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Comment on acp-2022-378

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Referee comment on "Driving mechanisms for the El Niño–Southern Oscillation impact on stratospheric ozone" by Samuel Benito-Barca et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-378-RC2, 2022

This is a nice and interesting paper – I do not have any mayor criticism. However, I would like to share a more fundamental thought regarding the analysis method and the assumptions made and wonder if the authors could reflect on them in the discussion and or summary.

Line 38 onwards discusses the somehow contradictory nature of results to how the stratosphere behaves during El Nino's of different "flavours". However, it is very hard for the reader to understand if the classification of El Nino's is always done the same way and if composites rely on the same metrics. Personally, I like to think about El Nino's as something quite dynamic – whereas the subconscious assumption in many studies seems to imply that El Nino can be easily described in classes that lend themselves to "quasi-stationary composites". However, I am not surprised that different assumptions regarding the construction of such composites may lead to slightly different results (as is mentioned in line 49).

The choice of NDJF as a season is logical, if one subscribes to a quite stationary picture of the "ENSO" classes. Here, I would find it useful to reflect a little more on the choice of this very long (and slightly artificial) season. True, averaging might make a problem more linear, but could I get away with less averaging? In other words: Is there a dominant feature in the NDJF seasons that determines the long term mean? The question seems an interesting one to me, because you are presenting time-dependent composites (from October to May) that show clear sign changes in NDJF (or, I am not getting the colour scale right). Here I would really like to see some explanation how the extended season and the time-dependent composites relate to each other.

When talking about statistical significance it would be helpful to state what kind of variability is forming the baseline – I am not always 100% percent sure if you base all assessments on the full ensemble information (or on individual realisations) – or just the case-by-case standard variability of the different ensemble members. Please clarify where appropriate.

In addition, some small thoughts regarding the ozone impact and a more continuous approach to the (stratospheric) ENSO impact. Because the thoughts are documented by some "old" papers I (co-)wrote, I have submitted the review under my name:

1) A strong anticorrelation between vortex strength and (total)ozone is somehow an intrinsic feature (regardless of ENSO state, e.g. figure 1 in doi:10.1029/2002GL015973). However, ENSO can certainly impact part of the signal when either the vortex strength and/or the equatorward transport of "ozone rich air" is modified. Even though such relationship can be established with a simple stratospheric wind metric, the underlying contribution for the ozone change is largely coming from a region that is suggestive of the shallow branch of the BDC (e.g. figure 3 (right panel in doi:10.1029/2002GL015973)). This illustrates the importance of monthly extremes as dominant features in an extended season. Thus, my point above regarding the extended season – is there a dominant effect / month in the extended season?

2) To avoid a strong quasi-stationary assumption a more continuous approach to diagnosing ENSO impacts could be useful that could involve lagged correlations. For example, Figure 4 in doi:10.1039/b417947c illustrates such a simple approach, using lagged correlations between the NINO3 index and three different latitudinal regions. It is presumably not surprising that the correlation magnitude is largest in the tropics, and that the relative extrema in the mid-latitudes lag by a couple of months. However, this behaviour implies to me that maybe composites might be a little flawed. However, I do admit that the mentioned example also requires more work to provide conclusive evidence of how a more continuous approach might help to unravel different ENSO classes.

Just to be clear: I am not expecting the papers to be cited – I would just like to trigger some (more) critical reflection regarding the composite method – either in the introduction or the summary. Otherwise, I am very happy to accept the findings of the study as they are (it all comes back to the "contradictory" statement in line 39 – in conjunction with a very extended season and "case" composites that show some significant changes in the extended season).

Because this review is more a request for additional information / clarification I abstain from minor comments and hope that the authors will deliver a small update to this nice paper in due course.