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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.

Anonymous Referee #1

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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.

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.

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13, C8–C15, 2013

Interactive Comment

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 Hockey-Stick be in the fit as well? What about considering temperature as an explanatory variable? Certainly advisable in the upper stratosphere! See also Fig. 8.

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.

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.

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.

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?

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.

ACPD

13, C8–C15, 2013

Interactive Comment

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.

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!

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

ACPD
13, C8–C15, 2013

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!

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

ACPD
13, C8–C15, 2013

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).

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).

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

ACPD
13, C8–C15, 2013

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).

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 $O_3 + O \rightarrow 2O_2$).

If these proxies are included, how would the ozone recovery look like?

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!

ACPD
13, C8–C15, 2013

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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?

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?

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

ACPD
13, C8–C15, 2013