RESPONSE TO REFEREE#2 COMMENTS

Review of "Antarctic ozone loss in 1989–2010: evidence for ozone recovery?" by Kuttippurath et al., 2012

It was a good review that critically examines various aspects of the objective of this paper. We thank the effort taken by the Referee and the time he/she spent towards the improvement of this paper.

We have carefully attended all the comments. For this revision, we had to revise ALL FIGURES, TABLES and related texts, and Section 4 on ozone trends. The analyses of ozone trends now start in 1979. Please find the answers to specific comments below. However, please note that we have removed the sections on "Annual ozone minima", "Ozone loss above stations", and "Ozone loss and VPSC" to present the main new results . Therefore, related comments and replies are excluded herein. Thank you.

GENERAL COMMENTS

1# This is a potentially interesting piece of research that aims to address past changes in ozone over Antarctica, but ultimately is a disappointing paper. The many errors in the manuscript, some serious and many minor, detract from the potential scientific quality of the work. This paper makes some very serious claims that contradict the existing scientific understanding of ozone changes over Antarctic. For example, the authors state that the trend in Antarctic ozone levels prior to 1996 cannot be completely explained by the reduction in ozone depleting substances (I am assuming they mean by the increase in ozone depleting substances - but that's another matter) in contradiction of Chapter 2 of the 2010 WMO/UNEP ozone assessment as well as hundreds of other studies that have demonstrated that the decline in Antarctic ozone prior to 1996 is entirely explicable by increases in ozone depleting substances. For this claim to be correct, the authors need to demonstrate why all previous analyses (many of which they were coauthors on) are wrong. It is not possible that both those papers as well as this paper are correct. What is particular disturbing is that the result on which this claim is based is contradicted by a result obtained by a different method in this same paper.

The slightly higher ozone trends estimated using the PWLT regression compared to the trends estimated using the EEASC based regression for the 1997—2010 period (i.e. ozone increase after 1997) are contributed by factors other than the decrease in stratospheric halogen levels (e.g. dynamics), as noted in WMO (2011) for the mid-latitude regions. This was what we meant by the sentence, but misplaced in the previous paragraph that discussed the trends during 1989—1997. Therefore, we would like to emphasise that we do not claim anything that stands in contrast to the existing polar ozone loss theories.

2# I recognize that the first author's first language may not be English with the result that there are many grammatical and more general writing errors in this manuscript. What particularly annoys me though is that there are co-authors on this paper whose first language is English and if they had taken the time to proof read this manuscript it would have (I presume) removed many of these grammatical errors. It should not be the task of the reviewer to correct a manuscript for these sorts of errors.

The English co-authors have proof-read it. Please note that, since this is an international journal, sometimes the time schedule is such that they cannot always perform a necessary second proof-read. Thank you.

SPECIFIC COMMENTS

3# Page 10776, lines 15-21: This doesn't make sense since the space-based instruments do not provide any measure of the ozone loss values, only of the ozone.

Section has been revised. Now the text is "These loss estimates are in good agreement with those estimated from satellite observations". Please find the revised text in Abstract, Lines 14—16

4# Page 10776, line 25: But you have a period of 9-10 years of data. In fact you have 14 years of data. So if you only need 9-10 years of data to derive a trend at the 95% confidence level, why are you only seeing trends at the 85% confidence level?

This was a problem of the selection of the turning point, as the year should coincide with the peak EEASC year. This was not the case for the year 1997. Therefore, we have completely revised the trend analysis with new break point (2000), EEASC data and a new reanalysed ozone data set (the Multi-sensor Reanalysis—MSR). The revised analysis shows that the ozone trends during 2000—2010 are significant at 95% confidence intervals for both EEASC and PWLT based regressions. Please find the revised statements in Lines 16—30

5# Page 10776, line 27: This last sentence of the abstract is not supported by any of the statements or conclusions that precede it in the abstract. Your analyses show that Antarctic mean ozone increased by 1 DU/year from 1997 to 2010. Your analysis provides no indication that that increase will continue, nor that Antarctic ozone will return to 1980 values or that Antarctic ozone will no longer be affected by anthropogenic halogens (the two accepted definitions of ozone recovery). Your abstract needs a much more appropriate sentence (or two) that summarizes what this all means i.e. what is the reader to conclude from your analysis.

We have reformulated the sentence as : "Therefore, our study suggests that Antarctic ozone shows a significant positive trend toward its recovery and hence, leaves a clear signature of the successful implementation of the Montreal Protocol". Please find it in Lines 30—33

6# Note also that a positive trend in Antarctic ozone from 1997 to 2010 says nothing about the recovery of ozone from the effects of halogens. That positive trend may be driven entirely by changes in dynamics. Attribution to causes is an essential part of analysis of ozone recovery.

Yes. This is the reason for the differences in trends estimated between EEASC and PWLT after 2000, as mentioned in Lines 26—28. Please note that including heat flux in the regression accounts for much of the dynamics.

7# After having read the abstract, the main question in my mind was "What of this is new?" My impression is that most of what is said in the abstract is already well known. I think that you need to highlight which of these results are new. In the abstract you also need to motivate this work. You have said nothing about why you did this analysis. What were you aiming to do over and above what has already been done in a number of previous studies?

The main points of this paper are:

1. **The long-term (22 years) ozone loss time series for Antarctic during 1989—2010 (Section 3).**

2. **The long-term (32 years) ozone trends in the Antarctic vortex in the 1979—2010 period using three different data sets classified with three different vortex definitions. In addition, we use two different approaches (PWLT and EEASC based regressions) to find ozone trends (Section 4.3).**

3. **To our knowledge these sorts of analyses using the ground-based and satellite overpass data have not been done before to estimate the Antarctic ozone loss and ozone trend studies. Even if there are ozone loss analyses for individual years (or for a few years together) from various sources, the available ones use different models, measurements, meteorological data and**

vortex analyses. Therefore, it would be very difficult to compare them as they lack coherency and consistency in the analyses.

4. We hope that different studies using various data sets trigger more discussions towards the elucidation of the evolution of Antarctic ozone.

5. Since there is a long-term ozone loss time series in the Arctic from ground-based measurements, such a kind of analysis in the Antarctic is not available until now. We try to fill this void with this new analysis.

These motivate us to do this work and we hope that our analysis is different and new, which offers some new insights into the trends of Antarctic ozone. Please also note that we have removed the sections on "Annual ozone minima", "Ozone loss above stations", and "Ozone loss and VPSC" to present the main new results. Please find the revised Abstract (e.g. Lines 1—8)

8# Page 10777, line 8: It would be good to know to what SZA the SAOZ can measure. Please include that here. Presumably it needs to see some sunlight to make a measurement.

SAOZ makes measurements between SZA 86 and 91°, mentioned in Line 106

9#Page 10777, line 14: I disagree with this statement. I think that the expectation of the recovery of the Antarctic ozone hole in a few decades weakens the need for surveillance of ozone levels over Antarctica. Surely ongoing surveillance would be deemed to be more important if ozone depletion over Antarctica were expected to remain unchanged or to worsen? In fact of the three options (ozone is expected to increase, remain unchanged, decrease) it is under the first where surveillance has least motivation. Most funding agencies realize this.

We have removed the sentence.

10# Page 10778, line 10: I am wondering why you restricted your analysis to 1989-2010 when you have TOMS data going back to 1979?

The revised analyses start in 1979. This has been mentioned in Line 80

11# Page 10778, line 24: After having read your abstract, and being very familiar with the Salby paper, and having reached this point in your manuscript, I am thinking: either you're right and Murry Salby is wrong, or you're wrong and Murry Salby is right. Or you're both wrong. But you certainly can't both be right because you come to different conclusions. So I am interested to see how this will play out in your paper.

Our turning point (1997) selection was not apt for the trend analysis and that corrupted our results. Therefore, we have re-analysed the data. Our new results presented in the revised version confirm those of Salby et al. (2011) for all data sets, as our analyses also show statistically significant positive trends for the 2000—2010 period. Please find the new results in Section 4.3.4 and Table 3

12# Page 10778, line 26: But ozone trends and ozone recovery have nothing to do with each other, no matter which of the two definitions you use for ozone recovery i.e. 1) The return of ozone to 1980 values. 2) Ozone no longer being significantly affected by halogens.

We have removed "from the perspective of ozone recovery" from the sentence. Please find the revised text in Lines 78—85

13# Page 10778, line 27: This is not correct. The Introduction precedes the discussion of the data used for the analysis. It does not follow it.

We have revised the sentence as "The plan of this paper is as follows: the data used for the analyses and the method applied to the ozone loss derivation ...". Please find it in Lines 86—88

14# Page 10779, line 15: Very small compared to what? If you mean compared to the Arctic you should say so. Without a comparison standard it is not possible to make sense of this statement.

Compared to the Arctic. Corrected in Line 101

15# Page 10779, line 17: Where is the support for this statement i.e. 'assures a robust diagnosis'? You haven't provided any analysis that demonstrates that 8 stations are sufficient to robustly estimate the Antarctic area mean total column ozone (if that is indeed your metric - you haven't said anywhere what the N stations are being used to measure).

We had a detailed discussion on these aspects in our previous publication (Kuttippurath et al., 2010), in which we introduced the method and tested the number of stations and their sensitivity to the derived ozone loss. Therefore, to avoid repetitions, we excluded a detailed description of that study here. However, the text has been revised as follows "As the Antarctic vortex is very stable and inter-annual variations in the meteorology are very small compared to those of the Arctic, the selection of the stations is less dependent on estimated ozone loss, as demonstrated in Kuttippurath et al (2010) ". We have also added a new Table and figure to give the details of these stations. Please find the revised text in Lines 99—103, Table 1 and Figure 1

16# I would prefer to see use of the terms random and systematic errors rather than precision and accuracy to be consistent with the recommendations outlined in the Guide to the Expression of Uncertainty in Measurement published by the International Bureau of Weights and Standards (see http://www.bipm.org/utils/common/documents/jcgm/JCGM_100_2008_E.pdf).

Thank you for this guide. We have given the details of the random and systematic errors instead of accuracy and precision as available from the published records for the measurements. Please find them in Lines 113, 115, 117, 124—125, and 131—132

17# Page 10780, line 5: So does that mean that the 0.5% or 1DU random errors on the Dobson measurements are not correct?

According to Basher (1982), the random error of Dobson measurements is of the order of 1% and the total error of 3%. However, the recent comparisons performed by Hendrick et al. (2011) show that the Dobson measurements are dependent of absorption coefficients used for the retrievals and solar zenith angles. Therefore, their study indicates the need of a careful revision of the error values. This has been mentioned in Lines 123—129

18# Page 10780, line 14: But the Nimbus-7 TOMS data go back to November 1979. Why do you only start in 1989?

We have revised the trend analyses with data from 1979 to 2010. Please find answer to 10#. Thank you.

19# Page 10780, 23: It isn't clear to me why you need the GOME, GOME-2 and SCIAMACHY ozone observations when complete coverage for the period is provided by TOMS and OMI, especially given the high bias of the GB data against GOME and SCIAMACHY

We have removed GOME, GOME-2, MLS, and SCIAMACHY from these analyses. Please find the revised text in Section 3.1 and Figures 3 and 4

20# And why didn't you just use one of the combined total column ozone data bases that are available where all offsets and drifts between the different constituent satellite-based data sets have been corrected for?

We have now used an additional data set that has been corrected for all biases, the MSR total ozone data. All the figures and related text are changed accordingly. For instance: please find the new Figure3 and related text in Lines 150—160

21# Page 10781, line 4: By definition biases are systematic, otherwise they would be random errors. So it is not clear to me why this bias could not be corrected for.

The biases were not systematic, as they were random. We have revised the text in Lines 149—150

22# Page 10781, line 6: You have still not defined what you mean by 'GB ozone loss'. What I have in my head is the difference between a ground-based total column ozone measurement and the total column ozone derived from a passive ozone tracer in some model. Now that assumption of mine may not be correct but it is absolutely essential that by this point in the paper I know exactly what you mean by 'GB ozone loss' so please ensure that you define it either here or, preferably, earlier.

The ozone loss from the ground-based/satellite measurements are calculated as the difference between the measurements and a passive tracer simulated by the REPROBUS chemical transport model. We have changed the "GB ozone loss" to "the ozone loss estimates based on the ground-based measurements" throughout the article. Please find the revised sentence in Lines 166—171

23# Page 10781, line 28: I think that if you added the words 'so that on 1 June each year the ozone loss is defined to be zero.' that would significantly aid the reader in understanding what you have done. Always put yourself in the position of the reader, who may have limited knowledge of this technique, and ensure that the reader can easily follow what you have done. It would improve this paper a lot if you could be more pedagogical in your writing.

Done. This has been stated in Lines 177—180

24# In Figure 1 don't colour the onset, rapid loss, max loss and recovery boxes across the middle. At first site I thought that these colours were somehow related to the colours of the dots on the plots which they are not. I can't see that there is anything to be gained by colouring those boxes.

Removed the colour boxes. Please find the revised Figure 2

25# Page 10782, line 12: Wouldn't it be more accurate to say that it depends on the history of the exposure of the air parcels measured to contact with PSCs?

Yes, changed in Lines 191—193

26# Page 10782, line 25: Now when you say 'in the region' do you mean in the edge region or in the core region? I think you should just say 'the ozone loss starts by mid-June…'.

Done. Please find it in Lines 211—212

27# Page 10785, line 22: Were the data series shown in Figure 4 derived by simply averaging the ozone loss signals at all 8 stations? Were stations weighted to account for the fact that they may be representative of different regions of the vortex? This isn't stated anywhere in the paper.

If the measurements are inside the vortex in accordance with the vortex definition, then they are used in the analysis. However, we have also analysed the ozone loss inside the vortex core and averaged over the equivalent latitudes 65—90S. There were no significant differences among these analyses. Therefore, no special scaling has been applied to find the ozone loss average. This has been mentioned in Lines 223—231

28# Page 10785, line 24: But so far you haven't said anything, or shown anything, related to PSCs. In fact this is the first instance of the acronym (which needs to be expanded at the very least). So there is nothing to support the assertion that the ozone loss starting in mid-June/early July is 'in agreement with PSCs' (whatever that means). You either need to demonstrate this or you need to cite a paper that supports this assertion.

"Polar Stratospheric Clouds (PSCs)" is mentioned in Lines 192—193. We have cited Solomon (1999) and Solomon et al. (1986). Please find the revised text in Lines 234—238

29# Page 10785, line 25: I would say that the early years of ozone loss in Antarctica were before 1980. Certainly not as late as 1989-1990.

Corrected in Line 237

30# Page 10786, line 5: But this statement that the levelling out of the ozone loss is due 'to saturation of ozone loss' is purely speculative. You have not presented any analysis to support that assertion.

We have cited Solomon (1999), Solomon et al. (2005) and WMO (2011) to support the statement. Please find it in Lines 249—250

31# Figure 5 caption: Do you mean averaged between mid-September and mid-October? What level were the temperature data obtained at?

Yes, the average between mid-September and mid-October. The temperature data are taken from the ECMWF (European Centre for Medium-Range Weather Forecasts) operational analyses at 475K. Please find the revised Figure 4 caption

32# Surely 100/150 DU ozone loss cannot be equivalent to 40/50% ozone loss. 150 is 1.5 times 100 while 50 is not 1.5 times 40.

We have revised the Figure 4 caption. Please note that these ozone loss values depend on the passive ozone tracer values, as the ozone loss is calculated as measured ozone-passive ozone tracer in DU and (100*(measured ozone-passive ozone tracer)/passive ozone tracer) in % as mentioned in Lines 166—171

33# Page 10787, lines 12-15: Given this statement it would be very instructive to add EEASC (equivalent effective Antarctic stratospheric chlorine) to the panels in Figure 5, plotted on an inverted scale.

Done. Please find the revised Figure 4

34# Table 1: Maybe I am missing something but I can't see how 0.78 DU/day ozone loss in 1994 is equivalent to 0.55%/day but in 1995 2.75 DU/day is equivalent to only 0.53%/day. If the 1994 value is wrong, then how many of the other values may be wrong?

Thank you. We have checked the values again for this. It is -1.67 DU/day in 1994 and -1.76 (70-160/50) DU/day in 1995. However, -0.55%/day in 1994 is correct because it is calculated as 22-50/51 =-0.549. Also, in 1995, -0.53%/day is calculated as 23-50/51=-0.529. Please find the revised Table 2

35# Page 10787, line 23: I don't know what you mean by 'also mark a comparable temporal evolution'?

We mean "they also show a similar time evolution". This has been rephrased in Lines 277—278

36# Page 10789, line 15: Huck et al. definitely do not use a reanalysis total column ozone data set.

We have removed the phrase. Please note the revised discussion in Section 4.2

37# Page 10789, lines 15-18: This is not a very clear description of the added value that access to a passive tracer field brings so such an analysis. Please clarify this.

Ozone loss is computed as the difference between measured ozone and passive tracer (i.e. measured ozone-passive ozone tracer). However, the ozone loss in percentage is deduced as 100*(measured ozone-passive ozone tracer)/ passive ozone tracer. By dividing the ozone loss by passive ozone tracer, it minimises the uncertainties induced by the passive ozone tracer in the loss calculation to a large extent. Please note that we have revised the discussions in Section 4.2 and related text in Lines 303—309

38# Page 10789, line 26: I don't know what you mean by 'due to the difference between the tracers'. I found this explanation of the potential differences between the results of your analyses and those of Huck et al. and Tilmes et al. to be very confused. This needs to be explained much more clearly.

Done. There are different reasons for these differences. (i) Since the ozone loss is computed as "measured ozone minus tracer", the tracer values are important. Any uncertainty or unreasonable values would lead to significant differences to other ozone loss estimates. In order to test this, we also used the same tracer used by Huck et al. (2007) to find the ozone loss (i.e. we applied the formula given by the authors to make a passive tracer). However, there were some significant differences between these two analyses. The difference for the maximum ozone loss was up to -50 DU, depending on year, compared to our original analysis with model tracer. Therefore, differences in tracers are one of the important reasons for the differences among the ozone loss analyses.

(ii) The Antarctic ozone loss occurs over a broad altitude range of 350—675 K (e.g Hoppel et al., 2005, Kuttippurath et al. 2012). Therefore, the partial column ozone loss over 380—550 K represents only about two-thirds of the total column ozone loss. This deficiency in column ozone loss computation is one of the reasons for the difference between our analysis and Tilmes et al. (2006).

However, we have to note that these (Huck et al., 2007 and Tilmes et al., 2006) are some of the important works on the long-term ozone loss analysis in Antarctic. Our ozone loss analysis can also be termed as an extension of their studies. Please find the revised text in Section 4.2, Lines 303—309 and 315—318

39# Regression model: Just to confirm, you don't need to consider seasonality in any part of your regression model since the model is always applied to September to November means? I think you need to say something to that effect in the paper.

We use the deseasonalised data here. This has been mentioned in Lines 325—326

40# Page 10792, line 3: Did you use EESC or EEASC? Or more specifically, what age of air and age of air spectrum width did you assume for your EESC basis function. I think that you need to say more about that because this does affect the shape of the basis function and will impact your results.

EEASC, as given in WMO 2011 report. However, we have used a new EEASC data generated from NASA GSFC for the revised analysis. It considers the WMO scenario A1-2010, Mean age of air 5.5 years, age of air spectrum width = 2.75 years and the bromine scaling factor of 60. These have been noted in Lines 381—388

41# Figure 7: The labelling of tick marks on the X axes of Figure 7 are ambiguous. Can you please make the 1990, 1995, 2000 etc. tick marks bigger than the intermediates so that the reader can see which tick marks the labels apply to. The same applies to most of the other figures in this paper. From Figure 7 it appears that you have selected 1996 as the 'break point' for your change in trend? For the Antarctic, that seems to be too early. I think that you should see when EEASC peaks and then use that to guide which year to select as the breakpoint in your PWLT terms.

We have redrawn all Figures with larger tick marks and have put vertical lines in Figure 5 for years 1980, 1985,1990, 1995, 2000 and 2005. We have now changed the break point to 2000 in accordance with the EEASC peak year. All analyses and text have been revised accordingly. Please find revised Figures and the revised text about break point in Lines 399—401

42# Page 10792, line 9: You need to provide strong justification for excluding 2002 from your analysis. Just saying that you excluded is because it was 'anomalous' is not sufficient. Surely with all of the explanatory variables that you have there is no reason why your regression model should not also track the 2002 ozone.

We have included 2002 in our analysis (e.g. Table 3 and Figure 5 and Section 4.3)

43# Page 10792, line 15: I don't know what you mean by 'our results are within the predicted lines'. What are these 'predicted lines'? I don't see any lines on Figure 7 and I just don't know what you are referring to here.

That was meant by "within the expected range of values". This has been mentioned in Lines 410—411

44# Page 10792, line 25: I disagree that the 'linear trend describes the contribution of gas phase chemical ozone loss'. It simply describes all linear change in your time series that is not accounted for in any of the other basis functions. More problematic is the following possibility. Let's say that there is no change in ozone from 1989 to 1997 but one of the other basis functions e.g. heat flux, shows a strong downward trend over that period. To compensate for that, the regression model would display a positive trend in the linear trend term. This does not mean that there was a positive trend in the ozone. There is nothing that physically relates your linear trend term to ozone loss. That is just your interpretation of the regression model result. It doesn't mean that that interpretation is, by design, correct. Furthermore, very little of the ozone loss in Antarctic is gas phase. It is mostly heterogeneous chemistry so I don't understand what you mean when you say 'gas phase chemical ozone loss'.

Yes, we calculate the ozone trend after removing all the dynamical proxies and the aerosol influence. Therefore, we have changed the definition to "The PWLT describes a linear trend in ozone after removing all dynamical proxies (heat flux, QBO, solar flux, and AAO) and aerosol influence". Please find it in Lines 348—352

45# Page 10793, line 1: It is not true that 'the ozone reduction in the Antarctic dominates the halogen/chlorine loading'. Rather it is the halogen/chlorine loading that dominates the ozone reduction in the Antarctic.

Corrected in Lines 429—430

46# Table 2: It is not at all clear to me how you are comparing 1989-1996 trends obtained from the EESC basis function and from the PWLT basis function. The first has units of DU/ppb while the second has units of DU/year? I really hope that you're not just directly comparing the values that you got from the regression model - you can't do that because they have different units and are therefore not comparable.

We have the trend in EEASC in pptv/year and the regression coefficient of EEASC from the regression in DU/pptv. Therefore, in order to obtain the trend of ozone, we multiply both of these (I.e DU/pptv* pptv/year = DU/year) (e.g. Stolarski et al ., 2006). However, the trends derived from the PWLT model is DU/year itself. So we can compare the trends from both models directly. This has been mentioned in the text in Lines 388—393

47# Page 10793, line 8: How did you convert the EESC basis function coefficient, which has units of DU/ppb, to units of DU/year?

Please find the reply to 46#. Thank you.

48# Page 10793, line 9: You claim that the trends derived from your PWLT and EESC basis functions are both significant at the 95% level and yet they are very different from each other - they certainly don't agree within their 2 sigma error bars. Therefore they can't both be correct. One set of values must be wrong. Please tell the reader which values are right and which values are wrong.

The break point selection in our previous analysis was not in agreement with the peak EEASC year. However, we have now used 2000 as the break point in conjunction with the EEASC peak year and the new analysis results are given in Table 3. The new results from both models are in good agreement with those deduced from all data sets (Ground-based, TOMS/OMI, and MSR) and the trends are around -4.6 DU/yr over 1979—1999, and are significant at 95% confidence intervals. Please find the new discussion in Section 4.3.3 and Section 4.3.4

49# Page 10793, line 12: So, just to be clear here, you are saying that Chapter 2 of the 2010 WMO/UNEP ozone assessment was wrong in stating that the negative ozone trend up to 1996 was primarily the result of increases in halogen loading? For you to be correct, not only does the ozone assessment need to be in error, but hundreds of other publications that have shown that the Antarctic trend in ozone up to 1996 was dominated by increases in halogen loading must also be in error. Are you really prepared to stake your scientific credibility on this claim?

NO. Please find answer to the comment 1# and 48#. Thank you.

50# Page 10793, line 17: That may well be true but they certainly don't include the 2.6-2.8 DU/year trend that you have derived from the second version of your regression model.

As stated previously, this mismatch was due to an inappropriate turning point 1997. Therefore, we have revised the trend analysis. The revised estimates show a trend of around -4.6 DU/year over 1979—1999 and both methods (PWLT and EEASC based regressions) yield similar values for all data sets. Please find the revised Table 3 and discussion in Section 4.3.3

51# Page 10793, line 21: Again, how did you convert your regression model coefficient, which has units of DU/ppb, to DU/year?

Please find the answer to the comment 46#. Thank you.

52# Page 10793, line 25: But at the 95% confidence limit this must surely exclude your value of +0.7DU/year. Therefore one of you is right and the other is wrong. Please let the reader know which.

We have revised the analysis with new break point. Now the trends are about 1 DU/year over 2000—2010 from the EEASC regression, which are consistent with those of the CCM/CTM analysis. Please find the revised text in Lines 463—466 and Table 3

53# Page 10795, line 8: And you have 15 years of data (I counted the dots on Figure 7) and so, noting that you only need 8.8 to 9.3 years of data to detect a statistically significant trend at the 95% confidence level, you should definitely be able to detect a statistically significant trend. And yet you do not. So what's wrong?

The problem was with the break point 1997. The revised analysis has 11 years of data from the break point 2000 to 2010. The revised trend values are already significant at 95% confidence intervals. Therefore, no need of this calculation now. The new results are given in Table 3 and the revised text can be found in Section 4.3.4

54# page 10795, line 13: They can't possibly be in agreement with Hassler et al. (2011). If you are right, then 8.8-9.3 years of data puts you at around 2007 at the very latest (noting the turnaround in 1997). If Hassler et al. says that the first sign of detection of ozone recovery will be between 2017 and 2021, then your results are very far out of agreement.

We have removed this sentence.

55# Page 10796, lines 7-9: You state that 'The estimated ozone loss time series is consistent with the EESC and temperature distribution in each winter'. However, earlier you concluded that the ozone trend from 1989 to 1996 was not entirely explicable by changes in EESC. Doesn't it seem counter-intuitive to you that while EESC would fully explain the intra-annual changes in ozone, it does not fully explain the inter-annual changes (in the form of the 1989-1996 trend) in ozone?

Please find reply to comment 1# and 48#.

56# Page 10796, line 18: I note that you are careful to avoid any mention of your -2.6 to -2.8 DU/year result which contradicts your -5 to -5.6 DU/year result.

We have included the results from new analysis in the Abstract now. Please find the revised text in Lines 16—33

57# Page 10796, line 22: The statement 'Our forecast suggests that it will take another 8–10 yr to be able to detect a 95% confidence levels' is wrong unless I have misunderstood what you have written - which would mean that you have not explained clearly what you have done. If equation (2) gives values of 8.8 to 9.3 years, this is not the number of additional years required to detect a statistically significant trend, it is the number of years required to detect the prescribed trend, given prescribed levels of variability and autocorrelation. Since the trend starts in 1997, it is by 2007 at the very latest that a statistically significant trend should be detected i.e. it should have already happened. Much of my confusion results from the fact that from what you have written it is not clear whether equation (2) suggested that an ADDITIONAL 8.8 to 9.3 years of data would be required, or whether 8.8 to 9.3 years were the values obtained from the application of equation (2).

Please find answer to comment 53#. Thank you.

58# Page 10797, line 1: But surely if you adequately account for this confounding factors, e.g. by using a regression model, you remove that 'camouflaging' to expose the true signal, much as what Salby et al. Did?

Yes. But this statement was meant for the analysis "without heat flux", which is the proxy used for the meteorological changes, in our analysis. However, please note that the revised analyses give significant results for all data sets. Therefore, we have removed this statement. Please find the revised Section 4.3.5

GRAMMAR AND TYPOGRPHICAL ERRORS

Page 10777, line 17: Replace 'ozone loss estimations' with 'ozone loss estimates'. And I would advise similar changes elsewhere.

This has been done throughout the article (e.g. Lines 14, 134)

Page 10778, line 13: Replace 'Some studies' with 'Many studies'.

Done. Please find it in Line 65

Page 10778, line 14: Replace 'in the Antarctic' with 'in Antarctic'.

Done. Please find it in Line 67

Page 10779, line 1: Delete 'to follow the study'. It is superfluous.
Page 10779, line 1: Delete 'In Results' otherwise this sentence does not make grammatical sense.
Page 10779, line 5: Delete 'In Discussion' otherwise this sentence does not make grammatical sense.

Done. Please find revised Lines 86—92

Page 10779, line 6: Replace 'the derived ozone loss and their inter-annual variability' with 'the derived ozone loss and its inter-annual variability'.

Done. Please find it in Line 90

Page 10779, line 10: Instead of 'Materials' wouldn't it be better to say 'Data'?

Changed to "Data". Please find the new Section 2 heading, Line 93

Page 10779, line 15: Don't you mean 'the estimated ozone loss is less dependent on the selection of the stations'?

Yes. Please find it in Lines 101—102

Page 10779, lines 22-24: This sentence does not make grammatical sense and needs to be corrected.

Yes. Please find it in Lines 115—118

Page 10780, line 7: Replace 'total ozone version (v)8.5 from TOMS aboard' with 'version 8.5 total column ozone measurements from TOMS onboard'.

Done. Please find it in Lines 135—136

Page 10780, line 10: Replace 'aboard' with 'onboard'. And please make similar corrections elsewhere.

Done. Please find it in Line 136

Page 10782, line 16: Replace 'breadth' with 'latitudinal extent' and replace 'brim' with 'perimeter'.

Done. Please find it in Lines 198—199

Page 10782, line 20: Replace 'are mostly' with 'are most often'.

Done. Please find it in Line 204

Figure 4 caption: Replace 'are compared to those of' with 'are compared to that from'. Also in the caption also please state that the GB-based ozone losses are shown in red. Replace 'is consisted of' with 'consists of'. The figure caption states that ozone losses of 50 and 150 DU are equivalent to losses of 25% and 50% which can't possibly be true.

Done. Please find new FIGURE 3 caption: "Temporal evolution of the chemical ozone loss estimated from ground-based observations (red) inside the vortex is compared to that from TOMS/OMI (green), and MSR (blue) in DU (left panels) and in % (right panels) in 1989—2010. The horizontal dotted lines represent -50 and -150DU of ozone loss (left panels) and -25 and -50% of ozone loss (right panels), while the vertical lines represent day 181, 225 and 275."

Page 10787, line 26: Replace 'ozone values in' with 'ozone values averaged over'.

Done. Please find it in Lines 284—286

Page 10791, line 13: When you say 'in August–September' do you mean averaged over August and September?

Yes. This has been changed in Line 356

Page 10792, line 14: Replace 'by the changes' with 'resulting from changes'.

Done. Please find it in Lines 408—409

Page 10792, line 15: Delete 'Presumably'. There is no presumption here at all.

Done. Please find it in Line 411

Page 10794, line 24: What do you mean by 'tangible'? Do you mean statistically significantly different from zero at the 2 sigma level? If so, say so.

Clear signal. The sentence has been revised in Line 493