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Atmospheric Measurement Techniques Discussions

Interactive comment on "Residual Temperature Bias Effects in LIMS Stratospheric Ozone and Water Vapor" *by* Ellis Remsberg et al.

Ellis Remsberg et al.

ellis.e.remsberg@nasa.gov

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Replies (denoted by asterisks *) to comments from Anonymous Referee #2

Anonymous Referee #2 Received and published: 4 December 2020

This paper deals with biases in the distributions of ozone, water vapour and nitric acid from observations of the LIMS satellite instrument and derived within the V6 retrieval version. The trace gas biases are due to biases in the temperature profiles T(p) that are caused be horizontal gradients in temperature that are not fully accounted for in the retrievals. The approach chosen compares the retrieved profiles from the descending and ascending orbit branches that are, at the same time, day and night observations. However, the assessment of biases in the trace gas fields is complicated by the fact

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that either real diurnal variations occur, or the retrievals are further biased by neglected Non-LTE effects.

General comment: The paper provides a theoretical assessment based on assumed horizontal temperature gradients along the light path through the atmosphere, and confronts these numbers with observed A-D differences. Comparisons to reference measurements are presented in order to validate the bias assessments. Over all, the paper is clearly written, concise and to the point. It fits very well into the scope of AMT. I recommend publication after some minor revisions.

*Thank you for your careful review of the manuscript and for your constructive comments.

Specific comments: Specific comments: As already said, the paper is clearly written. The only overarching issue I could not resolve is the quantitative assessment of the temperature bias caused by not fully accounted horizontal gradients (second row in Table 1). The authors state in the introduction (l67 - 69): "While the LIMS algorithm makes first order corrections for T(p) gradients, residual bias effects are still apparent in the V6 species distributions.", and in section 2.2 they state (l146 - 147): "Estimates of a bias in V6 T(p) are in Table 1 (row 2), according to the error simulations of Remsberg et al. (2004).". I have checked this paper, but I could not identify the numbers in Table 1 of this manuscript in the Remsberg et al. (2004) paper. I suggest that a short outline of the assessment of the temperature bias due to horizontal gradients should be included in this manuscript.

*Table 1 and the paragraph beginning at line 141 describe how the LIMS retrieved species profiles are sensitive to temperature profile or T(p) bias. Then, the paragraph at line 207 discusses analyzed A-D temperature differences on a pressure surface, both before and after their first order correction for horizontal temperature gradients. To be clearer, we will revise lines 146-147 to say “Estimates of a bias in V6 T(p) are in Table 1 (row 2), in accord with the temperature bias estimates in Remsberg et al. (2004,

their Table 2, row g)". We will also add to the discussion in the following paragraph at line 207 and refer to the findings of Gille et al. (1984) from their Section 5 entitled "Corrections for Atmospheric Gradients".

Abstract, l38 - 40: The authors state here: "We recommend that researchers use the average V6 Level 3 data for their science studies of stratospheric ozone and water vapor wherever diurnal variations of them are unexpected." However, pseudo-diurnal variations appear for ozone, and, to a lesser degree, to water vapour, due to the neglect of NLTE effects (l57 - 59 and l120 - 123). A simple averaging of day and night values does not help here. I suggest that a more careful wording is used in the abstract.

*Abstract, at lines 38-40, instead of "wherever diurnal variations of them are unexpected", we will say "except for daytime ozone in the lower mesosphere and for daytime water vapor down to the uppermost stratosphere, both of which have uncorrected NLTE effects". While we agree that there are real diurnal effects in stratospheric ozone, they are not determined accurately for V6 because of small biases in T(p).

L161 - 163: "The sharply increasing H2O near the tropical tropopause is due, in part, to residual emissions from cirrus cloud tops that were not screened completely from the bottom of the LIMS H2O radiance profiles prior to retrieval." Is this just a presumption, or have you demonstrated this within an other publication? In the first case you should indicate that you assume this, in the latter case you should provide the reference.

*Lines 161-163 The presence of radiant emission from cirrus cloud tops was detected, based on a threshold criterion for the vertical slope of retrieved ozone mixing ratio profiles at pressure-altitudes below 45 hPa and between $\pm 30^{\circ}$ latitude (see Section 2.2 of Remsberg et al. (2007)). Those cloud top estimates apply to the other LIMS species, as well (see also Section 2.2 of Remsberg et al. (2009) for H2O). Locations of cloud tops are in separate daily files that are a part of the LIMS V6 Level 2 or daily profile data set. We will add several sentences to this paragraph about the cloud sensing approach of V6.

L341 - 342: I do not understand the following argument: "... the residual biases in the T(p) distributions are related to seasonal changes for the Brewer-Dobson circulation ...". Some clarification would be helpful.

*Lines 341-342 We agree that this argument is not developed well and appears speculative. We will delete the sentence at line 340 and move its parenthetical phrase to the end of the sentence at line 339.

L345 ff: HNO3 does not appear in the title, abstract or any section heading. I suggest to give HNO3 the appropriate place in the manuscript.

*Lines 345ff We will generalize the title of the manuscript to “Residual Temperature Bias Effects in Stratospheric Species Distributions from LIMS” and include findings for HNO3 and NO2 in Section 7 and in the Abstract. We will change the Section 4 heading to “Day/night differences in V6 species”. Subsections 4.1 and 4.2 will keep their original focus on ozone and water vapor. We will then add Subsection 4.3 about HNO3 and NO2 and include brief discussions about temperature bias effects in both species. This reply includes a plot of March NO2, based on a more standard diagnostic of the distribution of its A to D ratios. NO2 has a large diurnal variation that changes with solar zenith angle slightly, as shown in the plot. V6 NO2 has low S/N below about the 30-hPa level and is inaccurate there. The LIMS observations cross the day/night terminator at the highest latitudes; the distribution of ratios should be accurate elsewhere. There is a slight asymmetry of the 0.7 contour about the equator, a feature that may be due to our account of the distribution of interfering radiance from methane, which is dominant in the tropical stratosphere. That interfering radiance represents a larger correction in retrievals of day versus night NO2 mixing ratios. Thus, we report no clear indications of temperature bias effects in V6 NO2.

L 404: "... due to uncorrected NLTE emissions from CO2 and ozone ...": has this been assessed quantitatively? If so, please provide the reference.

*Line 404 First-order assessments of uncorrected NLTE effects in CO2 and in LIMS

ozone are from Mlynczak and Drayson (1990) and Solomon et al. (1986).

L407 - 408: Is it reasonable to assume a negative T(p) bias? Is the comparison with ROCOZ ozone sondes the only indication for that? Could it be that this result is caused by an over- or underestimation of the Non-LTE effect?

*Lines 407-408 We are commenting on the results in Figure 14 for April and May at 1.5 to 3 hPa. Although NLTE is an unlikely factor at those pressure-altitudes (see Edwards et al., 1996), we agree that we did not confirm that V6 T(p) has a negative bias. More likely, there is a real ozone difference at Wallops Island for those months of 1979 versus the ozone climatology of the mid-1970s. We will revise the text, accordingly.

L435 - 436: For me, having scientifically "grown up under the ozone hole" the statement in the first sentence of this para is a bit strange - although it might have been true (at least within some limits) at the time of the LIMS measurements. Maybe a link to the pre-ozone hole area of the LIMS observations should be made here.

*Lines 435-436 We are referring to the behavior of ozone transport in the middle stratosphere or near 10 hPa. LIMS made no measurements poleward of 64°S for June through late October. Even so, Remsberg et al. (2020, ACP, https://doi.org/10.5194/acp-20-3663-2020) reports evidence from the V6 species for chemically induced loss of ozone in subpolar regions of the lowermost stratosphere from late October through November 1978.

L483 - 486: Similar to the abstract, the neglected non-LTE effects in ozone and water vapour retrievals should be kept in mind, and the statement about averaging the A and D observations needs a bit more caution.

*Lines 483-486 We will add a cautionary statement to that effect.

Technical comments: L 183: ... temperatures for (or in) March. References: several dois are incorrect.

*Line 183 We will make the change. References Although you do not point out any

specific doi errors, we will check about them. See also the attached figure for NO2.

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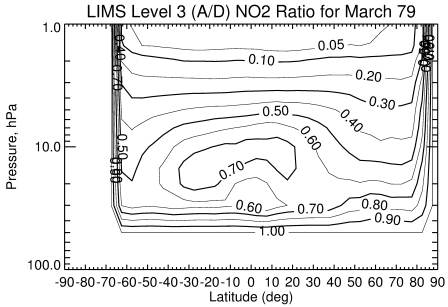

Fig. 1. Distribution of the A/D or day/night NO2 ratios for March

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