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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Stratospheric ozone trends and variability as seen by SCIAMACHY during the last decade" *by* C. Gebhardt et al.

Anonymous Referee #2

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1 Overall rating

The manuscript presents a fairly standard multiple linear regression analysis of ozone profile variations recorded by SCIAMACHY limb measurements from August 2002 to December 2011. The focus is on the linear trend term, which shows some unexpected large decadal changes. A negative decadal change, up to -20% per decade, is found in a layer around 34 km in the tropics, and is largely confirmed by MLS and OSIRIS data over roughly the same time period. In SCIAMACHY data this negative decadal change extends, to a lesser degree, to much of the Northern Hemisphere, but this is not fully corroborated by the MLS data.

In the lower stratosphere, below 30 km, SCIAMACHY, MLS, OSIRIS and SHADOZ all show a large ozone increase, up to 10% or 20% per decade. Below 20 km, SCIAMACHY shows a smaller increase than the other data sets. Apart from that, the data show a small ozone increase in the 40 to 50 km region (around 5% per decade). Only this small increase is expected from the beginning ozone recovery due to now declining anthropogenic chlorine.

The reported decadal variations near 34 km, and around 20 km, in the tropics (I hesitate to call them trends) are quite large, and are not expected or explained. This is definitely worth reporting, and it is good that the authors are doing that!! The authors mention NO_x changes as one possible cause, but this would certainly have to be confirmed in separate papers.

I also think that, given the large and increasing decadal trend reported below 20 km in the tropics, the authors should not discard ENSO as fast as they do (pg. 11277, lines 24 to 27). The ENSO MEI index, e.g., does show a substantial decadal trend over the 2002 to 2011 period. This might have substantial impacts in the tropical lower stratosphere. It might explain why the authors find a positive trend, whereas other studies are finding negative trends (Randel and Thompson, 2011). ENSO should be reconsidered in the regression at altitudes below 25 km or so. ENSO should also be revisited at least in the discussion/conclusions at the end of the paper.

There is another point, where I think that the presented evidence (Figs. 14 and 15) does not support the authors conclusion (that the ozone column decadal changes shown in these figures are consistent and essentially zero). I would think the opposite: The decadal changes are not consistent, and one decadal change is significantly positive. This part needs to be changed as well, see my suggestions below.

Overall, however, this is an interesting paper. It should eventually be published in ACP, once the substantial revisions suggested here have been done. These revisions would, in my opinion, make the paper better founded, and in large parts shorter and more concise. They would help to bring out the main findings much better.

I commend the authors for the high quality and clarity of their figures. Still, I feel that some Figures should be combined, and others are not needed at all. See also my suggestions below.

2 General remarks and suggestions

Overall, I think the paper is lengthy and wordy. The authors should really go through the entire text with the aim of shortening and removing unnecessary details. I think the paper would benefit substantially. Also, the logical flow of the paper seems to break apart after the discussion of Figs. 5 and 6. After that, there is unfortunate switching forth-and-back between results, discussion of scientific context and possible explanations, more introductory material and additional results (see below).

I also feel that there is a lot of redundancy in Figs. 10 to 13. It would help a lot to remove this redundancy and rearrange the text to have a much clearer logical flow in this half of the paper (see below).

In the introduction, it is good that the authors put their paper into the context of previous work and existing literature. However, I think that they have really overdone this. The introduction should be shortened and crisped up. E.g. omit pg. 11271 lines 15 to 26. An ACP reader of this paper will know most of these things. Shorten the next paragraphs (pg. 11272, 11273).

The reference list is also much too long: 10 pages of references (small font !!), compared to 19 pages of text, i.e. more than one third of the paper. This should be weeded out considerably, by removing at least half, if not two thirds, of the references. See also the unnecessarily long lists e.g. on pages 11271, 11272, 11277.

Further examples to be shortened: much of pg. 11276, 11277. There is no need for

two separate subsections here. QBO, solar cycle and ENSO proxies are standard and have been used in ozone regression analyses for about 20 years.

Pg. 11278, 11279: I think it is necessary to also show a Figure with the tropical time series near 20 km, analogous to Figs. 1 to 4 . This is another region where the decadal change maximizes, and a very interesting region (Randel and Thompson, 2011). It is also a region where the reported large decadal change is quite different from the expectations e.g. of Randel and Thompson (2011). For the lower levels (say 25 km and below), I think ENSO should be considered in the regression, and should be discussed.

From pg, 11280 on, I think the logical flow of the paper falls apart:

After the presentation of Figs. 5 and 6 with the remarkable 2003 to 2011 decadal changes seen by SCIAMACHY in the tropics (up to +10% per decade around 20 km, up to -20% per decade near 34 km), section 5.2 starts a discussion of scientific context and possible explanations. Then (pg. 11281, line 15) follows the presentation of Fig. 7 (=global distribution of the decadal changes), and then again some discussion of the scientific context. After that, sections 6 and 7 are used to establish the credibility of the SCIAMACHY decadal changes by comparing with results from other profiling instruments.

Would it not make much more sense to

1. discuss the global distribution in Fig. 7, right after Figs. 5 and 6.
2. then compare the SCIA decadal changes to the other results in order to establish credibility of the SCIA decadal changes
3. then discuss scientific context and possible explanations of the decadal changes, (including ENSO for the lower levels) and come to the conclusions.

Right now, everything from page 11280 to page 11287 is an unfortunate forth-and-back between presentation of results, discussion of possible explanations, introduction of new data sets, ...

To me, Figs. 10 to 13 give more or less redundant information: Not only SCIAMACHY, but also MLS and OSIRIS show a significant ozone decline from 2003 to 2011 in the tropics around 34 km, and a significant ozone increase over the same time in the tropics below 30 km (also SHADOZ). I think this information could easily be combined in one Figure (and should be combined!), e.g. Figure 12 with an additional curve for SHADOZ (from Fig. 13). I think the additional details presented by Figs. 10, 11 and 13 are really minor and do not warrant three additional Figures. Too much non-relevant details tend to confuse readers. The main point, in my opinion, is already made quite well by Fig. 12.

In my opinion, sections 6 and 7 should also be combined into one section. Both sections do more or less the same thing – compare SCIAMACHY data with other data sets. After that, the conclusions section should discuss the major findings, put them into the scientific context and provide possible explanations. Especially the latter should not be done already on pages 11280 and 11281.

I am not sure at all, what the point of Figs. 14 and 15 is. Do the authors want to show tropical total ozone? If so why? Or do they want to establish credibility of the SCIAMACHY limb decadal changes? If so, I disagree with the authors, that both SCIAMACHY integrated stratospheric ozone and the merged nadir total ozone show no decadal change in the tropics. The nadir decadal change is clearly positive and significant: 0.3 ± 0.1 is significant, even if the ± 0.1 is 1σ only (which should also be clarified). I have actually digitized the integrated limb and nadir time series, and looked at the difference limb – nadir. This difference shows a clear annual cycle and a very significant declining trend ($-0.13 \times 10^{18} cm^{-2}$ with a small 2σ uncertainty ($-0.04 \times 10^{18} cm^{-2}$).

In my opinion, Figs. 12 and 13 give a possible explanation for the smaller integrated limb decadal change: The smaller (or more negative) SCIAMACHY limb profile decadal changes compared to all other instruments below 20 km, and near 34 km (see Figs.

12 and 13!!), result in a smaller, or more negative, integrated column decadal change for SCIAMACHY – exactly what is seen in the comparison of Figs. 14 and 15. I do not concur with the authors conclusion that integrated limb and nadir decadal change are consistent (pg. 11270, lines 14 to 16, pg. 11287, line 24 to 11288, line 2, and pg. 11288, line 26 to pg. 11289, line 6). In fact Figs. 14 and 15 confirm the general picture from the comparison with MLS, OSIRIS and SHADOZ, that SCIAMACHY shows smaller / more negative decadal changes than the other instruments at certain altitudes.

I would suggest to leave out Figs. 14 and 15 and their discussion completely. Minimum, combine them into one Figure, which would be easy. The main point of the paper, however, are the limb profile trends, and the unexpected large decadal changes in the tropics near 34 and around 20 km. So I would leave the column trends out.

3 Detailed points

Abstract / line 2: also give the time period over which the trend was calculated (presumably 09/2002 to 03/2012)

pg. 11270, line 18: I don't think that "the earth ... is shielded by ozone absorption ...$''$ Rephrase, e.g. "The stratospheric ozone layer shields the earth from UVB and UVC radiation in the 240 to 320 nm range. The absorbed energy ...$''$

pg. 11274, line 12: It would also be fair to admit that SCIAMACHY does not measure polewards of 60° in winter. So maybe add "and the lack of SCIAMACHY measurements in the absence of sunlight$''$ after "polar vortex$''$

pg. 11274, line 26: Good!!

Figs. 1 to 4: I think these Figures are good and important. One important panel, however, is missing in all panels (in my opinion). This additional panel should show the linear trend underlaid by the times series with seasonal, QBO, and solar terms removed. Since the rest of the paper largely discusses the trend, I feel it would be fair and very necessary to clearly show the trend part of the time series as well. I am still wondering where these large decadal trends come from – and other readers will wonder too. It would be very important to see the underlying data, i.e. time series with seasonal, QBO and solar terms removed.

Also Figs. 1 to 4: I think adding a title line giving the altitude and latitude band to each Figure, and labeling each panel (e.g. with data+fit, residual, seasonal, QBO, solar) would help the reader a lot. I realize that this information is also given in the legend, but having it in the Figure would help.

Pg. 11280, line 25 to page 11281, line 13: Ok, indeed these investigations show ozone decreases in a "narrow$''$ layer around 35 km due to increasing N_2O / NO_x. However, the estimated ozone trends are 0.5% per decade maximum, NOT 5 to 20% per decade as seen here. This factor 10 or more difference needs to be mentioned and discussed. Also, I don't see the narrow maximum response near the equator for 2005 to 2095 in Fleming et al. Fig. 4. I see the same shape as for 1979 to 1996, but a weaker response. This should be checked. The text should be corrected.

Pg. 11281, lines lines 25 to 27: if this is true for $60°$ to $70°$, where clouds and tropopause are below 10 km, it must be true even more in the tropics, where clouds may reach up to 16 or 18 km.

Pg. 11282, lines 9 to 15: Which Figure of Stiller et al. are the authors referring to? Please clarify. I would expect to find an age-of-air pattern somewhat similar to the decadal change pattern of the authors Fig. 7. However I did not find that in the Stiller et al. paper. Figs. 10/11 of the Stiller et al. paper, e.g., show large negative age-of-air decadal changes near 35 km in the tropics and around $50°$N (reminiscent of the current Fig. 7), but they also show a large positive decadal change near 35 km around $25°$N and $50°$S – in contrast to the current Fig. 7. So the current statements should be qualified a bit. Overall, my impression is that there is a few ideas, but no really

very plausible explanation for the current large decadal changes in certain layers. This should probably stated in the text.

Pg. 11284, lines 19 to 21: I would disagree, comparison with OSIRIS and SHADOZ indicates that the higher MLS decadal changes are correct (Figs. 10 to 13, MLS +20% decade near 18 km, OSIRIS +15 to +20% per decade near 18 km, SHADOZ + 20% per decade near 16 km), and the lower SCIAMACHY decadal change (0% per decade near 17 km) is probably not correct.

Pg. 11285, line 14: As does SHADOZ !!

pg. 11285 line 15: As said above, I would suggest to combine sections 6 and 7 into one section. The section title is not correct as well: MLS and OSIRIS are just as independent from SCIAMACHY as SHADOZ is. And I would expect that the GOME/SCIA/GOME2 combined nadir decadal change is less independent from the SCIAMACHY limb decadal change, since both use them same instrument, ancillary data, . . . MLS, OSIRIS and SHADOZ are all completely different instruments from SCIAMACHY.

Pg. 11285 lines 23, 24: I would change the 30 km to 35 km, as most balloons will reach above 30 km (except maybe for the polar winter stratosphere). Apart from balloon-burst, evaporation and/or freezing of the wet-chemical sensing solution used in the sondes also limits the reachable altitude range to ≈35 km (triple point of water is at 6 hPa). This could be mentioned as well.

Pg. 11288, line 11: I think it would be fair to add that below 20 km, SCIAMACHY shows smaller / more negative decadal changes than the other instruments.

4 References

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 11269, 2013.