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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Antarctic ozone loss in 1989–2010: evidence for ozone recovery?" *by* J. Kuttippurath et al.

Anonymous Referee #1

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

GENERAL COMMENTS

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 co-authors 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 authors seem somewhat unaware of much of the extant literature which shows much of what is shown in this paper. Too often results are presented as if they are new and known for the first time. For example, at the top of page 10784 it is stated 'The day when the minimum was measured is between day 260 and 270 at all stations, which falls in the end of September and early October period, indicating the period of maximum ozone loss in the Antarctic'. Now I know that many papers have shown that chemical ozone loss over Antarctic maximizes in late September/early October, but no citation of these papers is made here. The authors need to do a much better job in recognizing and citing the existing literature that shows the same, or similar, results to those reported on in this paper. As just one example, a highly relevant paper for the work presented here is the ACP paper titled "Stratospheric ozone chemistry in the Antarctic: What controls the lowest values that can be reached and their recovery?" by Grooss et al.

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.

SPECIFIC COMMENTS

Page 10776, lines 13-14: small interannual variability in what? And would the correlation be better if the interannual variability were bigger or smaller? From what you have written this is not clear. It's also not clear to me that it would be primarily the effects of interannual variability that would affect the correlation between V_PSC and ozone loss rates.

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.

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?

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.

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.

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?

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.

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.

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

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.

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.

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.

Page 10779, line 2: When you say 'minimum ozone measurements at each station' it is not clear whether you mean the minimum number of ozone measurements at each location required to derive robust statistics, or whether you mean measurements of ozone minima. As a result I really couldn't understand what this sentence was trying to say even after I had read it a number of times. Please rewrite this sentence for clarity.

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.

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).

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).

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

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

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

Page 10781, line 2: Why was the correction not done for the analysis of minimum ozone. Surely this would be one instance where such a correction would be essential? 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?

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.

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.

Page 10781, line 9: Does this translate into a 5-10% uncertainty in the derived total column ozone. If so, I would suggest this would be so large as to make the MLS total column ozone data unusable for your analysis.

Page 10781, line 11: I guess then, by definition, they are not overpass data and it is inappropriate to label them as such.

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.

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.

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?

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...'.

Page 10783, line 5: The term 'The minimum ozone distribution' is ambiguous. Either you mean: 1) The time series of annual ozone minima at a given station, or 2) The spatial distribution of ozone minima over the course of some period, or 3) The distribution of ozone on the day when the minimum ozone over the Antarctic was experienced. So, which of these is it? Please make it very clear in the text what you mean by 'The minimum ozone distribution'.

Figure 2 caption: These are not distributions, they are time series. So start the caption with 'Time series of...'. Now these are annual minima. So say so. Then in the caption you say 'The SCIMACHY and GOME measurements are shown by dots for clarity reasons'. First, please correct the spelling of SCIAMACHY. Secondly, it appears to me that all data are shown with dots, not just GOME and SCIAMACHY?

Page 10783, line 22: There are many other papers that have shown that the Antarctic vortex is not always centered on the South Pole and you need to cite some of them here.

Page 10784, line 12: this value of +-30 DU seems to be very large given the stated random errors on the respective measurements. Either the random errors on the measurements have been underestimated or the co-location criteria used for the intercomparisons are too weak.

Page 10784, line 15: So what confidence is the reader to place in this +-30 DU value given that 'the observational characteristics and measurement gaps could also con-

tribute to the observed differences in the minimum ozone values'. Does it mean they should not place too much value on that result? You've kind of left the reader hanging here, not knowing what to do with this +-30 DU result.

Page 10784, line 20: I don't know what you mean by 'The average ozone distributions' because Figure 3 shows no ozone distributions, only ozone time series.

Page 10784, line 24: What you have written here is not true. Please replace 'the GB ozone is shown' with 'the ozone loss based on the GB measurements is shown'.

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.

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.

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.

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.

Page 107876, line 14: 'exceptionally good' is a value judgment. Just state quantitatively what the agreement is and let the reader decide whether this should be considered as being 'exceptionally good'.

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

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.

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?

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

Page 10788, line 4: I don't believe that the WMO use of October averages suggests that the expectation is that ozone loss maximizes in October. It is simply a matter that if one has to choose between a September average and an October average (since often chemistry-climate models only save monthly fields) then it is better to choose the October average. Paul Newman, for example, averages over the last 10 days of September to get an annual value - and you should at least be citing one of his papers that do that.

Page 10788, line 8: By 'in good accordance with' do you mean 'well correlated with'? I think that this sentence needs to be reworded more carefully.

Figure 6: We know that it is not the volume of PSCs that drives ozone loss. No matter what the PSC volume in 1940 was, ozone loss would have been zero or close to it. It is the combination of PSCs and halogens that drives ozone loss. If you do the correlation over a short enough period, especially if chlorine and bromine are roughly constant over that period, then you're likely to get a good correlation. In the Arctic, with its greater variability, you get a stronger correlation because you're correlating on

the year-to-year variability rather than on the underlying secular signal. The bottom line is that you will almost certainly get a much stronger correlation if you correlate V_PSC*EEASC against ozone loss. I think this is what Simone Tilmes did in a paper or two and so you should go and read those.

Page 10788, line 19: It wasn't clear to me why you excluded 2002. Surely if there is a physical mechanism that connects V_PSC to ozone loss, then that mechanism should also have been active in 2002? You can't just exclude it because it might corrupt your correlation. If it does corrupt your correlation then that tells you something.

Page 10788, line 21: So giving you an Rˆ2 value of 0.25 suggesting that V_PSC explains only 25% of the variance in ozone loss.

Page 10788, line 23: And I think that the greater correlation in the Arctic results from the larger inter-annual variability which swamps the EEASC influence and drives much of the correlation.

Page 10788, line 25: I don't know what you mean by 'has also shown' because you have not shown that V_PSC in the Antarctic has reached a saturation level.

Page 10788, line 26: I don't know what you mean by 'every other winter is warm'. Do you mean that every second winter is warm? If so, I don't think that's true.

Page 10789, line 3: Or all winters are extreme in the Antarctic.

Page 10789, line 4: I think you're getting close here. I would strongly recommend that you try correlating V_PSC*EEASC against ozone loss.

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

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.

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.

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.

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.

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.

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.

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.

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'.

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.

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.

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

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.

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?

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.

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

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.

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?

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.

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?

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.

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).

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?

GRAMMAR AND TYPOGRPHICAL ERRORS

Page 10776, line 9: It is not the loss that recovers but the ozone that recovers and so I would change this to 'and ozone recovers thereafter'.

Page 10776, line 25: Are you really so pressed for space that you have to abbreviate year to yr? Whenever you introduce an acronym you reduce the readability and easy flow of the text. This incurs a cost to the reader. Always question whether it is worth that cost.'

Page 10777, line 4: Replace 'a number of instruments has been deployed' with 'a number of instruments have been deployed'.

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

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

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

Page 10778, line 29: Replace 'previous article' with 'previous publication'. Articles are not, generally, peer reviewed.

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.

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

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

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

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

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'.

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

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

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

Page 10783, line 20: Replace 'is comparable with those' with 'is comparable with that'.

Figure 3 caption: Replace 'represent 150DU of ozone or 150DU/50% of ozone loss' with 'represent 150DU of ozone or 50% of ozone loss'.

Page 10785, line 17: Replace 'to those of GB' with 'to those of GB observations'.

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.

Page 10786, line 12: Replace 'affirms' with 'confirms'.

Page 10786, line 22: Replace 'ozone total column loss' with 'total column ozone loss'.

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

Page 10788, line 2: Again, not ozone distributions (which creates the mental impression of maps) but rather ozone time series.

Page 10788, line 11: Replace 'temperature in mid-September/mid-October' with 'temperature averaged from mid-September to mid-October' and make similar changes elsewhere.

Page 10788, lines 16-17: This sentence is not grammatically correct. Please correct it.

Page 10788, line 18: It would be more instructive to say 'averaged between' rather than 'computed between'.

Page 10789, line 10: I don't think that you mean 'temporal resolution'. I think that you mean ’temporal coverage’. Temporal resolution refers to how often the measurements are made not to when they are made.

Page 10790, line 13: Replace ’ozone in 1997–2009’ with ’ozone from 1997 to 2009’.

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

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

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

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 10775, 2012.