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Comment on acp-2021-647

Anonymous Referee #2

Referee comment on "Enhanced upward motion through the troposphere over the tropical western Pacific and its implications for the transport of trace gases from the troposphere to the stratosphere" by Kai Qie et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-647-RC2, 2021

The manuscript presents an analysis of atmospheric upward transport through the upper troposphere and lower stratosphere over the tropical West Pacific based on reanalysis data and model observations. Long-term changes in the upwelling are linked to increasing global sea surface temperatures leading to a strengthening of the Pacific Walker circulation and deep convection. Implications for stratospheric entrainment of CO and H2O are discussed.

The research question addressed here is an important one and the topic is of general interest to the readers of ACP. Some parts of the analysis are solid and provide valuable insights into long-term changes of the underlying processes. However, I have some major concerns (listed below) and recommend major revisions before the manuscript can be published.

Major comments

1) Caution is advised when using reanalysis data for trend detection as the quality and character of reanalyses may have changed over time and non-physical trends can result from changes in the observing system or execution stream. This has been demonstrated for many atmospheric quantities such as stratospheric temperature (Long et al., 2017, ACP) and residual circulation velocities (Chapter 5, S-RIP report, 2021).

Here, the trends derived from reanalysis are presented without any discussion of these aspects, but instead are used as if they would be reliable sources of long-term changes. A discussion of the limitations of reanalysis data for trend studies and words of caution are needed and the text should be changed accordingly throughout the manuscript, in particular when using reanalysis before 1979.

2) Trends of the vertical wind derived from the three reanalysis data sets agree in some regions but disagree in others as seen from Figure 2. A discussion of the level of agreement is needed. At the same time, it is not clear which region exactly is referred to as the tropical western Pacific (TWP). In many cases the authors would us the TWP in cases when the text and figures suggest that they refer to the Maritime Continent (e.g., ERA5 shows increasing trend of w over the Maritime Continent but decreasing trends over larger parts of the TWP). It would be very helpful, if the authors would define the regions upfront and use them consistently throughout the manuscript.

3) It seems that the upwelling trends (averaged over the region of interest) are hardly significant even at the 90% confidence level. The uncertainty ranges and trend values need to be provided in the text or figure. Furthermore, it is not clear why the averaging is done over 20S-10N. Looking at Figure 2, my impression is the averaging over 20S-20N will not result in trends significant at the 90% confidence level. If this is the case, it should be stated in the text.

4) Where is the cold point temperature trend coming from (Figure 4)? This data source is not listed in the text or caption. Given that it starts at 1958, most likely the trend is derived from JRA55. Again, some words of caution are needed, given that cold point temperature trends from reanalysis data sets can show significant differences even for the satellite period (Tegtmeier et al., 2020, ACP).

5) The discussion of the trends of stratospheric upwelling needs to refer to Chapter 5 of the SPARC S-RIP report. Chapter 5 states in its abstract: 'However, estimates of long-term trends in tropical upwelling are inconsistent among different products, showing either strengthening, weakening, or no trend.' Therefore, results shown in Figure 11 based on JRA55 are most likely not consistent with other reanalyses.

6) I don't agree with the interpretation the CO changes based on various model runs as presented in Figure 9. Both simulations have the same sources and the control run shows enhanced convective uplifting brining more CO to higher altitudes. For the tropical West Pacific, the trends are larger for the Control run throughout the whole vertical extent of the troposphere. However, enhanced upwelling would result in a less strong trend at the surface and boundary layer, opposite to what the simulations indicate here. In fact, some recent studies showed that over the Indian Ocean, CO abundance in the boundary layer decreases (despite the growing sources) while it increases in the mid to upper troposphere due to enhanced convective activity (e.g., Girach and Nair, 2014). The discussions and conclusions regarding this figure need to be revised.

Minor comments

Should the title say '… implications for …'?

For the fact that halogenated gases are enhanced over the WP, a citation is needed. The citations given at the end refer to tropospheric halogen chemistry. What is meant with the second part of the sentence? A general statement, that halogens impact stratospheric ozone chemistry? Or that halogens injected over the West Pacific have a relatively large impact on stratospheric ozone chemistry?

Line 190: What is an intensifying trend? A trend increasing over time?

Line 272: figure 2f shows wind fields at 500 hPa. Do you mean a different figure here?

Line 270-274: This line of argumentation doesn't make any sense to me, and it is not clear what the authors are trying to say.

Nearly all figures are too small, and the captions are very hard to read.