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Interactive comment on "Relative drifts and stability of satellite and ground-based stratospheric ozone profiles at NDACC lidar stations" *by* P. J. Nair et al.

Anonymous Referee #2

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General comments:

This is an interesting and generally well written manuscript dealing with the investigation of possible long-term drifts between different sets of stratospheric ozone time series. The ozone data sets include lidar measurements at 6 NDACC stations, ozone sonde measurements at most of these stations as well as satellite measurements by SBUV(/2), SAGE II, HALOE, and the MLS instruments on UARS and Aura. It is demonstrated convincingly that most of the stratospheric ozone time series used in this study are characterized by near-zero instrumental drifts and that they are suitable for long-term ozone trend studies. There are however some inconsistencies between MLS/Aura

and some of the lidar stations that need to be addressed (see also specific comments below). Fig. 4 indicates fairly large drifts between Aura/MLS and the lidars at MOHp, OHP and TMF (up to -15 % / decade). The paper briefly suggests that this may be explained by anomalous featues or trends in the lidar time series. I believe, this aspect – being a crucial one, also in terms of the usefulness of the MLS/Aura time series for long-term trend studies – needs to be expanded significantly. If there are indications that the MOHp lidar measurements were affected by a high bias after 2007, they should be presented. Similarly, anomalies in the TMF lidar measurements in 2008 and 2009 should be presented and discussed in more detail. If the conclusion is maintained, that Aura/MLS ozone data is suitable for long-term trend studies, then – in my opinion – the authors need to demonstrate convincingly, that issues with the lidar measurements are responsible for the relative drift between Aura/MLS and the lidars. Still, overall this is a relevant and timely paper that will make an important contribution to the field. I recommend publication of this manuscript after the following specific comments were considered and addressed.

Specific comments:

Page 473, line 11: "and drifts are less than $\pm$0.3%yr-1 at all stations". Looking at Fig. 4 this statement is not correct. There are larger drifts between 20 and 40 km, e.g. for MLS/Aura relative to several of the lidar stations. See also comments below.

Page 473, Line 22: "of THE Antarctic ozone hole" ?

Page 473, Line 26: "The analysis of stratospheric ozone trends .. are currently" -> "The analysis of stratospheric ozone trends .. is currently"

Page 474, Line 15: "In a recent study .." The exact meaning of the sentence is not clear (at least to me). What irritates me is the phrase "to a lesser extent". Do you mean to suggest that Dhomse et al. found that the TOC increase is to a lesser extent (compared to what?) caused by an enhanced residual circulation? What is the main effect then? What exactly is confirmed by Harris et al. (2008)?

Page 476, line 15: I believe the expression “radiations” does not exist. Suggest to change to: “ .. emission of lidar radiation at two wavelengths”

Page 477, Line 4: “in the use of reference wavelength” -> “in the choice of the reference wavelength”?

Page 477, line 16: Remove comma after “and”

Page 481, line 1: “and are” ->”that are”

Page 482, line 1: “in the work by Froidevaux et al. (2008); Jiang et al. (2007); Livesey et al. (2008).“ -> "in the works by Froidevaux et al. (2008), Jiang et al. (2007) and Livesey et al. (2008).“

Table 1: Why is SBUV/2 data really only used until 2007?

Fig. 1, panels d – f: These panels suggest that there are no co-locations between ozone sondes and SBUV, SAGE II and HALOE, which can certainly not be true.

Page 483, line 26: “with a time difference maximum of $\pm$12 h.” Diurnal variations of stratospheric ozone may also be important below 40 km altitude, and certainly between 40 and 50 km.

Page 484, equation (2): Why is the sum in the numerator on the right hand side also over j, i.e. altitude or pressure? I think this should just be a sum over i.

Page 486, line 10: “Hence, the analysis excludes the outliers such that the profile consisting of very low and high ozone values at all altitudes are removed from the analysis.” What are the exact quantitative criteria for this selection process? What are very low and high values?

Page 486, lines 23 – 27: Are the MLS averaging kernels not available for this purpose?

Page 487, line 15: What is a “slightly large drift”? Can you provide a quantitative statement?

Page 487, line 19, Arletty data: If I understand correctly, then this data set consists of the MSIS-90 climatology for altitudes above 30 km. MSIS probably does not include any possible temperature trends caused by dynamics changes (possible enhancement of Brewer-Dobson Circulation), changing radiative balance due to increasing greenhouse gases etc. Please provide more detailed information on MSIS 90 and its limitations. I’m not an expert here, but I’d be quite skeptical whether MSIS 90 really performs better than ECMWF in this context and at these altitudes.

Page 490, line 26: I suggest changing “slightly large” to “slightly larger” or “an increasing negative”

Page 491, line 15: “However, some significant drifts .. for SAGE II .., for HALOE ..” MLS should also be mentioned here, because it exhibits really large drifts at MOHp, OHP, and TMF.

Page 492, lines 1-6: This paragraph suggests that anomalous drifts in the lidar measurements at MOHp and TMF are responsible for the relative drift between MLS/Aura and the lidars. Is this the intended meaning of this paragraph? Ins’t there good reason to believe that the lidar measurements are more accurate? I also don’t fully understand the statement “and TMF lidar shows high ozone values in 2008 and 2009 above 30 km compared to all other measurements”. What does “other measurements” refer to? Out of the satellite instruments discussed here, only MLS/Aura was operational in 2008 and 2009, and perhaps the drift is actually mainly caused by a drift in the MLS data set. Please provide more detailed information on the possible issues with the TMF measurements in 2008 and 2009.

Page 492: “It implies that the lidars can be taken as a reliable reference for drift evaluation of satellite and other ground-based measurements.” Given the apparent drift between MLS/Aura and the lidars at MOHp and TMF this statement is somewhat contentious, because it is not generally applicable. It is not generally applicable, because SBUV/2 data were only used up to 2007, and the differences between MLS/Aura and

the lidars at MOHp and MTF start in 2007 and 2008/2009, respectively. I suggest changing the statement, otherwise it directly suggests that there is an anomalous drift in MLS/Aura (see previous point).

Page 493, line 4: “drifts maximum” -> “maximum drifts”

Same sentence: “and Lauder and $\pm$0.4%yr-1 at OHP”. Looking at the Fig. 6a) this doesn’t seem to be correct, because differences well exceed 0.5%/yr between about 33 km and 43 km and also below 18 km.

Page 493, line 10: “At MLO, the coincidences are available in 1999–2003 only. This is the reason for the estimated large drifts at MLO.” These two sentences already appear 3 lines above.

Page 494, line 13: “relatively fewer” -> “relatively few”

Page 494, section 2.4.2: “Average of the drifts of long-term measurements” Are the averaged drifts simply arithmetic mean values of the 3 individual drift values, or are the weighted by the number of coincidences with each of the other instruments?

Page 495, line 1: “Generally, as found in the previous comparisons, all data sets show small drifts of around $\pm$0.2%yr-1 at 18–45 km.. “ Does this statement refer to drifts averaged over the 18-45 km altitude range, or to typical values at individual altitudes within this range? Looking at the panels, the first is/should be the case, and I suggest to mention this explicitly in the paper.

Page 495, line 3: “slightly large” -> “slightly larger”

Page 495, line 4: “play” -> “plays”

Page 495, line 8: “It is obvious (from Fig. 4) that the 8 yr data record of Aura MLS yields comparable drifts as of the long-term measurements at all regions.” Looking at Fig. 4 this statement is not correct. Fig. 4 shows that at MOHp there is a -1 % / year negative drift between MLS and the lidar over an extended altitude range. At OHP the

drift reaches -1.5 % / year near 40 km, and at TMF a large negative drift is observed. 1% / year corresponds to 10% / decade, and if MLS is the origin of these anomalous drifts, then MLS is probably not suitable to extend the SAGE/HALOE time series.

Page 496, line 6: “because of the increase in ozone lidar measurements after 2007, as discussed in Sect. 4.2.1.” Again, this suggests that the MOHp lidar measurements are erroneous. From the analysis presented here this conclusion cannot be drawn, in my opinion. How do we know that the increase in the lidar measurements has not actually occurred? Looking at Fig. 8, and at altitudes between 23 and 42 km my visual impression is that there is a drift between MLS/Aura and MOHp lidar not only after 2007, but already from 2004 onwards.

Page 496, line 18: “which can be due to higher lidar ozone during the period as compared to other measurements“. Again, what "other measurements“ are you referring to? Can this be backed up with other published results?

Page 497, line 3: “It indicates that the combination of these satellite observations provides a potential long-term data set for the evaluation of long-term ozone trends in the stratosphere.” I agree that the drifts of the combined time series are very small, and this is a very good result. However, given the inconclusive drift analysis between MLS/Aura and MOHp, OHP and TMF, I’m not convinced that the MLS data set is suitable for long-term trend studies at the current stage. I believe the remaining inconsistencies have to be resolved (not necessarily as part of this study), before such a general statement can be made.

Page 497, line 20: “This is attained first by comparing all measurements with respect to lidars, which yields drifts of less than $\pm$0.3%yr-1 at 20–40km for all observations. Aura MLS with 8 yr of observation also shows drifts that are comparable to those from the long-term data sets at all stations.” I disagree with these statements. Looking at Fig. 4 there are larger drifts of MLS/Aura with several lidar stations. They may of course – as you suggest – be due to issues with the lidar measurements, but this needs to be

demonstrated.

Page 511, caption Fig. 5: "The error bars correspond TO the .."

Page 514, caption Fig. 8, line 2: remove extra space after “(left panel)”

Page 499, line 13: “[Data].“ ?

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