Response to Review 1

René Hommel* et al. 20 November 2014
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We thank the referee for her/his thoughtful comments and suggestions for improvements. We critically revised the manuscript and think that the manuscript has significantly improved after the comments and suggestions have been considered.

In the following, we respond to individual comments. Original remarks of the referee have been enclosed in quotation marks, using an *italic* font. Responses are given below each comment and are marked by "Answer" in a ***bold italic*** font.

Major comments:

"1) It is often difficult to interpret the magnitude of variations in the (color) height vs. time lag plots with small labels (Figs. 4-11), and it is too much work for readers to determine if the variability is large or small. The authors might consider making these plots in % of the respective background values."

Answer: We will carefully revise the figures and increase the font size relative to the size of the figures.

Regarding the 2nd part of this comment we like to state the following: During times of manuscript preparation we carefully elaborated the presentation form of the figures. We also tested whether it makes sense to show composite plots in relative units. Finally we decided to present anomalies in absolute values and denote their relative strength in the respective paragraphs, where the figures and the mechanisms of the individual QBO modulations are discussed. Our decision is based on the following reasons:

1) QBO induced anomalies in stratospheric parameters are commonly presented in absolute terms within diagrams, and we would like to use this common approach. This is in particular true for the composite plots like those we are showing.
2) When the composites are presented in relative units, the colour shading of quite a few figures will change with the result, that the visual impression of these plots ("guiding the eye by colours") in quite a few figures generates a message which is even more difficult to explain as in the current form, focussing on absolute units. For example, QBO anomalies in the aerosol effective radius are small below 20 hPa (Fig. 7b). Using the red-white-blue colour shading, which is commonly used to illustrate anomalies, would largely suppress the existence of the induced anomalies in this region. The shading would then be simply too bright and too close to the zero line so that the very first visual impression is "no effect". But in absolute quantities it is clear that this is indeed not the case. It would also be misleading if one attempts to interpret how the QBO modulates the different processes, which determine an integrated quantity like the effective radius. After a very critical examination of our results we decided to withdraw all composites showing relative units in favour of well described relationships in the respective sections. However, we attempt to refer to the relative strength of the modulations when it is important.
3) We also checked whether it would help to show such plots in relative units in an additional panel on the right hand side of each figure from Fig. 4 - 11. However, we felt the information content of those figures did not increase - instead, it rather led to confusion due to that what we explained in 2).

"2) I had difficulty in understanding the take-home message of the CCMI data results in Fig. 6. Both the climatological mean and the composited QBO variability are substantially different from the model results in Fig. 5. I do not worry about the statistical significance of the model results because the QBO is the dominant variability, but I am less convinced about the observed data, where the patterns look confusing and noisy. Can the authors evaluate the statistical significance of the QBO variations in Fig. 6b, and critically assess the ability of these data to constrain the model results?"

Answer: We revised the CCMI data set and indeed it turned out that we made a mistake in the calculation of the anomalies. We replace Fig. 6b by an anomaly composite plot, which is less patchy. Now the SAD anomalies in CCMI between 1996 and 2006 are much closer to our model results. The largest improvement is found in the transition periods from QBO east to west phase, where CCMI SAD anomalies are now negatively modulated below approx. 15 hPa. This behaviour is very similar in our model (Fig. 5b and d).

Differences remain in regions directly above the TTL, which presumably is due to volcanic influence in the last 3 years of the analysed time-series. In the revised manuscript we will further discuss the differences of the two data sets (model and observation), and mention the importance of the potential volcanic signature in the observations.

Regarding differences in the mean SAD, we will improve our discussion to make clear why the data sets generally differ from each other. Please note that the analysis of the climatological mean states was in the focus of our companion paper, Hommel et al. 2011, where we compared model results with two SAGEII data sets. Here, we are using the gap filled and extrapolated CCMI data set (Arfeuille et al., 2013) which is a merger between ERBS/SAGEII and Calipso/CALIOP measurements (CALIOP in the last 1.5 years of the analysed time period). This has been mentioned in our manuscript on page 16258. We will carefully revise the respective paragraph.

Furthermore, we add a paragraph about the statistical significance of the inferred anomalies in model data as well as in the CCMI SAD. We performed Student t-test's for all model parameters relative to our reference simulation (Hommel et al., 2011) and the F-test against the theoretical red noise spectrum as an appropriate model of variability for a wide range of atmospheric parameters (e.g. Gilman et al, 1963; Yang and Tung, 1994; von Storch and Zwiers, 1999). Significances are now considered in the figures 4-11. The potential volcanic impact in the observed SAD is not only affecting the magnitude and timing of the derived anomalies, it also leaves an imprint in their statistical significance.

"3) The overall results are probably intuitive to experts on stratospheric aerosols, but less so to the general reader. It might help to complement the Discussion section with a summary figure or cartoon highlighting the important aerosol processes and their physical links identified in this study. What are the explicit 'non-linear relationships' mentioned in the Abstract and Discussion section?"

Answer: We will revise the paragraphs discussing the interactions of the aerosol processes (affecting whole Sec. 3) in order to highlight that the processes are not linearly coupled. To elaborate this a little bit further, this means nothing else than that small relative deviations in one aerosol process due to the QBO (within a certain altitude range) may cause inhomogeneous anomalies in a different aerosol parameter (within the same altitude range). In addition, they may also trigger other aerosol processes, in turn affecting the latter parameter. With the current state of analysis we hesitate to call this process "feedback", because more in-depth studies are needed to clarify how the processes are coupled due to the three major pathways of potential QBO imprints in stratospheric aerosol: advection (of aerosol and precursors), microphysics (in particular nucleation and mass transfer of H_2O and H_2SO_4) and chemistry of precursors. As we state in our manuscript, the simulated aerosol was not coupled to radiation and the full stratospheric chemistry, which would induce more pathways for the QBO to affect the Junge layer.

We agree with the referee and think it is a good idea to sketch the relationships. Although this has been done several times in other papers (e.g. Choi et al, 2002), we shall at least attempt to illustrate the advective component of QBO modulations in stratospheric tracer constituents. We will carefully examine whether we find an appropriate form to illustrate the relationships in a simplified manner.

"4) p. 16255, lines 1-2: it is not easy to identify the 5 km height difference in aerosol mixing ratio in Fig. 3. One suggestion might be to add a figure simply comparing the vertical profiles of mixing ratio for snapshots of QBO east and west phases."

Answer: We will critically examine this suggestion and consider an additional figure.

"5) Regarding the ozone QBO above 20 hPa: because the ozone photochemical lifetime is short above this level, ozone chemistry is important or dominant in this region, rather than the direct effects of transport (transport influences species such as NOy, which in turn influence ozone)."

Answer: We agree with the referee and revise the respective sections.

"6) p. 16261, line 2: 'interfere' rather than 'infer'? "

Answer: Typo, will be corrected.

"7) In addition to Fig. 8, it might be useful to show the aerosol size distributions for extreme QBO phases (perhaps at one or two altitudes where the changes are large)."

Answer: We agree with the referee that this improves the understanding on how the processes influence the size distribution. We will produce a Figure.

"8) I could not find any reference or discussion of the DMS results in Figs. 11 e-f."

Answer: Thank you for pointing this out. The Figure should have been removed already in the submitted manuscript. DMS is a very minor sulphate precursor in the stratosphere (Weisenstein et al, 1997; SPARC ASAP, 2006; Hommel et al., 2011), and the QBO is not affecting its mixing ratio distinctly. Therefore, we decided to remove the Figure. In the revised manuscript we will, however, mention the DMS behaviour in a small paragraph.

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