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Comment on acp-2021-719

Anonymous Referee #1

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-RC1, 2021

Review regarding „Long Range Prediction and the Stratosphere" by Scaife et al.

A nicely written opinion piece that merits publication after some revision.

After reading the paper, I was left slightly wondering: Is the paper trying to do too much or too little? It reads like a very nice essay, but somehow, I was wondering if the framing was too narrow or too wide. Reading the paper further, I found it hard to grasp how the authors suggest to handle the seamless nature of weather and climate modelling in the future (is there a recommendation?), and the transition from initial value dominated problems to boundary value dominated problems could and should be clearer. Of course, the main point of the paper is to discuss the usefulness of including the stratosphere into (atmospheric) predictions of weather and climate. However, is there still anybody left who doubts the usefulness of such an approach? Technically, when assimilating (satellite) data with wide vertical weighting functions the usefulness seems obvious (presumably this should be mentioned more strongly) – otherwise no good initial state for any kind of prediction could be generated. From fluid dynamical understanding, when e.g. thinking about wave propagation in the atmosphere, the case seems settled as well and many good and valid examples are given in the paper. In terms of coupling between composition, thermal structure and circulation, evidence exist on different scales as well, e.g. for volcanic eruptions (tropical presumably more than extra-tropical) or the role of the ozone hole for the seasonal evolution of surface temperature in Antarctica (presumably this could be addressed clearer in the paper). In particular the role of the land surface (and its changes – including the hydrology) in providing – on some time horizon – an added benefit for prediction seems missing. How this is link to the stratosphere is presumably less clear-cut than the role of the ocean (I understand this), however I found this a strange omission that should be addressed (at the moment it is sort of mentioned as a caveat – maybe it could be mentioned more as a research need). The paper follows the philosophy by structuring the content by time horizon – however, given the review nature of the paper the time horizons should be clearly motivated in the introduction and a small sketch that illustrates the transition from the initial value to the boundary value regime should be included. I know that such figures exist. However, I believe it would be useful to start with a clear map to motivate the structure of the paper and to provide a caveat for decadal to multidecadal prediction. I have to admit that I personally have a problem with the use of the word "prediction" on such a long timescale, because the prediction will strongly depend on the chosen scenario. I would actually prefer the use of the term "projection" (over prediction) to make this more obvious to the uninitiated reader, that most of the “answer” (projected state at the end of the integration) might be actually in the scenario.

The stratosphere and monthly prediction

I am surprised that so much space is given to the MJO – I would have assumed that the ENSO state would be even more fundamental (even on this timescale) – also regarding its connection to the MJO occurrence. Presumably ENSO – similar to the QBO – can be seen in some cases as something that provides a certain persistency to the system. If the initial state is correctly captured in the analysis that is used at the start of an integration the resulting prediction should benefit from the “accuracy” of the initial state. Here, I would have expected more emphasise on the initial state (ocean, land, QBO, BDC, …) and benefits that result from kicking-off the “forecast model” in the correct way.

The stratosphere and seasonal prediction

Of course, the monthly to seasonal scales are fluent (or seamless). Presumably this is now the range were initial conditions become less important and some boundary conditions count in more. Thus, I am surprised that the QBO is featured in this context stronger (my subjective feeling, line 241) as before. I would have assumed that transition timescales of the QBO are well within the scale range considered here, and that most models still perform poorly for the phase transition, even those models having some persistence when started with the right initial conditions. Given the ENSO link discussed here, I was wondering if the authors would like to comment on the question to what extent one can assume certain sources of predictability as independent. ENSO and MJO seem to be so closely linked (in some aspects) that they might more reflect the seamless nature of the problem than independent added benefits of predictability. To some extent lines 309-312 pick-up this point (stratosphere as source or conduit of predictability) – however at the same time I find the closure of the section confusing. What precisely are the first principles? Even the best models are built “only” on discretised versions of a certain set of coupled partial differential equations (close to first principles). However, the resulting model is an approximation that allows idealised model integrations that can be valid as sensitivity studies. Thus, often consistency and not causality can be tested – and I assume this is true for a forecast problem as well. Thus, I would phrase this sentence more carefully.

The stratosphere and annual to decadal prediction

Figure 2 seems to be more misleading than helpful – I appreciate that this is an adaption of an already existing figure. However, the classification of boundary conditions and initial state seems not very clear to me (in particular thinking through the role of the carbon cycle – including how composition responds to emission changes). Even though halogen loading might be considered as a boundary condition resulting from prior emissions, certainly knock on effects, including ozone, are not as clear cut (affected by other emissions and sometimes - also at lower chlorine loading than now - subject to sometimes “unusual” variability). In particular the climate (and carbon uptake in the SH) will change with ozone recovery, which is something that will happen in the near future on a decadal timescale. Thus, it would be nice to have a more critical reflection on the figure or an alternative approach (preferred).

The stratosphere and multidecadal prediction

As already mentioned, I feel a little uneasy that in a projection context (scenario dependent) the word prediction is used. I understand that a prediction is certainly something that can depend on assumptions (e.g. scenarios). However, I have the feeling that many people judge uncertainties different, depending on a prediction or projection framing. That said, this could be rectified by a much clearer statement regarding the transition towards a boundary value problem that is dictated (to an important extent) by the chosen scenario in the introduction. In line 470 I find the "doubling strength of a teleconnection" a weird concept, please explain precisely what is meant. In line 472-474 I am slightly lost: Why is there just talk of strengthening teleconnections – is it no longer not an open point if the explained variance (e.g. of a certain EOF) could change? For example, to stay in the EOF picture: Will the order of EOFs change?

Outlook

I am surprised by the interpretation summarised in lines 489 – 497. I am not doubting the general assumption – that the initial state of the stratosphere is important on certain timescales – however, also following the discussion above regarding ENSO and MJO more detail seems to be necessary, to explain the special situation and time horizon for which this statement is true. I guess it would be charming to cite in line 500 really some ground-breaking early studies – and not just the meta-citation that follows. Lines 525-528 presumably requires a clear distinction between mean biases and teleconnection errors. The former seems to be used in the meaning of a classic bias (systematic deviation from a reference), the latter is presumably linked to a change in variance or order of the EOFs (or shape/phase/shift of the probability density functions, PDFs). Presumably both biases / changes are not independent – however, they are not the same either. Line 550-552 seem to be out of context – I am not getting the point. Presumably it would be interesting to finish with a small recommendation what kind of stratospheric representation should be achieved in the future. How good should a QBO be? And how about the ability to reproduce the PDFs of warmings, etc. … Where do we want to be in a decade?

In summary – the seamless nature of going from initial value dominated prediction to boundary value dominated (prediction and) projection should be far clearer. Even though I understand in the logic of the paper that the last prediction chapter is called "multidecadal prediction" and needs to be far clearer that we really talk about projections that heavily reflect the chosen scenario. I do not doubt that even in projections the stratosphere has a crucial role – in particular for the "quality" of the teleconnections and their changes under climate change (given that the stratosphere cools when the troposphere heats up). Thus, I find lines 472-474 awkward and not well put into context. Here, also the role of composition beyond GHGs is certainly important (e.g. the recovery of the ozone hole in the southern hemisphere or changes in Sahara dust outbreaks in the northern hemisphere). Overall, the paper is a nice summary rationalising the importance of the stratosphere for weather and climate modelling (predictions and projections) that could do with some additional tuning before final publication following the comments above.