Reply to Comments of Reviewer 1 (*in italics*)

This study investigates variability in stratospheric CH4 as measured by HALOE during the 1992-2005 time period. This is a follow up to a previous study on this topic by the same author. The trends over three 5-year periods are found by multiple linear regression with the expected terms included to account for stratospheric variability. The focus on 5-year periods rather than the entire 13-year period as in the author's previous paper is understandable due to the variability in the 5-year trends. Reexamination of available measurement data of this kind to try to infer more information is useful and important.

While I support this study and applaud the brevity, the discussion of the trends is confusing and needs some rewriting before publication. I have listed some specific instances below where I was unclear about the discussion and I would suggest that at least these points be addressed. But I would also encourage the author to review all of Section 3 for clarity and consistency. The summary section is written clearly so that does help get the main points across in the end.

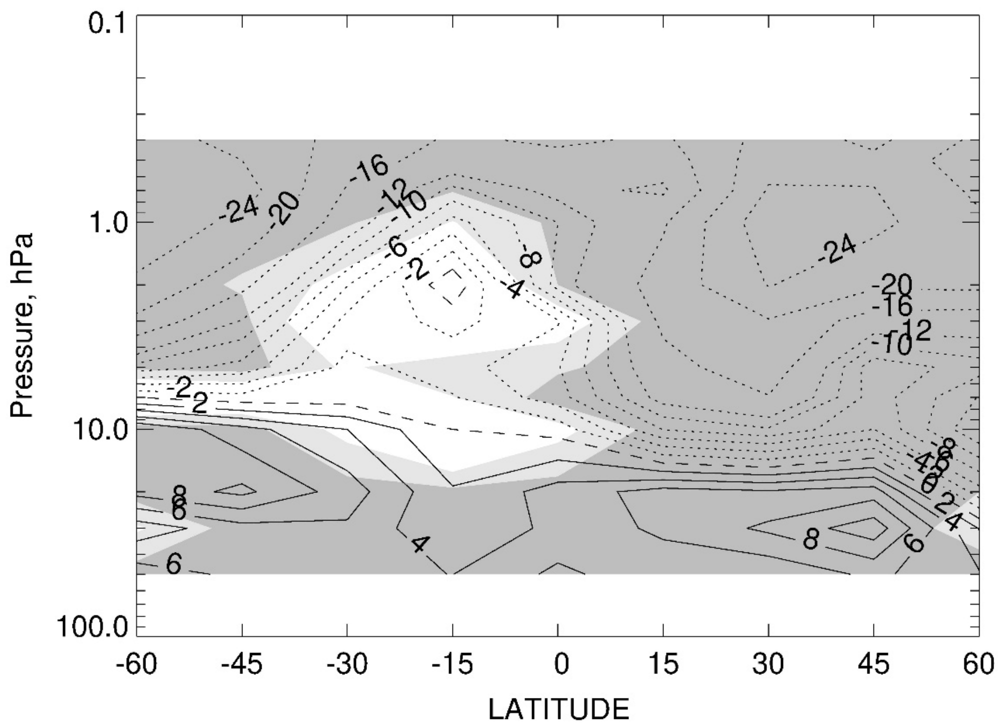
Thank you for your careful review of the manuscript and for pointing out several inconsistencies about my interpretations of the findings from Figures 3-5. I will revise Section 3, as noted in my replies below to your specific comments.

Specific comments

Line 77: The 5-year time span chosen to evaluate trends likely doesn't eliminate all bias due to the QBO since the QBO has a variable period that is almost never exactly 2.5 years at all latitudes and altitudes. How different would Figures 3-5 look if you started the time series in June or August? Your only mention of this is in lines 187-9. Related to this, it would be helpful to show the statistical significance of the trends in Figures 3-5.

I tested the sensitivity of the trend coefficient from the MLR fit in Fig. 1a (30°N, 10 hPa), where a QBO cycle shows clearly. For the testing I changed the length of the QBO cycle (28 months versus 29.5 months) and then separately by considering a time span of 58 months rather than 5-

yr (or 60 months) for the MLR analysis. The resulting coefficients differed by less than 6% from the trend coefficient of Fig. 1a. I conclude that the 5-yr trend distributions in Figs. 3-5 are robust with respect to my approximations of the QBO cycle. On your related suggestion, I added shading to indicate the significance of the trends, where dark shading is where the confidence interval (CI) for the trends is greater than 90% and where light shading is for CI between 70 and 90% (see revised Fig. 3 below).



Lines 108-112: I don't understand the reasoning here. The trends are negative throughout the upper stratosphere during this period and you state that the 'ascent within the deep branch of the BDC occurred mainly in the northern subtropics' where the trends are most negative. Ascent will bring relatively large mixing ratios to essentially any region of the stratosphere as shown by Figure 2. You actually state this further down in lines 131-132 when referring to the lower stratosphere. And then further down in lines 142-3 you state that the 'negative changes in CH4 in upper regions of Fig. 3 imply that there was a slowdown in the deep branch of the BDC.' This is what I would conclude as well but doesn't seem consistent with the earlier statements. Some clarification is necessary.

I will move the two sentences from lines 142-145 to the end of the first paragraph of Section 3, giving better continuity of the findings for the upper stratosphere.

Lines 128-9: Wouldn't it be easier to infer BDC changes if you removed the tropospheric growth rate before doing the MLR analysis? If you had a period with no BDC changes there would still be CH4 trends in the stratosphere due to the tropospheric growth rate.

You are correct that there should be a residual increase in stratospheric CH_4, due to its positive trends within the troposphere. However, I am not confident of the exact tropospheric trend curve to use for such a correction. Nevertheless, the CH_4 trends in Figs. 3-5 are much larger (or smaller) and override tropospheric trends throughout much of their pressure/latitude domains.

Lines 140-1: Positive trends in the tropical lower stratosphere should indicate stronger upwelling and yet you say they indicate a reduced tropical upwelling. Again, some clarification is necessary.

The sentence beginning on line 140 is incorrect; thank you for pointing that out. I will revise it to say, "The 5-yr changes in Fig. 3 for the lower stratosphere indicate an accumulation of CH_4 at middle latitudes".

Lines 151-3: Here you state that the SSWs led to greater ascent of CH4 in the tropical upper stratosphere where it was chemically destroyed leading to negative trends. Whereas, in the next section (Lines 183-6) you state that the SSWs in the later period 'accelerates the deep branch of the BDC bringing more CH4 to high altitudes' resulting in a positive trend. These seem to be inconsistent statements.

I will correct those inconsistencies and revise the sentence at line 151 as "The negative changes in the upper stratosphere are enhanced in Fig. 4 compared to Fig. 3, where the chemical conversion of CH_4 to H_2O and H_2 is most effective". I will also change the sentence starting on line 183 to say, "There was an increase in CH_4 at upper altitudes, where the effect of SSWs may have also led to greater poleward transport of CH_4 to higher latitudes".

Finally, I plan to add the following figure to the revised manuscript to show details of the effect of the Pinatubo eruption on CH_4 in the lower stratosphere, in addition to Figure 3. The top panel is for 15N, 50 hPa and shows an initial increase in CH_4 in 1991-mid1992, followed by decreasing values through 1993. Those variations are in accord with changes in the shallow branch of the BDC, as reported by Diallo et al. (2017). The bottom panel is for 45N, 30 hPa that shows the steady increase of CH_4 from 1992 to 1997 as a companion to the trends in Figure 3.