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Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on “Long term changes in the upper stratospheric ozone at Syowa, Antarctica” *by* K. Miyagawa et al.

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Dear Referee #1,

We thank the referee for their helpful questions and comments. Please see our responses below. We marked the reviewer’s and the author’s comments by RC: and AC:.

RC: 1. Summary This manuscript presents an analysis of long-term variations of ozone in the stratosphere based on Umkehr and satellite measurements at Syowa station in Antarctica. The main message is that ozone values there have declined during the 1980s and 1990s, and have increased slightly since about 2000. Effective equivalent stratospheric chlorine (EESC) curves with ages of air between 5 and 10 years

are compatible with the observed long-term ozone variation, with some differences between Umkehr and SBUV. I think such old ages of air are not unexpected for the Antarctic upper stratosphere. They authors claim that the observed ozone recovery is slower than expected from EESC, and that this is attributable to changes in vortex position and transports However, I was not able to understand their arguments behind this, and I don’t think their presented evidence supports that. In part this may due to a presentation that was not clear and concise to me. Overall I think that the paper presents too much and too detailed information, that is often not relevant. I have had a hard time reading and understanding the paper. There is too much introductory material, too much well known information and references, also too many Figures that are similar and a lot of too detailed information. The main findings, however, are not discussed clearly. There are several Figures with minimal or no discussion. There are major conclusions with little or no supporting evidence. This is not balanced. I think the paper would benefit greatly from a reduction in the number of Figures, omission of much peripheral material, and a focus on the important and new findings. I think the paper needs to be much more clear and concise. In my opinion, major revisions, or a complete rewriting, are required.

AC: Thank you for pointing the lack of the discussion. We have reworked our paper, eliminated some figures and added the discussion of results. Our conclusion is based on the matches of the ozone residuals (CUSUM method) with the two sets of EESC curves shown in new Fig.7b. Unfortunately there are no long-term EESC measurements for the Antarctica’s stratosphere, and therefore, we rely on the modeled estimates of ozone depleting substances (EESC data) to evaluate changes in the Syowa Umkehr ozone time-series. Recently published papers (Laube et al., 2012; Rigby et al, 2013) re-assessed the stratospheric life time of many halogens regulated by the Montreal Protocol and estimated somewhat longer life-time for major ODSs, such as CFC-11, CFC-12 and CH3CCl3. The non-linear relationship between the age of air and fractional release time complicates the assessment and prediction of the EESC concentrations. The authors of both papers found the age of air to be longer than those published by Newman et al (2006) and Ozone assessment (2011), which in turn would affect the EESC curves. Since our data agree less closely with the Polar 2 curve and more closely with Polar 3 curve, it means that the age of air in the upper stratosphere over Syowa most likely corresponds to an age of air longer than 5.5 years suggested by the 2010 WMO ozone assessment for the polar regions. However, both papers emphasize that the FRF and age of air correlations are done under the assumption of the steady-state of the stratospheric transport. Since transport is expected to change in the future according to climate change scenarios (acceleration of the Brewer-Dobson circulation, more wave breaking in stratosphere, re-circulation, etc), the concentrations of the ODS in the stratosphere would also be expected to change over time (i.e.Neu and Plumb, 1999; Ray et al, 2010). We analyze the Syowa data time series as a way to assess the appropriateness of using various EESC curves in prediction of ozone recovery in the Polar stratospheric regions. Laube, J. C., Keil, A., Bönisch, H., Engel, A., Röckmann, T., Volk, C. M., and Sturges, W. T.: Observation-based assessment of stratospheric fractional release, lifetimes, and Ozone Depletion Potentials of ten important source gases, Atmos. Chem. Phys. Discuss., 12, 28525-28557, doi:10.5194/acpd-12-28525-2012, 2012. Rigby, M., Prinn, R. G., O'Doherty, S., Montzka, S. A., McCulloch, A., Harth, C. M., Mühle, J., Salameh, P. K., Weiss, R. F., Young, D., Simmonds, P. G., Hall, B. D., Dutton, G. S., Nance, D., Mondeel, D. J., Elkins, J. W., Krummel, P. B., Steele, L. P., and Fraser, P. J.: Re-evaluation of the lifetimes of the major CFCs and CH3CCl3 using atmospheric trends, Atmos. Chem. Phys., 13, 2691-2702, doi:10.5194/acp-13-2691-2013, 2013 Neu, J. L., and R. A. Plumb, The age of air in a "leaky pipe" model of stratospheric transport, J. Geophys. Res., 104, 19,243 – 19,225, 1999. Ray, E. A., et al. (2010), Evidence for changes in stratospheric transport and mixing over the past three decades based on multiple data sets and tropical leaky pipe analysis, J. Geophys. Res., 115, D21304, doi:10.1029/2010JD014206.

RC: 2. Length of Introduction, Number of References. The introduction is too long. Pages 380 to 394 (=14 pages) are basically introductory material. Pages 395 to 407 (=13 pages) are new material and discussion. So more than half of the paper is a long

and wordy introduction. This should be shortened, say to 4 or 5 pages and should be much more focused. The reference list is too long as well (11 pages of small print compared to 13 pages of new material and discussion). The number of references should be weeded out and shortened.

AC: Thank you for pointing the length of the introduction and references. It was driven by the attempt to collect all relevant material in one place. We re-wrote and shortened the introduction. See attached document.

RC: 3. Critique of statistical trend model results. Section 3.2. "Statistical trend model" and Table 2: It is commendable that the authors went through all these combinations. But there is virtually no discussion of and there does not appear much thought about the presented results. What proxies should go in the fit? What proxies are the best? What about correlations / non-orthogonality between related proxies (e.g. equivalent latitude, heat flux and SAM)? What about overfitting? Each additional proxy will reduce the mean square error, as long as there some orthogonality added, but eventually (after 3, 4 or 5?) additional proxies become meaningless. Why is the Solar-Cycle always in the fit? Should not EESC or HockeyStick be in the fit as well? What about considering temperature as an explanatory variable? Certainly advisable in the upper stratosphere! See also Fig. 8.

AC: We checked all proxies for correlations and finalized the set that has no correlation, but also helps to reduce variance in the residuals of the model fit. We re-did a lot of analysis in order to find the best set of proxies. We found that separation of data analysis into spring and summer helped to define the best set of proxies that relates to different processes specific for each season (see new Table2). Solar Cycle is required to address ozone changes in the long-term time series in both stratosphere (chemical reactions) and troposphere (secondary effects of transport changes). We also show the enhancement of correlation between ozone and SAM index during the high ozone years, when correlation in layers 5 – 10 is significantly enhanced as compared to the low sun activity years. The EESC Polar 2 is used in the statistical model (Equation 1).

It is also used as a reference when results of additional proxies to the model fit are summarized in Table 2. The temperature is not considered as proxy because we do not have full time-record of radiosond temperature data that would be co-incident with Umkehr ozone profiles. Change in ozone–temperature sensitivity to detect reduction in ODS depleting substances is proposed in several papers (i.e. Stolarski et al, 2012). However, it works only at 1 hPa where the effects are only driven by chemistry. Below 1 hPa, the relation is complicated by dynamics. Umkehr data are also noisy (3-5%) in the upper stratosphere, which makes the temperature sensitivity in ozone harder to define. Moreover, radiosond profiles do not extend to the upper stratosphere. At the same time, the MERRA data in the upper stratosphere are not considered of high quality. We investigated correlation between MERRA temperature and ozone at 4 hPa level. We did not find significant changes in the ozone-temperature ratio and thus could not detect chlorine reduction in the recent decade. In addition, temperature proxy in lower stratosphere is highly correlated with other proxies (EqLat) chosen for trend analysis. The temperature is affected by both ozone change (feedback through chemistry and absorption of radiation) and by dynamics (descends of chemicals into vortex). Therefore, temperature proxy is not helpful for separation of the ODS or climate related changes in Syowa Umkehr ozone profile data.

RC: In Table 2 there is no R (contrary to line 301), only RMSD (in what unit? DU?). From table 2 there is no way to assess how good the fits are. Changes in RMSD between different fits seem to be marginal, and may not be significant.

AC: We changed table 2 to show reduction in explained variance by proxies (%).

RC: What lags are used in the 2nd part of Table2? There are 4 proxies but only one lag? Lags with respect to QBO proxies are meaningless, because the different level winds have "random" phase-differences to the physical mechanism, i.e. QBO related wave and transport anomalies.

AC: Attached file lists lag for three proxies. Only SOR, QBO, and AOD have lag determined in each layer, other proxies do not have lag.

RC: Lags of 14 to 24 against ENSO or heat-flux seem very large and unphysical. What would be a plausible mechanism? I suspect that at these large lags these proxies pickup random variations, or variations that are due to something else.

AC: yes, we agree with this comment. We removed the lag in ENSO and Heat Flux proxies. We also found that the Heat Flux at 10 hPa and EqLat 520 K are not highly correlated. In the spring, when heat flux values are significant, the heat flux is anti-correlated with the size of the polar vortex. When the Heat flux is enhanced, it leads to warmer temperatures in the polar vortex, which shrinks the area of the polar vortex. On the other hand, as spring advances (i.e. Sept>Oct>Nov) the polar vortex is rarely centered over the South Pole and a wave 1 exists. It suggests, that the chances for Syowa to be under the depleted ozone decrease as Spring advances and the ozone hole wobbles. So at the station location, the zonal mean heat flux will not necessarily correlate well with the station's EqLat. We find that the variations in EqLat at Syowa are likely more closely related to the vortex shape or position, while its size (area) is better defined by the Heat Flux. Therefore we use both proxies to explain ozone variability over Syowa.

We re-analyzed data to look into the contribution of individual proxies separately for the summer and spring seasons. Explained variance in ozone in layers 1, 2+3, 4 and 5 is increased during spring when ENSO is added to the statistical model composed of Solar, QBO and AOD (see New Table 2). Further improvement (from 57 to 62% in layer 5 and from 44 to 50 % in layer 6) is found when model includes other variables (SOR, QBO, AOD, EqLat 850 K and ENSO). However, at the end, most of the improvement in explained variability is found when SAM or HF10 proxy is used in place of the ENSO. At the same time, summer ozone variability is improved by including ENSO into model (2.4 vs 1.1) in layers 2+3, 4 and 5. It is also similar to the results of the fit with EqLat850K in place of ENSO, which could mean that EqLat 850K represents variability that is driven by ENSO.

RC: Fig. 1: I would much prefer to have the proxies scaled, so that the reader can see the size of the ozone variation attributed to the proxy. Should the annual cycle not be removed from the heat flux?

AC: We have produced Table 2 that summarizes reduction on variance caused by each proxy separately and in combination with others. We use seasonal Heat Flux proxy in seasonal statistical trend model.

RC: Also: It looks like there is really no data before 1988. So fitting two 11-year solar cycles only might be asking for too much.

AC: There are about 41 profiles that had been collected prior to 1988. The use of this earlier data helps to constrain the statistical model fit and helps with the solar cycle correction for trend analysis.

RC: I think this entire section needs much more discussion of the results. It should also come up with a clear recommendation for the best set of predictors. Maybe only discuss that set, and why it is used. Right now there is no focus at all, and the reader is left alone and in the dark.

AC: We performed more analysis and now the statistical model has been changed to use only proxies that improve the fit. See the new version of Table 2. We have also added discussion to the text to explain how the proxies were chosen (also see above response to the question about Heat Flux contribution to ozone variability at Syowa station). As the reviewer correctly points out, the contribution of the ENSO proxy was not significant as compared to other listed proxies. We find the contribution of Heat Flux to ozone variability comparable to the EqLat850K in the springtime season and only in the lower and middle stratosphere, According to our analysis of ozone variability in the summer season, we find that Solar, QBO, AOD and EqLat explain 2-35 % of the variability, whereas EqLat at 520 K is preferred to fit time series in layers 1 and 2+3, whereas for layers 5, 6, 7 and 8, EqLat at 850 K is a better proxy. It is also interesting to note that ozone variability in layer 2+3 and 4 during summer time is defined by

Solar, AOD, QBO, EqLat 850 K and SAM proxies. Layer 7 and 8 appears to have additional improvement in the explained variances when Heat Flux proxy is used. On the other hand the improvement is similar when SAM is used in place of Heat Flux. Therefore, it is inconclusive results and thus we cannot say what exactly attributed to the improvement of the fit. We also have to notice that Heat Flux does not have large variability in summer time. During spring, stratospheric ozone (layers 2+3, 4, 5, and 6,) is explained by major contributions from Solar, QBO, AOD, EqLat 850K and Heat Flux , whereas ozone in layers 1, 7, 8 and 8+ requires all the above, but Heat Flux proxy. The best fit with the statistical model was obtained by applying time-lag for SOR, QBO, and AOD proxies (See Table2b and 2c) and no lag to other proxies.

Here are some findings conclusions from working on Syowa dataset analysis. 1) We find it very important to match dates between proxies and observed ozone (the same date proxy works better than monthly averaged proxy that includes all days). It is very important for analysis of the station data where variability is very high, such as in spring time at Syowa. 2) The reduction in variance can be marginal, which makes the method of finding the best lag estimate less robust. 3) Ideally, for each proxy there should be a physical mechanism that connects the variability in proxy to variability in ozone. For example, we do not know the mechanism that would correlate ENSO to the variability in the upper stratospheric ozone. However, we could find reference in the literature that relates effects of ENSO with the middle, lower stratosphere in tropics (Randel et al, 2009, Calva et al, 2010). Since tropics are connected to the Polar regions through Brewer-Dobson circulation, we can expect that ozone in the Polar stratosphere can be correlated to the ENSO variability. Since ENSO is correlated to ozone in the lower/middle stratosphere, to the extent that ozone in the middle/lower stratosphere is correlated to that in the upper stratosphere, then ENSO should be correlated to ozone in the upper stratosphere. Therefore, part 2) makes part 3) extremely important for selecting explanatory parameters for the best model fit.. 4) The EqLat proxy is useful, since it helps to separate the chemical and dynamical source in ozone variability. However, it does not explain what physical mechanism influences the dynamical variability of the atmosphere in the vortex and over Syowa. 5) It also means that EqLat proxy can be possibly related to SAM, ENSO or any other dynamical proxy, and while we do not know the exact mechanism yet, we would like to point out the high correlation of EqLat to ozone variability in Polar regions.

RC: 4. Critique of section 3.3. EESC trend curves. Section 3.3: A very lengthy discussion of Fig. 2. Otherwise nothing new. Should be shortened, the references are there. Essentially EESC is just one other proxy. Ages of air in the Antarctic upper stratosphere between 5 and 10 years are what I would expect. What are the error bars and interannual variations on age-of-air? Substantial I would think!

AC: Thank you for suggestions regarding the EESC discussion. We removed some text and added explanations regarding the ages of air and uncertainty of current estimates. The Polar 2 curve is simulated by the Goddard automailer program, and is based on the current state of knowledge (WMO Ozone assessment, 2011) for the transport of Halogens from tropics to high latitudes and the release of chlorine and bromine radicals from halogens, which is determined by their chemical life-time and fractional release factors (FRF). Recently published papers (Laube et al., 2012; Rigby et al, 2013) re-assessed the stratospheric life-time of many halogens regulated by the Montreal Protocol and estimated somewhat longer life-times for major ODSs, such as CFC-11, CFC-12 and CH3CCl3. The uncertainties also come from the fact the meridional transport is assumed to be steady-state and does not include the recirculation of air-masses back into tropical stratosphere (Neu and Plumb, 1999; Ray et al, 2010). This changes the distribution of the age of the air and the release fractions of chlorines and bromines compounds into stratosphere. However, no observations of EESCs are available in the upper stratosphere in the vortex region, and therefore the age of air distributions are hard to characterize. We use the Syowa ozone time series to address the uncertainties in the EESC curve and the need for measurements. More importantly, there is also great need for quantification of the transport processes in the models that predict future levels of the EESCs in the upper stratosphere in the Antarctic.

RC: 5. Critique of section 3.4. Umkehr vs. SBUV data. I find this section very lengthy and very confusing. The authors compare two types (zonal mean vs overpass) of two different SBUV V8.6 products (NASA, MOD without inter-satellite adjustments vs. NOAA, with inter-satellite adjustments). It looks like the authors refer to this as ZM vs OP and MOD vs IS, respectively. To me, text and figures of this section. do not always agree. To me, the authors do not report a clear and consistent picture. I don't understand the authors conclusion from Fig.4a: They way I see it ZM-IS shows the same difference to MOD-ZM (gray squares, dotted fit line) as it does to MOP-OP (colored dots, solid fit-line). To me that indicates that OP-MOD and ZM-MOD are about the same, and the jump comes from the inter-satellite adjustments (IS vs MOD). Fig. 4b also shows to me that there is no significant temporal change or jump between zonal mean and overpass, for two latitude bands. However, the authors conclude that the jump is due in changes of vortex position, resulting in a change of OP vs ZM (page 396 line 20 and after, page 380 line 19 to 21). Either the plot or labeling are wrong, or the authors' conclusion is wrong. Note that already Fig. 3 (rightmost panel) indicates a jump between N7 to N11 and N16 to N18 (IS version?), consistent with the jump of the colored dots in Fig 4a. In summary, I don't see the need to go to overpass data. I disagree with the authors conclusion that there is a significant change over time between zonal means and overpass data. Instead I think that there is a significant change over time between MOD and IS data. The use of both IS and MOD SBUV data confuses the reader (at least me). Only one set should be used! After all this is a paper about Umkehr data!

AC: Since trend analyses are often done using zonal mean data, we wanted to emphasize that in the proximity of the polar vortex the use of zonal mean data is often confusing, and makes interpretation of trends difficult. We found that the MOD V8.6 OP dataset for Syowa agreed better with Umkehr than the ZM data. We also took a close look at the differences of the adjusted SBUV zonal product (ZM) and the unadjusted product (IS) and confirmed the reviewer's comments for section 3.4 that the differences between the overpass data (MOD OP) and the adjusted zonal data (ZM) arise more from the adjustment procedure than from the representativeness of the zone for the overpass. We have attached a plot showing the differences between the adjusted and unadjusted zonal products. It is similar to Figure 4a in the document. It is generally recommended that the adjusted product be used in the 50N - 50S regions, and with extreme care elsewhere. Unfortunately the zonal mean product does not work well for the latitude of Syowa. Therefore, we decided to remove Fig. 4 and the discussion of it from the paper. (see Fig. A)

RC: 5.1 Critique of Fig. 5 and Table 3. Fig. 5 and Table 3: Is that really necessary for the message of the manuscript? There is no discussion at all of the 9 time 15 = 135 numbers presented in Table 3. What is the point of presenting them? For me this is symptomatic for the manuscript: Bombarding the reader with largely meaningless information, not discussing it, and then moving on to more information. Again Table 3: If the slope is negative, then the R must be negative as well! Clearly, there are months (with negative slopes!, especially after 2001) where SBUV and Umkehr data have nothing to do with each other. One, or both of them must be wrong. All these slopes seem to suffer from the fact that the applied linear regression assumes no error in the x-coordinate (SBUV-MOD data). However, each data point has errors in both coordinates. What Table 3 does show to me is that comparison of Umkehr and SBUV data is only meaningful on multi-monthly means or annual means. When that is done, the difference between all SBUV days and matched only SBUV days is actually fairly small (compare top and bottom 3 lines of Table 3).

AC: Thank you for pointing out the lack of the discussion of result presented in figure 5 and table 3. We conducted additional analysis of ozone with Umkehr and SBUV by using averages for spring and summer seasons. It does reduce the errors that arise from differences in the temporal and spatial sampling. A new summary Table 3, and new Figure 3 are added to show seasonal comparisons for three time periods, and also to test the temporal correlation of the data.

RC: 5.2 Fig.6 Fig. 6, top of page 398. How were QBO and solar cycle removed? On

the basis of monthly means, or seasonal means, or annual means? The ENSO pattern in Fig. 6 looks very different from the ENSO pattern in Fig. 1. Why? Why were SAM, EQ-Lat Heat-Flux proxies not removed (after all the discussion and results from section 3.2)? 2002 was the 1st antarctic polar vortex split. It should be treated with care, and maybe removed for some of the analyses. Was 2002 an ENSO effect? I doubt it. Then you are left with positive ozone anomalies in one ENSO (1988), negative anomalies in one ENSO (1998), and nothing in the 3rd ENSO (2009).

AC: SOR, QBO, AOD and ozone are monthly averages after the proxies are matched by the date with Umkehr measurements. ENSO is a combination of El Nino and La Nina. ENSO timing and ranking information is found at the http://www.esrl.noaa.gov/psd/enso/mei/rank.html. In 1986-1987 there was a strong El Nino, but there was a short La Nina at the end of 1988. Figure 6 showed a large positive increase in 1988. We agree with the reviewer that 2002 was a very anomalous year in the vortex and ozone development. Therefore we removed 2002 data from ozone trend and CUSUM analysis. In addition, we decided to remove Fig. 6 from the paper as it appeared unnecessary for the discussion.

RC: 6. Critique of section 4. Section 4.1, 4.2, Figure 7: Again, I find the text lengthy and confusing. I wonder if results would be any different if the data from Fig. 6 (only solar and QBO removed) were used? Would it not be better and clearer for the reader to just have Fig. 7 and not Fig. 6. I think there is a lot of redundancy between these two Figures, and the differences are marginal, and more confusing than enlightening. Even with Fig. 7: There are 24 panels in the Figure. Are they all necessary? I am pretty sure that the narrow confidence interval in the right CUSUM panels of Fig. 7 is wrong and the results are very dependent on the end-year: The large positive anomaly of 2002 (vortex- split!!) brings the CUSUMs to very high values right away. So the CUSUMs start "outside" of the confidence interval right away!! What happens if 2002 is removed? Or if 2002 is included in the linear trend estimation. What happens if the very high positive anomalies early in the record (when the Umkehr data are quite sparse and uncertain compare Fig. 1, Fig. 5) are removed? Then the slopes of the linear fits become smaller, and the CUSUMs don't become significant before 2005. This should be critically discussed by the authors! They should not just report sheer numbers and take them at face value. I don't think that Fig 7. supports the authors claim (e.g. page 380 lines 19 to 21, page 406 lines 9 to 20) , that observed ozone recovery is slower than expected (blue EESC curves, 5.5 years age-of-air), and is closer to the green EESC curve (10 years age-of-air).

AC: Thank you for pointing the issue with figures and discussion. We removed Fig. 6 and modified the CUSUM analysis presented in Fig. 7. We removed data from 2002 to eliminate large residual around 2001. These changes shifted the residuals closer to the Polar 2 curve at the beginning of 2000-2010 periods. This conclusion is based on comparisons of the ozone residuals (CUSUM method) with the two sets of EESC curves shown in Fig.7b. The cumulative residuals of ozone in the upper stratosphere do not seem to follow the either the Polar 2 or Polar 3 curves after 2001, which is the turning point for the EESC concentrations estimated for the Polar region. From the updated CUSUM analysis of ozone residuals in layer 8 during both spring and summer season, it appears that the turning point in the ozone time-series could be few years later than 2001. Also, the rate of ozone changes after 2001 is faster than predicted by the Polar 2 curve. Therefore, the recommendations for the EESC curve for the polar stratosphere should be re-evaluated for prediction of ozone recovery over Antarctica. We also performed more statistical analysis of Syowa data, and now the statistical trend model contains only proxies that significantly improve the fit. We have limited presentation of trend results to spring and summer. We tested robustness of the trend derived using Umkehr measurements using three periods, such as 1977-2001, 1988-2001, and 1982-2001 (and also using the 24 hour matching criteria for Umkehr and SBUV data). We find that though there are only 41 data profiles available at Syowa station during the 1977-1988 time period, the fit becomes more robust when time series are extended to include these infrequent but very important data. We find that difference in derived trends based on three periods is insignificant. However, the

uncertainties of the trend are increased when shorter period of time is used for trend analysis. Therefore, we prefer to use 1977-2001 time period for trend analysis.

RC: 7. Critique of Section 4.3. Section 4.3 is a whole new discussion! Maybe a separate paper? The reader should be prepared for it. Fig. 8 indicates that temperature might be a better proxy than ENSO or SAM in Equation 1, or an additional proxy. Clearly this should be tested! In November homogeneous photo-chemical equilibrium is reached very quickly in layers 8+, and temperature very directly affects ozone through the temperature dependence of gas-phase chemical reactions (mostly $O_3 + O$! $2O_2$). If these proxies are included, how would the ozone recovery look like?

AC: unfortunately Syowa station does not have temperature record above 10 hPa (meteorological balloons do not reach the upper stratosphere). Only MERRA data are available, but again it is not a co-incident measurement, but rather assimilation of various satellite measurements that are not of high quality. The idea was to test data for several time periods to see of ozone/temperature sensitivity would change, which according to the paper by Stolarski et al (2012) would indicate the change in the chlorine loading, Unfortunately, the uncertainties in both ozone measurement and temperature from MERRA do not allow to detect this type of change. Stolarski, R. S., A. R. Douglass, E. E. Remsberg, N. J. Livesey, and J. C. Gille (2012), Ozone temperature correlations in the upper stratosphere as a measure of chlorine content, J. Geophys. Res., 117, D10305, doi:10.1029/2012JD017456.

What is also confusing is the change of the ozone and temperature slopes in November. Analysis show that SBUV V8 MOD ozone and temperature are ant correlated (as expected from the chemistry in the upper stratosphere) and the relation is defined by the negative slope (-0.08 DU K-1), which is similar in September, October, and November. At the same time Umkehr ozone produces very similar slope to the SBUV results in September and October (-0.10 DU K-1). However, the slope in Umkehr data in November (-0.16DU K-1) appears to have a larger slope against the temperature data. All temperature data are from MERRA analysis at 2 hPa over Syowa location. We are not sure what causes this difference. At this high altitude, the MERRA data have large uncertainties. Also, particularly in the SH where there are fewer ground-based data to assimilate, changes in the data inputs to the assimilation over the period of the MERRA reanalysis could be significant. It is important mentioning the possibility of issues with consistency of the MERRA temperature record over the time period as something that “cannot be ruled out”.

RC: After hardly discussing Fig. 8, very complex Fig.9 is thrown at the reader. Fig. 9 is barely discussed in 8 lines (page 403 line 14 to 22). What is the message from Fig. 9? Let the reader figure it out!

AC: We agree with the reviewer that Figure 9 does not fit into the discussion of the paper and therefore we removed it.

RC: Same thing for Fig. 10. I don’t understand it. It is not discussed clearly. I cannot see a message. Probably it should be omitted. These Figures need to be explained and discussed to the reader in much more detail. Or they should be omitted. Do they introduce new findings? I don’t know. Cooling of the lower Antarctic stratosphere especially in spring/summer is well known, as is the slight warming above. Cooling of the upper stratosphere is also well known. What do Figures 9 and 10 show?

AC: we do not agree with the reviewer about Figure 10. This figure demonstrates the complexity of statistical modeling of ozone distribution over Syowa and attribution of its variability to specific proxies. Figure 10 points out that correlation between ozone and SAM proxy is not very clear when all years are used in data analysis. However, during the years with high Solar activity the correlation is very high during the spring season and at multiple levels through the stratosphere. We believe that is an important result of the analysis.

RC: Figure 11 and it’s discussion: Is this not the same as Fig. 7 and / or Fig.6? What is new here? Is this Figure necessary? Where is the discussion of Fig. 11 in the text? There is one line (page 406 line 9/ 10) Page 404 line 27 to page 405 line 3, page 406

line 9/10: I fail to see that. To me, the authors have not shown any evidence for this. What if the last Umkehr data point in Fig. 11 is omitted / off? The SBUV data are above the blue curve! Are they wrong? Why are they discounted, and why is that not even mentioned?

AC: Thank you for pointing out the lack of explanation. We removed Figure 11 as being redundant and unnecessary for the discussion, and added more explanation to the text. We have removed Figs. 6, 9, and 11.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 379, 2013.

Fig.A

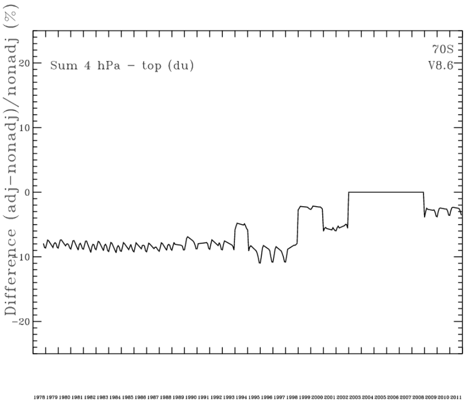
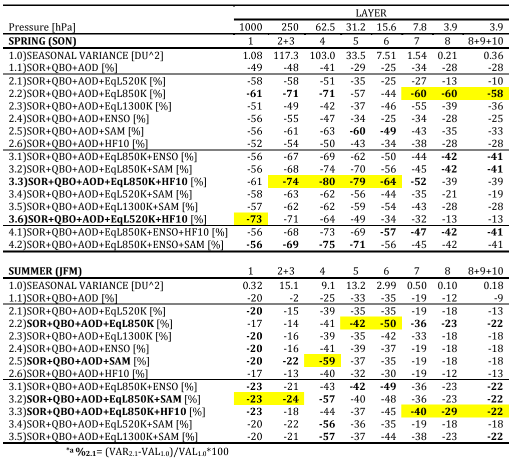


Fig. 1. Fig A



	LAYER							
Pressure [hPa]	1000	250	62.5	31.2	15.6	7.8	3.9	3.9
SPRING (SON)	1	2+3	4	5	6	7	8	8+9+10
1.0)SEASONAL VARIANCE [DU^2]	1.08	117.3	103.0	33.5	7.51	1.54	0.21	0.36
1.1)SOR+QBO+AOD [%]	-49	-48	-41	-29	-25	-34	-28	-28
2.1)SOR+QBO+AOD+EqL520K [%]	-58	-58	-51	-35	-25	-27	-13	-10
2.2)SOR+QBO+AOD+EqL850K [%]	**-61**	**-71**	**-71**	-57	-44	**-60**	**-60**	**-58**
2.3)SOR+QBO+AOD+EqL1300K [%]	-51	-49	-42	-37	-46	-55	-39	-36
2.4)SOR+QBO+AOD+ENSO [%]	-56	-55	-47	-34	-25	-34	-28	-25
2.5)SOR+QBO+AOD+SAM [%]	-56	-61	-63	**-60**	**-49**	-43	-35	-33
2.6)SOR+QBO+AOD+HF10 [%]	-52	-54	-50	-43	-34	-38	-28	-28
3.1)SOR+QBO+AOD+EqL850K+ENSO [%]	-56	-67	-69	-62	-50	-44	**-42**	**-41**
3.2)SOR+QBO+AOD+EqL850K+SAM [%]	-56	-68	-74	-70	-56	-45	**-42**	**-41**
3.3)SOR+QBO+AOD+EqL850K+HF10 [%]	-61	**-74**	**-80**	**-79**	**-64**	**-52**	-39	-39
3.4)SOR+QBO+AOD+EqL520K+SAM [%]	-58	-63	-62	-56	-44	-35	-21	-19
3.5)SOR+QBO+AOD+EqL1300K+SAM [%]	-57	-62	-62	-59	-54	-43	-28	-28
3.6)SOR+QBO+AOD+EqL520K+HF10 [%]	**-73**	-71	-64	-49	-34	-32	-13	-13
4.1)SOR+QBO+AOD+EqL850K+ENSO+HF10 [%]	-56	-68	-73	-69	**-57**	**-47**	**-42**	**-41**
4.2)SOR+QBO+AOD+EqL850K+ENSO+SAM [%]	**-56**	**-69**	**-75**	**-71**	-56	-45	-42	-41
SUMMER (JFM)	1	2+3	4	5	6	7	8	8+9+10
1.0)SEASONAL VARIANCE [DU^2]	0.32	15.1	9.1	13.2	2.99	0.50	0.10	0.18
1.1)SOR+QBO+AOD [%]	-20	-2	-25	-33	-35	-19	-12	-9
2.1)SOR+QBO+AOD+EqL520K [%]	**-20**	-15	-39	-35	-35	-19	-18	-13
2.2)**SOR+QBO+AOD+EqL850K** [%]	-17	-14	-41	**-42**	**-50**	**-36**	**-23**	**-22**
2.3)SOR+QBO+AOD+EqL1300K [%]	**-20**	-16	-39	-35	-42	-33	-18	-18
2.4)SOR+QBO+AOD+ENSO [%]	**-20**	-16	-41	-39	-37	-19	-18	-18
2.5)**SOR+QBO+AOD+SAM** [%]	**-20**	**-22**	**-59**	-37	-35	-19	-18	-18
2.6)SOR+QBO+AOD+HF10 [%]	-17	-13	-40	-32	-30	-19	-12	-13
3.1)SOR+QBO+AOD+EqL850K+ENSO [%]	**-23**	-21	-43	**-42**	**-49**	-36	-23	**-22**
3.2)**SOR+QBO+AOD+EqL850K+SAM** [%]	**-23**	**-24**	**-57**	-40	-48	-36	-23	**-22**
3.3)**SOR+QBO+AOD+EqL850K+HF10** [%]	**-23**	-18	-44	-37	-45	**-40**	**-29**	**-22**
3.4)SOR+QBO+AOD+EqL520K+SAM [%]	-20	-22	**-56**	-36	-35	-19	-18	-18
3.5)SOR+QBO+AOD+EqL1300K+SAM [%]	-20	-21	**-57**	-37	-44	-38	-23	**-22**

^{*a} **$\%_{2.1}$**$= (VAR_{2.1}-VAL_{1.0})/VAL_{1.0}*100$

Fig. 2. table 2a

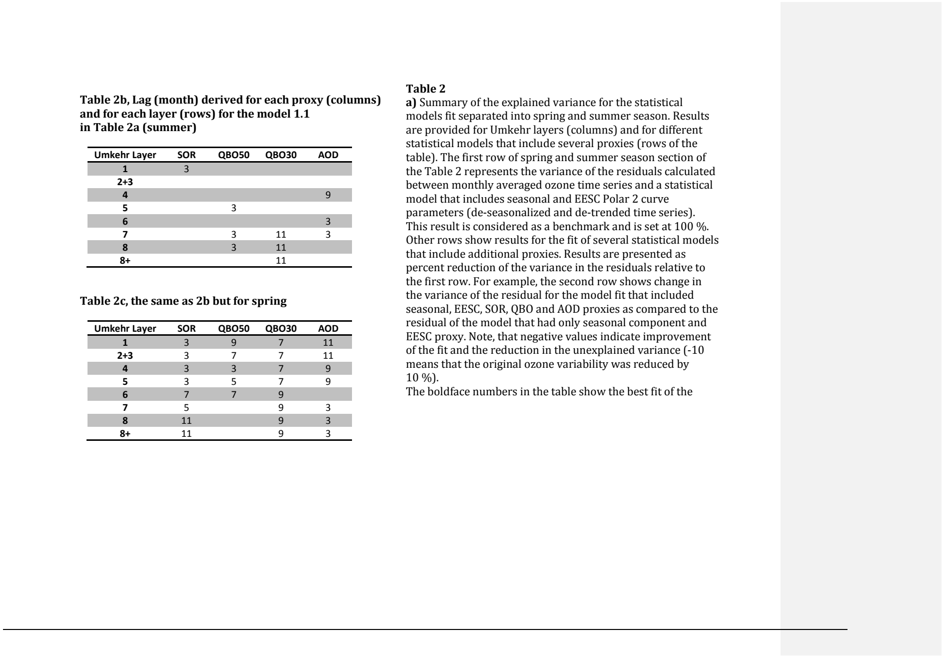


Table 2b, Lag (month) derived for each proxy (columns) and for each layer (rows) for the model 1.1 in Table 2a (summer)

Umkehr Layer	SOR	QBO50	QBO30	AOD
1	3			
2+3				
4				9
5		3		
6				3
7		3	11	3
8		3	11	
8+			11	

Table 2c, the same as 2b but for spring

Umkehr Layer	SOR	QBO50	QBO30	AOD
1	3	9	7	11
2+3	3	7	7	11
4	3	3	7	9
5	3	5	7	9
6	7	7	9	
7	5		9	3
8	11		9	3
8+	11		9	3

Table 2
a) Summary of the explained variance for the statistical models fit separated into spring and summer season. Results are provided for Umkehr layers (columns) and for different statistical models that include several proxies (rows of the table). The first row of spring and summer season section of the Table 2 represents the variance of the residuals calculated between monthly averaged ozone time series and a statistical model that includes seasonal and EESC Polar 2 curve parameters (de-seasonalized and de-trended time series). This result is considered as a benchmark and is set at 100 %. Other rows show results for the fit of several statistical models that include additional proxies. Results are presented as percent reduction of the variance in the residuals relative to the first row. For example, the second row shows change in the variance of the residual for the model fit that included seasonal, EESC, SOR, QBO and AOD proxies as compared to the residual of the model that had only seasonal component and EESC proxy. Note, that negative values indicate improvement of the fit and the reduction in the unexplained variance (-10 means that the original ozone variability was reduced by 10 %).
The boldface numbers in the table show the best fit of the

Fig. 3. table 2b,2c