SAP 2.4 "Trends in emissions of ozone-depleting substances, ozone layer recovery, and implications for ultraviolet radiation exposure."
Public Comment Period: 18 March through 2 May 2008.

USG Reviewers

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PUBLIC Reviewers

NOAA Research Council
The Research Council, as part of its major project oversight duties for the Climate Change Science Program (CCSP), was asked to review the draft report for CCSP Synthesis and Assessment Product 2.4: Trends in Emissions of Ozone-Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure.

The Research Council Exec Sec sent the document to all Research Council members and to additional NOAA scientists who were identified for their expertise to review the document. Dr. Susan Solomon and Dr. Bob Portman submitted their comments directly to the CCSP website. Dr. Craig Long has also replied, and his comments are included below.

The Research Council Executive Secretariat sent the document to all Research Council members and to specific NOAA staff that were identified to review the document. Following are the consolidated Research Council comments. These comments do not necessarily represent official NOAA viewpoints, since these have not been vetted throughout the whole agency.
Do not consider these comments as part of the official government review on SAP 2.4
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S. Fred Singer
President
Science & Environmental Policy Project

GENERAL USG COMMENTS

Responses to the Public Review comments are shown in blue typeface.

Samuel P. Williamson, NOAA/OFCM
Thank you for the opportunity to review Synthesis and Assessment Product (SAP) 2.4. The OFCM believes the document adequately assesses the trends in emissions of ozone-depleting substances (ODS), ozone layer recovery, and implications for ultraviolet (UV) radiation exposure.
The OFCM has provided general comments to improve the readability of the document and specific, suggested changes/additions to help clarify or strengthen a few areas within the SAP.
Response: We appreciate the above comments and the reviewer's other comments below.

The 20 Questions and associated responses at the end of SAP 2.4 are particularly informative for all levels of readers of this document. The less informed reader should be directed to read that section before reading the report. Additionally, providing page numbers for the 20 Questions section would be most helpful.
Response: The Preface directs the reader to the 20 Questions. We will address the page numbering in the final layout of the document.

No information is provided anywhere in the report on the impact of aircraft contrails on ODSs or UV radiation. This topic should be discussed at least briefly and some assessment should be made on the potential, future impact or the need to do further research to determine contrails' possible impact.
JUSTIFICATION: A research team of American and German scientists, headed by Patrick Minnis of the NASA Langley Research Center in Hampton, Virginia, reported in the June 21, 1999 AGU RELEASE NO. 99-19, that contrails cause a warming of the Earth's atmosphere, although their impact is currently small as compared to other greenhouse effects. They predict, however, that the impact may grow by a factor of six over the next 50 years. In 1992, for example, contrails added an estimated 0.02 watts of warming per square meter globally, about one percent of all manmade greenhouse effects.
Air traffic and, therefore, contrails, are not evenly distributed around the globe. They are concentrated over parts of the United States and Europe, where local warming reaches up to 0.7 watts per square meter, or 35 times the global average. The resulting temperature increase is not computed in this study, but is estimated to reach between 0.01 and 0.1 degrees Celsius (0.02 and 0.2 degrees Fahrenheit) over the northern temperate zones for current air traffic. In the future, increased air traffic will raise these values.
Large, linear contrails can be observed in satellite imagery. Although their total global coverage has not yet been determined, it is computed from traffic and weather data to amount to 0.1 percent of sky coverage. In the parts of Europe and eastern North America with the heaviest air traffic, however, contrails currently cover up to 3.8 percent and 5.5 percent of the sky, respectively.
Minnis and his colleagues report that global air traffic rose by over seven percent per year from 1994 to 1997, in terms of passenger miles flown. Growth is likely to continue,

meaning contrails will play a larger role in future climates than they do today. Taking into account such factors as number of flights per day, fuel consumption, and altitudes flown, Minnis and his colleagues conclude that by 2050, average contrail coverage over Europe will be four times higher than at present, or about 4.6 percent. In the United States, the increase will be 2.6 times current levels, or 3.7 percent coverage; and in Asia, the increase will be ten times current levels, or 1.2 percent.
Please update the text to incorporate a discussion on the impact of contrails on ODS or UV radiation and include the following two citations in the list of references:
(1) Minnis, Patrick, Ulrich Schumann, David R. Doelling, Klaus M. Gierens, and David W. Fahey, "Global distribution of contrail radiative forcing, June 21, 1999, AGU RELEASE NO. 99-19, Washington, D.C., USA
(2) Handwerk, Brian, Airplane Contrails Boost Global Warming, Study Suggests, June 14, 2006, National Geographic News, Washington D.C., USA.
Samuel P. Williamson, NOAA/OFCM
Response: We have not addressed this issue separately. We have adopted the climate forcing from IPCC assessments, which include the forcing by contrails. Thus, some of the issues noted are addressed. To our knowledge, there are no reports of the influence of contrails on ODSs in terms of their lifetimes, special reactions, etc. Some of the specifics noted here, though scientifically interesting, lie outside the scope of SAP 2.4 as defined in the Prospectus.

SPECIFIC USG COMMENTS

Chapter 2, Page 80, Lines 1937 – 1966
The text in these lines does not mention the impact of ozone on climate warming. The OFCM notes that in the first global assessment of the impact of ozone on climate warming, scientists at the NASA Goddard Institute for Space Studies (GISS), New York, evaluated how ozone in the lowest part of the atmosphere (the troposphere) changed temperatures over the past 100 years. Using the best available estimates of global emissions of gases that create ozone, the GISS computer model study reveals how much this single air pollutant and greenhouse gas has contributed to warming in specific regions of the world. Ozone was responsible for one-third to half of the observed warming trend in the Arctic during winter and spring, according to the new research. Ozone is transported from the industrialized countries in the Northern Hemisphere to the Arctic quite efficiently during these seasons. These findings will be published in the American Geophysical Union's Journal of Geophysical Research-Atmospheres. Based on this information, please consider adding text describing the impact of ozone on climate warming.
Additionally, please reference the GISS study on the impact of ozone on climate warming. To be consistent with the references cited already in the SAP, the reference should appear as:
Goddard Space Flight Center, 2008: Tropospheric Ozone Impacts Global Climate Warming – Arctic Dissolves, submitted.
http://svs.gsfc.nasa.gov/vis/a000000/a003300/a003340/index.html
Samuel P. Williamson, NOAA/OFCM

Response: The comment raises a scientifically interesting point. However, the scope of Chapter 2 (and more generally SAP 2.4) is stratospheric ozone, not tropospheric ozone. Because ozone-depleting substances and replacements, with the exception of a few very recent substitutes, are not very chemically active in the troposphere, their influence on tropospheric composition (specifically tropospheric ozone) is minimal. The interrelation between stratospheric ozone and climate is addressed in Chapter 4, and we do not believe that this point would be appropriately mentioned in this section of Chapter 2. Lastly, the suggested citation was not published when this SAP was completed and is not appropriate for this SAP; therefore, it is not cited.

Chapter 3, Page 202, Lines 4430 – 4457
The text in these lines does not mention the impacts of UV radiation on human health. The only instance where a discussion of the impacts of UV radiation on human health appears is in the 20 Questions section of the SAP. The OFCM believes that not including the impacts of UV radiation on human health in Chapter 3 is a significant omission. Stratospheric ozone depletion is a concern because the ozone layer in the stratosphere prevents 95-99% of the sun's UV radiation from striking the earth. A number of consequences can result from increased levels of UV radiation striking the earth (e.g., genetic change, eye damage, and damage to marine life). Increased UV radiation in the lower atmosphere can result in increased amounts of photochemical smog. Photochemical smog is already a health hazard in many of the world's largest cities. Please consider adding text which described the impact of UV radiation on human health to this part of Chapter 3.
Additionally, a reference on the subject of the impact of UV radiation on human health should be added to citations for this chapter. The OFCM offers the following as a reference on this subject:
National Academies of Science, 2008: Stratospheric Ozone Depletion, http://www.nas.nasa.gov/About/Education/Ozone/ozone.html
Samuel P. Williamson, NOAA/OFCM
Response: There are basic mentions of UV effects on human health including references to NIH results on cancer incidence just after Figure 3.22. We have added a comprehensive reference to the 2006 WHO report, "Global Burden of Disease from UV Radiation." Detailed discussions of the impact on human health are outside the scope of this document.

Chapter 6.4, Page 332, Lines 7393 – 7469
Section 6.4 of this chapter is entitled "Potential Management Options." However, few management options are presented except compliance with the Montreal Protocol. It would be useful to provide a more comprehensive list of management options, especially at the regional and local levels. Please consider amending this section by addressing a broader range of management options.
Samuel P. Williamson, NOAA/OFCM
Response: We address possible options only in general terms. It is not within the scope of this SAP to detail specific policy options, particularly at the regional or local levels as suggested. The effects of curtailing emissions from banks are presented in terms of the

effects on EESC recovery. Similarly, the magnitudes of non-regulated uses of methyl bromide are quantified.

Chapter 6.4, Page 335, Lines 7463 – 7469
The text beginning on Line 7463 states: "Action could be taken to limit the release of chlorofluorocarbons (CFC) and hydrochlorofluorocarbons (HCFC)." However, there isn't any possible actions/solutions provided. What actions or management options are available to limit the release of CFCs and HCFCs? Please consider updating this text by incorporating example actions to limit the release of CFCs and HCFCs.
Samuel P. Williamson, NOAA/OFCM
Response: As stated above, it is not within the scope of this SAP to suggest specific actions pertaining to limiting the release of CFCs and HCFCs from banks. We present, in quantitative terms, the potential magnitude of such emissions relative to EESC recovery thereby providing a framework for the consideration of future regulatory actions.

Chapter 6, Page 336, Lines 7471 – 7494
"The World Avoided" section within this chapter discusses the role of Montreal Protocol regulations in reducing equivalent effective stratospheric chlorine (EESC). However, the role of education and outreach is not discussed in this section. The OFCM believes that providing information to decision makers and others (whether they are emergency managers, legislators, educators, students, or the general public) is absolutely critical in facilitating informed decision making. Informed decision making and subsequent actions taken by those informed constituencies can also facilitate reductions in EESC and other ODSs. Please consider amending the text in this section or develop a new section to discuss the role of education and outreach.
Samuel P. Williamson, NOAA/OFCM
Response: Thank you for this interesting point. Now, additional text has been added in Section 6.4.1 to highlight the importance that public awareness has played in success of the Montreal Protocol.

Chapter 6, Page 337, Lines 7490 - 7494
The text in these lines reads: "The coming decades will be a period of changing atmospheric ODS levels superimposed on changing climate, climatic variability, and other factors. Box 6.1 outlines the key gaps in scientific understanding that can be identified at this time and that could help inform future decisions …." This text alludes to the transition of research findings into operations. The OFCM believes that this text should be amplified to clearly (a) make the connection between new findings and transitioning those finding into operations and (b) articulate the difficulty of making that transition. Therefore, the OFCM recommends that the text be amended to read: "The coming decades will be a period of changing atmospheric ODS levels superimposed on changing climate, climatic variability, and other factors. Transition of new knowledge from the research domain to the operational environment can be a long, arduous process, replete with impediments. Box 6.1 outlines the key gaps …"
The additional text is taken from the OFCM document, Report of the Assessment Committee for the National Space Weather Program. Please consider citing this

document within the list of references for this chapter. To be consistent with the references already cited in the SAP, the reference for this document should appear as: OFCM, 2006, Report of the Assessment Committee for the National Space Weather Program, FCM-R24-2006, Washington, D.C., USA, Chapter 3, page 48.
Samuel P. Williamson, NOAA/OFCM
Response: We believe that the details of the transition from research to operations are not science issues but rather programmatic issues for agencies and decision makers. Therefore, we believe the generalities are appropriately addressed in Box 6.1.

GENERAL PUBLIC COMMENTS

S. Fred Singer
I could not find answers to the following questions in the CCSP Report on Stratospheric Ozone
1. What is the expected (calculated) rate of increase of stratospheric Bromine as a result of the continuing use of Methyl Bromide?
Response: The answer to this question is shown in Figures 5.5 and 5.6. The bottom right panel in Figure 5.5 shows that future emission of CH_3Br will result in an additional 0.5 ppt of CH_3Br at the surface. This translates into 20 ppt of EESC. The impact in terms of integrated EESC is shown in Table 5.2. .The text was not changed since the information is available in the Figures.

2. What evidence is there for an observed secular increasing trend in stratospheric Bromine -- such as might indicate a human contribution ?
Response: This question is answered in Section 2.4.2 on atmospheric bromine in the reviewed text. The point was directly addressed on lines 2734-2745 and this text is retained in the revised version.

3. What is the expected (calculated) rate of ozone destruction from HOx and NOx in the stratosphere as a function of altitude?
a] How do these rates compare with rates from ClOx? Can you show a graph comparing these rates?
Response: The relative contributions of HOx, NOx, and ClOx in the mid-latitudes of the SH (similar to the NH midlatitudes) are shown in Figure 1.11 of the IPCC/TEAP Special Report on Safeguarding the Ozone Layer and the Global Climate System (IPCC/TEAP, 2005). We have now referenced that Figure in Section 3.1 so that interested readers may find the answers to these questions.

b] Repeat the calculated comparison of 3a, but in the presence of heterogeneous reactions (in the presence of stratospheric particulates)
Response: The ozone changes noted here include the effects of heterogeneous reactions. Comparisons of the calculated rates of ozone loss with and without heterogeneous processes were done more than a decade back and they have been reviewed and assessed in earlier International Assessments. Therefore, we believe that the suggested comparison is unnecessary in this document. We just note here that heterogeneous

processes dominate in the cold Antarctic & Arctic conditions of the polar lower stratosphere. Because of the observed very high levels of ClO in the polar lower stratosphere, the ClO-ClO and ClO-BrO catalytic cycles are overwhelmingly dominant. These include reaction sets 3 and 4 and they are discussed in Section 3.1. We have further pointed out the dominance of these reactions in Section 3.2.3.2.1, dealing with Polar chemistry.

4. How important are possible anthropogenic sources for stratospheric HOx and NOx that might cause ozone destruction?
Response: Section 5.2.1 discusses the effects on ozone from CH_4 and N_2O that could change the HO_x and NO_x radical concentrations. In addition, possible effects on NO_x from aviation, and HO_x from H_2 (associated uses of liquid hydrogen in energy generation) are also mentioned. The authors feel that there is not enough information for more quantitative assessment in the absence of realistic emission scenarios.

5. What is the impact of the recent JPL measurements on the photo-dissociation of the molecule O-Cl-Cl-O ?
Response: The ClOOCl photolysis rates measured by Pope et al. (2007) are discussed in the first 3 paragraphs of Section 3.2.3.2.1.
Pls respond in quantitative detail where appropriate and include in the Report.
Response: The implications of this new study are being explored in the scientific community and their findings were not available when this report was written. In the absence of such material, we cannot assess the impacts any further in this Synthesis and Assessment Product.

Thank you for the opportunity to comment on CCSP
S. Fred Singer
Response: Thank you for your comments.

Craig Long, NOAA
This is a very good document combining and synthesizing information from other recent assessments and peer reviewed publications to provide the public with the most recent understanding of ODS, ozone, and UV trends and impacts. The ozone section is well written and inclusive of the latest information and concepts pertaining to the ozone layer, its trends, and recovery. Comments concerning the UV portion of the chapter follow:
Craig Long, NOAA
Response: Thank you for commenting.

A reasonable addition to ozonesonde sites at a low cost with large scientific returns would be a low cost total ozone instrument and a broadband UV instrument. Very few sites have these three instruments. The total ozone instrument would complement the ozone profile from the ozonesondes and the UV measurements from the broadband instrument.
Response: The SAP is an update of scientific understanding and gaps but does not attempt to make specific research recommendations. Therefore,we have not made a revision in response to this comment.

As presented in many of the summaries, it is difficult to determine what the impacts of future changes in ODS and climate changes have upon ozone and UV within the U.S. Yet it is clearer that the role the U.S. plays in the mitigation and destruction of ODS can have large global impacts.
Response: We agree with this comment. The text does reflect the difficulty noted. However, we believe that we cannot do any thing more in this SAP.

Susan Solomon, NOAA.
Lines 1-7700. Overall comments: This SAP presents an excellent summary of a great deal of important material. I complement the authors on a job well done. There is a lot of good review of history and published information, and some very important new information and analysis. I would like to note that the concentrations and emissions chapter, which took on a lot of challenging tasks and did them very well. The use of published versus non-published work in that chapter is done well, and is very appropriate. My comments follow in order occurrence. Most are minor. A few are substantive – my biggest concerns have to do with the discussion of ozone's cooling effect on climate and the degree to which it does or does not offset ODS warming. I think these concerns can be met with wording changes and rearrangement of material as I suggest below, and I think that these changes are important for ensuring a balanced document.
Susan Solomon, NOAA.
Response: We appreciate the reviewer's overall comments above and the detailed comments below. We have addressed this issue in the text and they are noted in more detail below under the specific comments.

SPECIFIC PUBLIC COMMENTS

EXECUTIVE SUMMARY

ES lines 405-406 Please change from 'turning the corner toward the return to 1980 levels' to 'turning the corner toward increased ozone' because we expect super-recovery, not a return to 1980 levels.
Susan Solomon, NOAA
Response: Changed as suggested.

ES line 513. Please change to 'Depletion of the ozone layer thereby has the potential to produce a cooling effect on climate'
Susan Solomon, NOAA
Response: Changed as suggested, using slightly different wording.

ES Lines 525-548. Major comment. This is an important issue but it needs revisions here for accuracy and balance. The current state of understanding must be carefully assessed, and I don't believe that has been done adequately. I have two major points:

First major point: please put error bars on these estimates – whether or not the cooling is negligible cannot be asserted in the absence of error estimates on both the warming and cooling trends. In fact, it is not at all obvious that it is negligible if you adopt the IPCC error estimates. Please ensure that any statement about cancellation doesn't just give a central value, but also an error range. Then avoid saying that cooling is negligible – this isn't clear when error bars are included. It would be more accurate to say "This forcing is likely to be a cooling term (i.e., in the opposite direction to climate forcing by the ODSs that caused the depletion) but has much larger uncertainties. Globally averaged, it may even represent a warming within error bars, or it could offset a large (up to YY%) portion of the ODS warming, while a current best estimate is an offset of 20%. Global averaged estimates for ozone layer depletion imply that it may have offset A (X to Y) W/m2 of the positive (warming) climate forcing by ODSs that caused the depletion." {fill in X-Y from the estimates with error bars; I believe this is -0.05 (+0.05 to -0.15) W/m2 from IPCC (2007).} Please also state that "These estimates are based on the observed ozone changes, and on the assumption that they are due entirely to ODSs."
Second, please state that: "Recent research has shown that ozone cooling and ODS warming often occur in different places and times, making it less appropriate to consider the two terms as offsetting one another than previously thought." This is a key issue in adding them up and it needs to be here.
Third, it would be helpful to have a closing statement that: "Thus, there are currently large uncertainties not only in the magnitude of global ozone cooling effects and the degree to which they offset the warming due to ODSs, as well as where and how these offsets would vary regionally. Further research is needed for better quantification."
Please propagate the above comments where appropriate throughout the report. Eg.., lines 2968-2973; 4967-4968, 5021-5026, etc. I am not going to list all occurrences here.
Susan Solomon, NOAA.
Response: This section of the Executive Summary has been substantially rewritten to clarify the points raised. Changes were made in numerous places throughout Chapter 2 to account for these points. Additional discussions were added in Section 5.5.3 to emphasize the impact of this uncertainty. Chapter 4 has been substantially revised. We have added a chapeau to the Section 4.2 to introduce this issue directly. The following sections go through the direct forcing by ODSs and the indirect forcing due to ozone depletion. To clarify this issue, a new box (Box 4.2) that is adopted from the TEAP report has been added. The issues of the extent of the offset and the uncertainties associated with this offset of terms are propagated throughout Chapter 4, Chapter 6, and the Executive Summary.

It is noted that the ozone RF estimates are based largely on observed ozone changes but these changes are not necessarily due entirely to ODSs. Stratospheric ozone RF values shown include uncertainties that derive from effects other than ODS amounts. This is noted in Section 4.2.2. Further, a comment on the regionality of the forcings is included in Section 4.2.2 and is propagated into the Executive Summary.

ES Lines 552-553. Please change to "The global average trend is attributed to ozone depletion, increased carbon dioxide (CO2), and changes in water vapor. Dynamical changes are also likely to be important for the local temperature changes, but are not significant for global mean stratospheric temperature trends."
Susan Solomon, NOAA
Response: Changed as suggested.

ES Line 611. Please change to 'there are no significant total ozone trends in the tropics' since there is evidence for decreased ozone at some altitudes in the tropics, as you discuss later in the report.
Susan Solomon, NOAA
Response: Changed as suggested.

ES Lines 613-617 need a little editing. Bromine is very unlikely to be doing much in the upper stratosphere and it should not be noted here. Language also needs to be improved – this region cannot be leveling off but it is not changing as quickly, presumably due mainly to the fast changes in CH3CCl3 having been realized; perhaps also with a solar or CO2 cooling contribution but I assume that has been taken out here. Please change to : Ozone depletion in the upper stratosphere, where the influence of chlorine is easiest to detect, has slowed, and has closely followed the trends in the sum of total chlorine. The slowdown of the negative (or decreasing) trend may be attributed to the fact that ozone-depleting chlorine is decreasing more slowly in the most recent years (give years), as the very short-lived substance CH3CCl3, declined earlier."
Susan Solomon, NOAA
Response: Revisions were made here to address these concerns and simplify this bullet. We specifically note that ozone changes follow the sum of chlorine. We also note that the bromine plays a lesser role in this region and that its trend is leveling off.

ES Lines 622-626. The statement here is a bit too strong. Please change to "The ozone hole area and ozone mass deficit were observed to be below average in some recent winter years while higher minimum column amounts have also been recorded. This variability results mainly from the strong influence of meteorological variability on ozone amounts, and any changes in the amounts of chlorine and bromine available for ozone depletion are not detectable."
Susan Solomon, NOAA
Response: Revisions were made, using slightly different wording, to address this comment.

ES Lines 639-641. This is a bit too strong. We are now seeing strat aerosols lower than the late 1970s or late 1980s, attesting to a continuing if smaller role for volcanic input at those times, I think, despite the fact that the major eruptions were quite a bit earlier. There is evidence for a lingering effect that lasts more than a few years; the key point here is just transience. I suggest: "The increases result in a transient shift in stratospheric ozone levels and climate because natural processes gradually remove the additional sulfate after the eruption." (please also change in lines 5041-5045).
Susan Solomon, NOAA

Response: Changed as suggested in the Executive Summary. Changes were also made to the pertinent bullet in Chapter 4.

ES Lines 755. Please say how many ODP-weighted tons of material is represented by this. Please also give an indication (with error bars, as in comments on line 542-548 above) of GWP-weighted tons this represents. How many Gt of CO2 eq is the accessible HCFC bank? Ozone loss offsets are known to be very small for HCFCs, so this is a very important option that should be in the summary.
Susan Solomon, NOAA.
Response: We have substantially rewritten these bullets by giving the ranges and separating out the ozone depletion related banks from the climate forcing related banks. In Chapter 2, additions were made to the Key Findings section (page 5) to quantify the HCFC and Halon contributions to the U.S. accessible bank. Quantitative information was also added to the caption of Box Figure 2.5-1.

CHAPTER 1

Chap 1 Line 1032, box figure. I like the figure in principle but the primary sequence OH+O3 followed by HO2+O3 is a major sink for ozone in mid-latitudes and tropics – it is not only in the polar regions that the bottom scheme operates. A quick fix to make it essentially right would be to change catalyst to 'chlorine' explicitly.
Susan Solomon, NOAA
Response: We have simplified the diagram to address this issue. We do not specifically note the importance of the two destruction pathways in different regions. Readers are referred to the text for details.

Chap 1 Line 1051, box figure. CH4 oxidation can also make ozone in the lowermost stratosphere via the same kind of chemistry. Please add to the box and change title to 'Ozone production in the troposphere and lowermost stratosphere'
Susan Solomon, NOAA
Response: We have changed "VOCs, CH_4, and CO" to "VOCs, CO, etc." so that contributions from chemicals such as H_2 are not ignored. We did not note that this mechanism contributes to some stratospheric ozone production since we are simply trying to highlight the difference between stratospheric and tropospheric ozone, i.e., show the counterpart to the simplified stratospheric chemistry of the other figure in this box.

CHAPTER 2

General note: These points were all specifically addressed based upon comments from this reviewer. In addition, 5 figures were redrawn, a new box was added (Box 2.2), a new table was added to the appendix, and new text was added in many other places to explicitly mention and include the ozone forcing in the discussions relating to GWP-weighted quantities or radiative forcing.

Chap 2 Line 1116-1119. Not sure I understand - Do you mean to say this is GWP-weighted? Please clarify. Please include error bars that factor in the ozone loss cooling, see comment above on lines 542-528. Susan Solomon, NOAA
Response: Footnote added for clarification that includes consideration of ozone cooling; the statement of relative decline is now qualitative.

Chap 2 Lines 1143-1146. I'm concerned that this is not really appropriate without ozone loss cooling and with error bars; see comment on lines 542-548, particularly since the offset is much larger for some gases than others. May be better to delete this. Susan Solomon, NOAA
Response: Numbers were replaced with qualitative statements that are valid when ozone loss cooling is or is not included.

Chap 2 Lines 1194-1198. Please include ozone loss cooling with error bars; see comment on lines 542-548. Susan Solomon, NOAA
Response: Done.

Chap 2 Line 1156. It does not seem very useful to give CO2-equivalent values for halons without including the ozone loss term. We would have to say that it is virtually certain that the ozone loss cooling for those gases far outweighs their warming contribution, and this is a good illustration of why neglecting ozone cooling can be very misleading. Please state the importance of ozone cooling for halons, don't give numbers for the halons that don't include the cooling term, and give the GWP weighted bank term for HCFCs alone.
Susan Solomon, NOAA
Response: Text was revised for clarity and points made by the reviewer are now included.

Chap 2 Line 1483, figure Same comment as on 1143-1146. Ozone offset could be big (within error bars) for CFCs, zero for HFCs and HCFCs, so the right hand panel doesn't seem to be a fair comparison. Susan Solomon, NOAA
Response: Figure was redrawn to include visualization of the magnitude and uncertainty in the ozone cooling influence.

Chap 2 Line 1898. Is it possible to see a similar figure to that of 2.6 for HCFC-142b, since that is the one molecule where a problem remains? Susan Solomon, NOAA
Response: Figure was redrawn to include HCFC-142b.

Chap 2 Lines 2968-2973. Please explain the change in error bars on the ozone cooling from earlier figures and discussion. Please also include complete discussion of error bars on ozone cooling and halocarbon warming offsets, instead of saying about 20% (see earlier comments regarding lines 525-548). Please also consider moving this up in the chapter. I think the chapter would be stronger if this section came before you began giving GWP-weighted figures without the ozone offset. I would suggest moving this section up to near the front of the chapter. This would help very much in addressing

many of the issues I pointed out earlier regarding the ozone offset in this chapter. Susan Solomon, NOAA
Response: Explicit consideration of weighting factors and this uncertainty is now included in a new box appearing in section 2.1.1 (very early on). Multiple figures and Table 2.1 were substantially revised to include these considerations.

CHAPTER 3

Chapter: 3.3.1 Page 184-185 Lines 4117-4140: The latter paragraph contradicts statements made in the former paragraph. Define "short-term" (minutes, hours, days?) and the same for "long-term" (days, months, years?). During the spring in NH mid-latitudes, day to day changes in total ozone can be quite large.
Response: This has now been fixed. We have clarified the issue about time scales and made the comments consistent with the following paragraph.

Chapter: 3.3.1 Page 186 Lines 4150-4162: Information in this paragraph is very basic and would be better placed in the beginning of this sub-chapter.
Craig Long, NOAA
Response: We are presenting some basic ideas since not everyone who reads this section is familiar with UV characteristics. In response to the comment, we removed the sentences that are already at the beginning of the subchapter.

Chapter: 3.3.1.1 Page 187 Figure 3.17: This may not be possible to do, but a better y-axis for these plots would be "Percent of Days". That would make them all similar. Also, the peak value and "percent at the peak" vs. the mean provides more information about whether the site is mostly snow covered, cloud covered, bare ground, or a mixture of these.
Craig Long, NOAA
Response: The change in scale from number of days to percent could be a possible option. However, we used figures that are based on published figures. We note that the panels can be directly compared in a manner similar to percent, since they all contain data from the same number of days.

Chapter: 3.3.1.1 Page 188 Lines 4177-4186: If these determinations are made via satellite, it should be stated so.
Response: Yes they were and they are now noted.
Also, in the next paragraph a sentence about how the satellite distinguishes between snow and clouds can be provided.
Craig Long, NOAA
Response: Since UV measuring satellites cannot distinguish clouds over snow and ice, a short paragraph to this effect has been added.

Chapter: 3.3.1.2 Page 189 Line 4212-4213: Dust is wavelength dependent. How significant is this dependence?

Craig Long, NOAA
Response: The effect of the wavelength dependence of dust is small since it will similarly affect all UV wavelengths. We have now added a sentence to address this issue.

Chapter: 3.3.1.3 Page 190 Line 4224: Please include the NSF/Biospherical network.
Craig Long, NOAA
Response: Thank you. This has been done.

Chapter: 3.3.1.4 Page 195 Line 4300: Replace "Estimating UV ttrends" with "Estimating UV trends"
Craig Long, NOAA
Response: This has been done.

Chapter: 3.3.2 Page 201 Line 4422: Replace "decrease in UVB irradiance caused by the" with "less UVB irradiance than San Diego because of the".
Craig Long, NOAA
Response: This has been done.

Chapter: 3.3.2 Page 202 Line 4427: inject "(eg. Barrow)" following "Arctic region".
Craig Long, NOAA
Response: This change was not made since we do not believe that fits here.

Chapter: 3.3.3 Page 202 Line 4431: add after UV irradiance: "and accumulated daily dosage".
Craig Long, NOAA
Response: The sentence has been modified, though slightly differently than suggested.

Chapter: 3.3.4 Page 207 Lines 4511-4517: This paragraph is not a good concluding paragraph. Better to leave it out.
Craig Long, NOAA
Response: We believe that the paragraph makes an important concluding point about the role of the Montreal Protocol and subsequent agreements. The "world avoided" is a theme we have raised in Introduction and the concluding chapter of this report; we believe that it is important for the technical chapter to substantiate and reinforce the point.

What is missing in the UV sub-chapters?
Multi-filtered instruments are entirely left out of this chapter, yet they play a very important role in determining aerosol optical depth and single scattering albedos. Throughout the UV chapters the importance of knowing the AOD and SSA is emphasized, but the mechanism of obtaining them is left out. UV-MFRSRs provide this information. The section in the 2006 Ozone Assessment pertaining to multi-filtered instruments can be added to this document to provide the needed text. The USDA /Colorado State University has a well established network of UV-MFRSR within the U.S. and at strategic places throughout the world. They have many papers that can be included in this section.

Craig Long, NOAA
Response: There are some issues with SSA determined by the UV-MFRSR's operated by USDA because of calibration issues. While this problem has now been partly resolved by using co-located CIMEL's for calibration, the calibration corrections are not available in the publications.

In line with the above, can the Aeronet instrumentation be altered to contain a 305 and 334 nm channel? This would go a long way to get UV aerosol information.
Craig Long, NOAA
Response: Good idea, but, no, this cannot be done. There are insufficient photons for the AERONET CIMEL sunphotometers to perform almucantor measurements needed to derive aerosol absorption.

Chap 3 Line 3385. The correct reference for the reaction HCl+ClONO2 is Solomon et al. 1986. This reaction was not considered in McElroy et al. 1986.
Susan Solomon, NOAA
Response: Thank you! We have made this correction.

Chap 3 Lines 3929-3942. Very good discussion of a complex issue. I suggest one minor edit: on line 3933-3934, it would be more accurate to say "However, one recent laboratory study of the absorption cross section….does not support this." This is needed to avoid the impression that multiple lab studies have obtained such a result, which is not the case. The wording using the plural, 'recent laboratory measurements' is less clear.
Susan Solomon, NOAA
Response: Thank you, We have made this correction.

CHAPTER 4

Chap 4 Line 4995. Change to 'Global average stratospheric temperatures have declined….' since dynamics can play a role in some regions.
Susan Solomon, NOAA
Response: Changed from 'Stratospheric temperatures have decreased. . .' to 'Global average stratospheric temperatures have decreased. . .'

Chap 4 Line 5028-5031. Change to 'analysis of observations indicates that…..' since such analysis includes ozone data as well as meteorological data. Please also add "; this has been well simulated in GCM calculations using many different models."
Susan Solomon, NOAA
Response: Changed: 'Meteorological analyses indicate' to 'Analysis of observations indicates that stratospheric ozone depletion over Antarctica has caused strengthened circumpolar flow throughout the troposphere over Antarctica and caused surface temperature changes. This effect has been well simulated using many different general circulation models (GCMs).'

Chap 4 Line 5125. The peak in GWP-weighted emissions of ODS was shown in detail first by the IPCC special report on ozone and climate (2005). Please reference this as well as the later 2007 Velders paper.
Susan Solomon, NOAA
Response: Thank you, the reference is added.

Chap 4 Line 5127. Please change to "The positive RF contribution from ODSs…." This figure doesn't include uncertainty in the ozone cooling so changing to indicate that you are neglecting the cooling term and its uncertainty is necessary.
Susan Solomon, NOAA
Response: We have now included a chapeau to this section to introduce the effects of direct forcing by ODSs and the radiative forcing caused by ozone depletion. This part has been substantially changed to account for these comments and the ones noted in the overall comments. As noted earlier, a new box has been added..

Chap 4 Line 5132. WMO 2007 is a nice figure but the background on this issue is very long and it's important this be acknowledged through appropriate references. The caption to figure 4.2 should reference the earlier literature: Daniel et al. (1995) and IPCC (2005) special report as well as WMO 2007.
Susan Solomon, NOAA
Response: References added to caption.

Chap 4 Line 5652. Please reference Thompson and Solomon 2002 in the caption, since the right hand of this figure is directly from that paper (you have chosen to remove the reference which was in the figure itself in the published paper, which is fine, but then it belongs in the caption).
Susan Solomon, NOAA
Response: Reference changed.

Chap 4 Line 5195. Table 4.1 provides a clear example of why neglecting stratospheric ozone cooling and its uncertainty is not appropriate. The halons are very effective ozone destroyers. There is little doubt that their cooling contributions are much larger than their warming contributions, so this table and other statements that don't provide a balanced assessment of both terms are not appropriate. I don't think this table is appropriate in view of this issue, and it should be deleted.
Susan Solomon, NOAA
Response: It is appropriate to display the direct RF of accumulated ODSs as done, for example, in Table TS-1 of IPCC/TEAP, which is the source of the displayed values. Our strong preference is to leave the table in the text since we have extended the discussion on RF changes due to ozone changes. We also note that the table was added based on an earlier reviewer's suggestion. We have added the word 'direct' to the table title.

Chap 4 Lines 5197-5199 make a very important point. Offsets must be considered and this needs to come before values not including them are given in an assessment aiming to inform policy. Please therefore move lines 5197-5262 up. The best place would be after line 5162, so that this comes before you begin talking about positive radiative forcing

terms so that the discussion is balanced. Please include error estimates in this discussion and the issue of whether the positive and negative forcings occur in the same places – see my comments above on lines 525-548. Then when you talk about forcings alone, please make clear that for that part of the discussion you are neglecting the negative forcing. This is helpful for the purposes of discussion but please be clear that it then is not a quantitative measure.
Susan Solomon, NOAA
Response: We strongly prefer to begin the discussion with the direct RF from ODSs. The direct and indirect ODS RFs are introduced in the beginning of Section 4.2 and more detail has been added in Section 4.2.2. Throughout, we have distinguished between the direct forcing by ODSs from the indirect forcing by the ozone depletion they cause. As noted earlier, we have a chapeau that sets this out at the beginning.

Chapter 4: Overall, I found this chapter to be informative, well written, and up to date.
Robert Portmann, NOAA ESRL.
Response: Thank you.

Chapter 4, Page 227, Line 5011-5013: Future trends are uncertain because both methane emissions and the temperature of the tropical tropopause are uncertain and their influence on water vapor is uncertain. Not because water vapor "responds" to these things.
Robert Portmann, NOAA ESRL. .
Response: Sentence changed to: 'Future water vapor trends are uncertain because of uncertainties in projecting methane emissions and the temperatures of the tropical tropopause.'

Chapter 4, Page 227, Line 5015-5018: You mention that the increased circulation may increase the flux of ozone to the troposphere, but what about its influence on stratospheric ozone?
Robert Portmann, NOAA ESRL.
Response: Changed to: 'Increased circulation will change stratospheric ozone amounts and increase the stratospheric flux of ozone to the troposphere.'

Chapter 4, Page 228, Line 5021-5022: Remove "because ozone is a greenhouse gas" as it adds nothing and it is not the reason for a negative radiative forcing.
Robert Portmann, NOAA ESRL.
Response: Phrase removed.

Chapter 4, Page 228, Line 5023-5024: This sentence is very poorly worded. Please rewrite. Do you mean something like: The cooling of the lower stratosphere due to local ozone decreases has lead to a cooling in the upper troposphere due to decreases in the long-wave radiation emitted from the lower stratosphere.
Robert Portmann, NOAA ESRL.
Response: Changed to: 'The forcing is a balance between a short-wave cooling of the lower stratosphere and a long-wave cooling below the region of ozone depletion.'

Chapter 4, Page 228, Line 5042-5043: The temperature and circulation changes in the lower stratosphere after an explosive volcanic eruption are not a "feedback response". They are just a "response" to the large increases in sulfate aerosol. What is the feedback loop in the process?
Robert Portmann, NOAA ESRL.
Response: The word 'feedback' has been deleted.

Chapter 4, Page 229, Line 5052-5054: The Kiehl and Trenberth paper cited does not show that ozone's warming of the planet is the third largest greenhouse gas. They show that it is the third largest long-wave greenhouse gas (7-10 W m-2, Table 3) but then in Table 4 compute -14 W m-2 shortwave forcing. They do not work out the net effect including stratospheric adjustment. The usual fixed dynamical heating method for computing the stratospheric adjustment will not work in this case (too big a change). Surprisingly, I think you have to go back to the old radiative-convective model literature to find an estimate of the largest greenhouse gasses for the natural atmosphere.
Robert Portmann, NOAA ESRL.
Response: The qualifier 'long-wave' has been added.

Chapter 4, Page 230, Line 5090: What does the "thus" in the last sentence follow from? I would remove it. The sign of the ozone radiative forcing is not obvious as it comes from the competition between large terms of opposite sign.
Robert Portmann, NOAA ESRL.
Response: The word 'thus' has been deleted. This section has been rewritten to include the range of values for the ozone forcing.

Chapter 4, Page 233, Line 5162-5163: The radiative forcing does not depend on the GWP.
Robert Portmann, NOAA ESRL.
Response: The sentence now reads: 'The radiative forcing of individual ODSs varies because of differences in emissions, lifetimes, and radiative efficiencies.'

Chapter 4, Page 237, Line 5244-5245: A positive RF could occur without an increase in ozone it some regions. A decrease in ozone in the stratosphere causes a negative LW RF and a positive SW RF. The positive SW term is generally larger. The adjustment causes an additional negative term that usually causes the net RF to be negative, but it can end up positive. Thus, competing large terms of opposite sign resulting in a small number is the cause of the large uncertainty.
Robert Portmann, NOAA ESRL.
Response: The RF uncertainties are discussed in Section 4.2.2 with a statement in the text and the addition of Box 4.2.

Chapter 4, Page 248, Line 5495-5497: Why is methane's effect on water vapor not clear? It is clear that methane change cannot explain all of the change in water vapor. And why do you say "indirect effect"? It seems like a direct effect on water vapor. Maybe I missed something here?
Robert Portmann, NOAA ESRL.

Response: Sentence is now removed.

CHAPTER 5

Chapter 5: Overall this is a good chapter. I do think there is too much emphasis on "return to 1980's EESC" concept. The exact year EESC starts affecting ozone is not known. Certainly there was some effect in the 1970's but it is hard to determine above the variability. Thus, there needs to be discussion that the "return to 1980" concept is approximate and while useful for illustration not to be pushed too far. Also, there does not seem to be a definition of "ozone recovery" in this chapter, as was done in the WMO 2006.
Robert Portmann, NOAA ESRL.
Response: Thank you for the comments. In response to the reviewer's comment, we added some text to explain the rationale for identifying the EESC recovery date and why ozone recovery cannot be expected on the same date. In addition, we make a distinction between the model-simulated ozone recovery date and the trend-derived halogen-induced ozone recovery date in the text as possible definition for ozone recovery date. This should help here and in the comment on lines 6270-6272 below.

Chapter 5, Page 271, Line 6118-6120: How did you come up with the "five to ten years" mentioned here? Five to ten years in the middle of the century amounts to only a few years in the late seventies in terms of EESC. So this is like saying "we need to wait until we get to 1980's EESC and then the effect on ozone will be minimal when we get to 1978's EESC". Anytime a year of recovery is mentioned it should be understood that this is approximate since we cannot determine the exact year EESC started to affect ozone.
Robert Portmann, NOAA ESRL.
Response: The 5 to 10 years statement was from the point of view of trend detection, not from the EESC point of view. We have reworded the sentence to clarify the definitions for date for the ozone recovery in terms of trend in ozone.

Chapter 5, Page 273, Line 6165-6166: This bullet does not present a key finding and should be removed.
Robert Portmann, NOAA ESRL.
Response: Agreed. It is deleted.

Chapter 5, Page 274, Line 6169, 6170: Very minor: change W per m2 to W m-2, here and elsewhere.
Robert Portmann, NOAA ESRL.
Response: We did not make this change because the editorial style guide calls for the format of "W per m^2."

Chapter 5, Page 278, Line 6270-6272: This depends on how you define "recovery". If recovery is defined in terms of EESC's effect on ozone after removal of other effects then the other factors will not make the recovery date from EESC earlier. This chapter seems to avoid making a formal definition.

Robert Portmann, NOAA ESRL.
Response: This part has been rewritten to clarify the points noted.

Chapter 5, Page 281, Line 6332-6334: In some interactive 2-D models the circulation depends on wave drag as well as the heating rates. Also, since the propagation of waves through the wind field is included, some feedbacks with wave forcing are simulated (but not changes in wave sources). Thus, this description is too strongly worded and makes interactive 2-D models into less than they can be.
Robert Portmann, NOAA ESRL.
Response: The sentence has been rewritten to clarify how some 2D models work; specifically, the allowance for the interaction of planetary waves with the mean circulation is noted.