On the other hand this raises the question what the review is actually trying to achieve? It certainly represents a nice and comprehensive collection of those phenomena relevant for long range prediction that are influenced by the stratosphere. But in parts these aspects can already be found in previous reviews (e.g., Gerber et al., who review the importance of including a well-resolved stratosphere in weather and climate models; Kidston et al., who point out the similarity of S-T coupling on a range of time scales; Butler et al. (2019

book chapter) about the role of the stratosphere in sub-seasonal prediction). To me this calls for a better justification for the present review.

I do think the material and expertise/experience on the author team offers a unique and welcome opportunity to synthesize our knowledge gained over the past ~20+ years in a way that offers new insights. For example, what can be said about the relative importance of the stratosphere compared to other sources of long range predictability? Does the stratosphere primarily act as an "integrator" of other sources of long range predictability (e.g., mid-lat weather regimes, MJO, ENSO, QBO) or does it have a more fundamental impact on predictability on its own (the latter perhaps more relevant for solar influences)? One way to synthesize our knowledge would be to create novel and meaningful schematics, e.g., to highlight the similarity across time scales and/or the interconnectedness of different climate system components. As it stands the review only includes 2 Figures: one on a recent SSW event (a case study) and one taken from another review article -- this seems a bit meager for an effort as this one.

Another fundamental aspect is the distinction of limits of predictability due to sensitivity to initial conditions (e.g., on sub-seasonal time scales for weather or annual-to-decadal time scales for climate) vs. limits of long-term climate projections due to interactions across different components of the climate system (a very different animal). The presented material seems a bit superficial when it comes to this distinction and I'd strongly encourage the authors to revisit all related statements throughout the paper.

A few specific comments by line number:

line 36: "parallel advances" may hide the fact that the listed advances happened, at least in part, because of interactions across the involved communities

line 45: whether the term "climate system" encompasses daily weather fluctuations is debatable; I'd suggest to avoid confusion here and start the sentence with "Daily weather fluctuations are thought to have ..."

line 55: the way it's written it may sound as if SSWs are predictable; what is likely meant here is that the state of the stratosphere is (somewhat) predictable following a SSW -- please clarify

line 114: suggest to change "affect" to "include"

line 147ff: there's a more direct QBO-polar vortex connection, so it seems strange that the teleconnection via the MJO, which is much more indirect, gets mentioned first

line 157: "Other mechanisms" may sound confusing, because the preceding paragraphs were focused on tropospheric wave activity providing a source for stratospheric variability, whereas here you focus on the mechanisms around downward coupling

Fig. 1: did you average over the 3 initialization dates; how did you compute the anomalies in panel b? please provide more detail about how these panels were produced

Fig. 2: it would help to modify this schematic in such a way that the role of the stratosphere in the individual components shown stands out more clearly

line 397: is "prediction" in this context still appropriate? or rather "projection"?

line 505: "are responsible" sounds too strong to me -- something like "contribute to" seems more appropriate (the climate extremes can in principle happen with or without stratospheric influence)

line 518: "but occurred again" -- unclear what this refers to?

line 528: ok, but perhaps important to mention that these nonlinear, state dependent impacts may be present for all biases in general, not just those involving the stratosphere

line 546: I agree that more research on the role of the stratosphere in the signal-to-noise paradox would be very useful; I think it'd be great for the general readership if the authors could expand on this idea a bit more