First, we'd like to thank the Dr. Damadeo for bringing to our attention the very recent (Dec. 2014) Damadeo et al. [2014] publication.

There seem to be three major thrusts to this comment. (1) Sampling issues and our basic fitting scheme, (2) our use of a linear trend term in analyzing the HALOE data, and (3) our use of a linear trend term in analyzing the MLS data. We will respond to each of these points.

(1) With respect to sampling issues (which do not apply to the MLS measurements), we note that the linear trends in HALOE ozone which we have derived have certainly been shown previously. Remsberg [2008] shows, in his Figure 13, a linear trend in ozone (in %/decade) which is very similar to what we report in Figure 2 (in ppmv/yr). The 10hPa tropical region in which we are particularly interested has, according to Remsberg [2008], a confidence interval of greater than 90% for their partial rank order correlations [Remsberg et al., 2001]. To quote from Remsberg [2008]: "This finding indicates that the sampling from HALOE was adequate for representing this term in the zonal average ozone even in the tropics." We did fail to bring up these points in the manuscript and will now certainly do so. While we certainly do not deny that sampling can have some effect on overall trends, we note that: (a) the study by Damadeo et al. [2014) is for a different dataset (SAGE, not HALOE), (b) it is for two different and shorter periods (1985-1995 and 1998-2005, while we use 1991-2005), and (c) even the largest quoted change in trends (it is not clear where his occurs) based on the difference between the two methods is 2%/decade, whereas in the trend at the tropical mid-stratosphere is ~6%/decade. And it seems, if we understand Figure 2 in the comment correctly correctly, that there is actually no disagreement on the occurrence of a decrease in ozone over the full HALOE dataset (although it is not clear to what precise level this figure refers). We do not understand the context of the comment: "Unfortunately, no combination of model terms yielded a negative trend in the tropical middle stratosphere in the late 1990s", since we make no specific claim about a negative trend in the late 1990s, only over the full period 1991-2005.

(2) Indeed, many studies have investigated the effect of Pinatubo aerosols on transport (e.g. Eluszkiewicz et al., [1997]; Rosenfield et al., [1997]; Considine et al., [2001]), and we do point out in our manuscript that "events such as the eruption of Mt. Pinatubo may alter the chemistry and dynamics of the stratosphere for extended periods". Considine et al. [2001] made use of SAGE post-Pinatubo aerosol measurements in a model with CH4 and H2O in an attempt to explain the results of Nedoluha et al. [1998a, 1998b] and Randel et al. [1999] and concluded that "These comparisons suggest that the aerosol from the Mount Pinatubo eruption could have contributed to the observed changes in CH4 and H2O following the eruption but was probably not the sole driver of the observed changes". Thus while assuming a volcanic proxy may provide a good fit to the data, it does not, in our opinion, actually solve the physical problem. Nonetheless, it is worth noting as an alternative approach that some may choose and we are pleased to reference Damadeo's adoption of this proxy while simultaneously noting that it comes with its own problems. In any case, regardless of how the changes between O_3 and NO_2 at the start and end of the HALOE timeseries (1991-2005) are fit, it is clear that the mixing ratios of these species at 10 hPa in the tropics have changed, and given the relationship between the changes in these two species we have shown that these changes can be attributed to a change in transport. We do not

understand how Figure 1, which simply shows annual average ozone anomalies, can in any way be considered misleading.

(3) The MLS data shown in Figure 1 and Figure 5 clearly shows that N_2O and O_3 at 10 hPa in the tropics is lower from ~2009-2012 than in the previous years. Although NCEP wind anomalies used in our fits do not indicate unusual wind strengths in those years it certainly is possible that this change could be somehow be attributed to a change in the QBO winds (QBO periods were unusually long during ~2008-2013). Regardless of whether the decrease in O_3 and N_2O in the MLS measurements from ~2009-2013 is fit as a trend or is fit using some QBO-related terms, this does not change the fundamental result that they were low during those years, and this indicates a change in dynamics from earlier years.

Finally, we of course do not dispute that had we chosen different starting and ending years the trends calculated from the HALOE and MLS timeseries would be different. However, regardless of the technique used to fit, the data shown in Figure 1, it is clear that most of the ozone annual averages near the end of the timeseries are lower than near the beginning. What we have shown is that this change is consistent with a dynamical explanation. We note that, while we have not provided a detailed statistical analysis of the significance of the observed changes, it seems to us that the close tracking of O_3 and $NO+NO_2$ (in the case of HALOE), and of O_3 and N_2O (in the case of MLS) provides a far more convincing indication that these are real changes than do statistical analyses of individual species (which are much more easily subject to undetectable instrumental trends).

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