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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Halogen Occultation Experiment (HALOE) and balloon-borne in situ measurements of methane in stratosphere and their relation to the quasi-biennial oscillation (QBO)" *by* P. K. Patra et al.

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Reply to Referee#1

The general approach in this paper has been to use HALOE observed CH4 profiles to understand the variabilities in the balloon-borne CH4 measurements. Since the balloon-borne experiments are expensive, thus not made very frequently at the Indian site (Hyderabad). This makes the interpretation of the widely spaced (one in about 4 years) vertical profiles often difficult. Therefore, our orientation in this paper has been to use the frequently measured CH4 profiles by the HALOE instrument to extract the dynamical information (e.g. QBO) and use that to understand the variabilities in the in situ observations.

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It is probably not fair to say that `the direct comparison of satellite data in 1994 to balloon data doesn't show any similarity'. In our opinion the changes in mixing ratio with height compares quite well between the two datasets from different measurement techniques. Infact the changes in gradient are the main focus of this work. We are not quite sure why an offset (˜0.1 ppm) is present during 1994. Larger than average negative anomaly (comparable to this offset) was also found in the HALOE CH4 data during 1994 over the tropics (Fig. 7 of Rosenlof, JMSJ, Vol.80, No.4B, p. 831, 2002). However, the match between the two datasets in 1998 is excellent, both in terms of altitude gradient and absolute concentration of CH4. The inclusion of NASA/GSFC 2-D model results compares better with the HALOE observations in 1994 and balloon measurement in 1998 (Fig. 4).

The references in the text to Randel et al. (1998) is directed to this paper. We are sorry for the incorrect bibliography list.

As we have tried to point out in the beginning, we are not really trying to isolate the dynamical signals in our balloon measurements. Instead take the information available from other datasets, such as the HALOE CH4 observations and NCEP/NCAR reanalysed winds, to understand the gross changes in the balloon measurements.

Yes, it is rather difficult task to explain every features in the CH4 vertical profiles by using the zonal wind shear. However, one can see that the large scale features to be quite consistent as explained in Section 2, para#3. We will modify this discussion during the revision for more clarity. As an example the 1990 profiles of both CH4 and zonal winds are quite different from all others in the stratosphere. It should also be kept in mind that the zonal wind shear do not produce the changes in CH4 profiles exactly at the same height and the affects can be seen only in the stratosphere. Generally, the effect of wind shear on CH4 occurs at the layers above as the slopes in CH4 mixing ratio are lesser at lower heights, e.g., the wind shear at 27 km would influence the CH4 concentration at 30 km. In the troposphere the story is quite different as we move in to a convective system from stratified system (the stratosphere). We see clear evidences

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that vertical transport is responsible for producing the observed slopes in CH4 between the middle troposphere (8-9 km height or ˜300 mb pressure level) and tropopause (at ˜17 km). The NCEP/NCAR reanalysis generated relative humidity at 300 mb, monthly means for March and/or April averaged over the global tropics (20S-20N), exhibit very strong anti-correlation (coeff.=0.99) with CH4 slope. This means that slope in CH4 mixing ratio is smaller when there are stronger upward motion in the tropics, which is also strongly supported by similar analysis of NCEP/NCAR vertical velocities (Omega in Pa/S). We have included this discussion in the revised version (see Table 1 and Sec#3, Para#2).

We think, the suggested overplot would be quite similar to that is shown in Figure 4. We have included the NASA/GSFC 2-D model in this plot, which clearly shows the influence of QBO meteorology in explaining the interannual variations in the observed CH4 profiles.

Since we are not dealing with precise amplitude and phase of the QBO signal and there is also quite a bit of discussion on the analysed HALOE CH4 distributions, we believe the analysed wind data is more relevant to this study. Only the amplitude of QBO signal is known to be smaller in the reanalysed winds compared to that is actually observed in the observed winds over Singapore (e.g. McCormak and Siskind, JGR, Vol.107, No.D22, 2002).

The figure caption has been changed.

Reply to Referee#2

We thank the referee for appreciating our work and providing us with the detailed list of up-to-date references. I read quite a few of those throughly and that have been a good learning process altogether. From the reference list (Fleming et al., 2002), we have been able to obtain the NASA/GSFC model simulations for our balloon flight dates and

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the results are shown in Figure 4.

The discussion relating to the new references are given in: 1. Sec#1, Para#1, 2. Sec#2, Para#3, 3. Sec#3, Para#1, 4, 6.

Finally, a few references, we think, are closely linked to this work and we included related discussion in this revised version of the manuscript.

1. Choi et al., 2002

2. Fleming et al., 2002

3. McCormak and Siskind, 2002

4. Shepherd, 2002

and Randel et al., 1998 (corrected)

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 1925, 2003.

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