Review of the U.S. Climate Change Science Program's Draft Synthesis and Assessment Product 2.4: Trends in Emissions of Ozone Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure

Committee to Review the U.S. Climate Change Science Program's Draft Synthesis and Assessment Product 2.4, National Research Council

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Committee to Review the U.S. Climate Change Science Program's Draft Synthesis and
Assessment Product 2.4
Board on Atmospheric Sciences and Climate
Division on Earth and Life Studies

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This report has been reviewed in draft form by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the National Research Council's Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the institution in making its published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their review of this report:

James G. Anderson, Harvard University, Cambridge, Massachusetts
Greg Bodeker, National Institute of Water & Atmospheric Research Limited, Auckland, New Zealand
Mary Anne Carroll, University of Michigan, Ann Arbor
Veronika Eyring, Institut für Physik der Atmosphäre, Wessling, Germany
Vitali Fioletov, Environment Canada, Downsview, Ontario, Canada
Ross J. Salawitch, University of Maryland, College Park

Although the reviewers listed above have provided many constructive comments and suggestions, they were not asked to endorse the conclusions or recommendations, nor did they see the final draft of the report before its release. The review of this report was overseen by Marvin Geller, The State University of New York, Stony Brook. Appointed by the National Research Council, he was responsible for making certain that an independent examination of this report was carried out in accordance with institutional procedures and that all review comments were carefully considered. Responsibility for the final content of this report rests entirely with the authoring committee and the institution.

Contents

Summary

The National Research Council (NRC) empanelled an ad hoc committee to review the U.S. Climate Change Science Program's (CCSP) draft Synthesis and Assessment Product (SAP) 2.4, *Trends in Emissions of Ozone Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure* (draft dated August 20, 2007). The National Oceanic and Atmospheric Administration asked the NRC committee to review SAP 2.4 based on criteria from the NRC's own review process for NRC reports. This committee has prepared the present report to point out the strengths of SAP 2.4 and to provide some suggestions for improving it.

SAP 2.4 is noteworthy as the first-ever attempt to assess the specific contribution of the United States to ozone-depleting substances and ozone recovery. Scientifically objective and policy neutral, the analysis is well-grounded in the international consensus of recent ozone assessments. The SAP authoring team has incorporated more information on ozone-climate interactions than has been done in previous assessments, advancing this approach in the right direction for future assessments. An impressive expert team has been assembled to provide comprehensive coverage of the scientific basis, and the committee acknowledges the unprecedented degree of interagency coordination involved with this effort. In particular, the committee appreciates the major effort required to provide the amount of detail in Chapter 2 of the SAP and to bring the information together. Chapter 2 of the SAP represents an excellent synthesis of the available material and adds value to what is already known.

The committee presents five overarching comments to address issues that span more than one section of the SAP. Therefore, a number of important major comments that are technical and specific to a single chapter are not mentioned in the following overarching comments.

- The authoring team should revise its discussion of the climate effects of ozone. In Chapter 4 of the draft SAP, the authoring team brings up the issue of the effects of ozone on climate without fully pursuing this topic because it is beyond the scope of this SAP. Nonetheless, the authoring team should include a more complete discussion that clearly states that ozone is part of the climate system and that explains the ozone-climate connection at the process level. Ozone is important to the climate system both through its absorption of solar radiation and as a greenhouse gas through its absorption of infrared radiation. Ozone is not only a greenhouse gas, but is the third most important greenhouse gas in the natural climate system after water vapor and carbon dioxide.

- The authoring team should revise its approach in estimating U.S. contributions to production, consumption, and emissions of ozone-depleting substances (ODSs). Specifically, for emissions of chlorofluorocarbons (CFCs) prior to 1975, the U.S. contribution should be estimated as a minimum of one-third and a maximum of two-thirds of global emissions for each type of CFC; post-1975 estimates should also be adjusted accordingly. The justification for this approach begins with the fact that the United States was a major consumer of fluorochemicals before EPA estimates became available in 1985. After the publication of the ozone depletion theory in 1974, there was a discontinuity in the use patterns of CFCs (namely, CFC-11 and CFC-12)—of which 70% of global use was as an aerosol propellant. Although U.S. consumption data are not available prior to 1985 there is information on which to base a more realistic estimate. There was almost a complete elimination of the use of CFCs as a propellant in the United States after 1974, while many other countries continued use of the CFCs as propellants. Therefore, the U.S. fraction of global emissions of CFCs for 1985-1990, as represented in the SAP, is not an accurate representation of the U.S. fraction of emissions prior to 1975, contrary to the approach presented in the draft SAP. The committee's recommended changes will provide a more realistic range of the U.S. contribution and will affect how U.S. contributions are estimated throughout the SAP.

- To address the SAP goal to "describe how these findings relate to human activities, with a particular focus on the United States," the authoring team should include a discussion of scientific issues that have policy implications. The draft SAP contains policy relevant information that should be stated more clearly as such in Chapter 6 and should be brought forward to the Executive Summary in a policy neutral manner. This proposed approach will make the SAP more useful for policymakers.

- To address the SAP goal to "identify where research supported by CCSP agencies is critical for future assessments," the authoring team should highlight research needs in each chapter where the research is discussed. Suggestions for accomplishing these goals appear in the committee's specific comments for each SAP chapter.

- The authoring team should specifically identify and accommodate the intended audiences of the SAP. The committee suggests some ways of reorganizing and editing the sections of the draft SAP in the following chapters of this report, including suggestions about the use of introductory material and the explanation of technical concepts.

The committee commends the SAP authoring team on developing a comprehensive document covering the scientific basis of this important climate issue. For the final SAP document, the authoring team would meet its goals and provide rigorous answers to its key questions by incorporating the committee's suggestions detailed in this report.

1

Introduction

The U.S. Climate Change Science Program (CCSP) was established in 2002 to coordinate climate and global change research conducted in the United States. Building upon and incorporating the U.S. Global Change Research Program of the previous decade, the program integrates federal research on climate and global change, as sponsored by 13 federal agencies and overseen by the Office of Science and Technology Policy, the Council on Environmental Quality, the National Economic Council, and the Office of Management and Budget. A primary objective of the CCSP is to provide the best possible scientific information to support public discussion and government and private sector decision making on key climate-related issues.

To help meet this objective, the CCSP is producing a series of Synthesis and Assessment Products (SAPs) that address its highest priority research, observation, and decision-support issues. The CCSP is conducting 21 such activities, covering topics such as the North American carbon budget and implications for the global carbon cycle, coastal elevation and sensitivity to sea-level rise, and use of observational and model data in decision support and decision making. Each of these documents will be written by a team of authors selected on the basis of their past record of interest and accomplishment in the given topic. A list of the CCSP SAPs is provided in Appendix A.

The purpose of SAP 2.4, the subject of this report, is to address key issues related to the stratospheric ozone layer, including its changes in the past and expected abundance in the future. Also, this SAP will address current abundances and emissions of ozone-depleting substances. Further, it will synthesize the best available information on the past and future levels of ultraviolet radiation at the Earth's surface. Lastly, it will explore the interactions between climate change and stratospheric ozone changes. The SAP will address all of these issues within a U.S. context to distill a national assessment from two global assessments: the World Meteorological Organization 2006 assessment on the ozone layer (WMO 2007) and the 2005 Special Report of the Intergovernmental Panel on Climate Change, *Safeguarding the Ozone Layer and the Global Climate System – Issues Related to Hydrofluorocarbons and Perfluorocarbons* (IPCC/TEAP 2005). A prospectus for SAP 2.4 was developed to describe the goals and the questions to be addressed (see (CCSP 2007).

In a review of the CCSP Strategic Plan, the National Research Council (NRC) recommended that SAPs should be produced with independent oversight and review from the wider scientific and stakeholder communities (NRC 2004). As part of its efforts to meet this goal, the National Oceanic and Atmospheric Administration (NOAA) requested that the NRC provide an independent review of SAP 2.4. The NRC appointed an ad hoc committee composed of nine members to provide this review (see Appendix B). The committee's Statement of Task is in Box 1.1. The committee conducted its work by reading the draft SAP 2.4 document, *Trends in Emissions of Ozone Depleting Substances,* *Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure* (draft dated August 20, 2007). Next, the committee met with the SAP authoring team, who provided in-depth presentations of their research and material used to formulate the draft document. The committee met in closed session to discuss the draft SAP in detail. This present report constitutes the committee's peer review of SAP 2.4 and results from the committee's careful study of the draft and its interactions with those present at the meeting. This report includes the committee's findings, recommendations, suggestions, and options for the authors to consider in finalizing SAP 2.4.

BOX 1.1
The Statement of Task for the NRC Committee

The committee will address the following review criteria (which are based on the NRC's own review criteria):

1. Are the goals, objectives and intended audience of the product clearly described in the document? Does the product address all questions outlined in the prospectus?
2. Are any findings and/or recommendations adequately supported by evidence and analysis? In cases where recommendations might be based on expert value judgments or the collective opinions of the authors, is this acknowledged and supported by sound reasoning?
3. Are the data and analyses handled in a competent manner? Are statistical methods applied appropriately?
4. Are the document's presentation, level of technicality, and organization effective? Are the questions outlined in the prospectus addressed and communicated in a manner that is appropriate and accessible for the intended audience?
5. Is the document scientifically objective and policy neutral? Is it consistent with the scientific literature?
6. Is there a summary that effectively, concisely and accurately describes the key findings and recommendations? Is it consistent with other sections of the document?
7. What other significant improvements, if any, might be made in the document?

The following two chapters provide detailed comments for improving SAP 2.4. Chapter 2 of this report presents the committee's overarching comments, which span more than one section of the SAP. Chapter 3 of this report provides both major and specific comments for the individual sections of the SAP.

REFERENCES

CCSP (U.S. Climate Change Science Program). 2007. *CCSP Synthesis and Assessment Product 2.4: Prospectus for Trends in Emissions of Ozone-Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure*. Washington, DC: U.S. Climate Change Science Program. Available online: http://www.climatescience.gov/Library/sap/sap2-4/sap2-4prospectus-final.pdf.

IPCC/TEAP (Intergovernmental Panel on Climate Change/Montreal Protocol's Technology and Economic Assessment Panel). 2005. *Safeguarding the Ozone Layer and the Global Climate System: Issues Related to Hydrofluorocarbons and Perfluorocarbons*. IPCC/TEAP Special Report. Geneva: IPCC. Available online: http://arch.rivm.nl/env/int/ipcc/pages_media/SROC-final/SpecialReportSROC.html.

NRC (National Research Council). 2004. *Implementing Climate and Global Change Research: A Review of the Final U.S. Climate Change Science Program Strategic Plan*. Washington, DC: National Academies Press. Available online: http://www.nap.edu/catalog/10635.html.

WMO (World Meteorological Organization). 2007. *Scientific Assessment of Ozone Depletion: 2006*. Global Ozone Research and Monitoring Project - Report No. 50. Geneva. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/index.shtml.

2

Overarching Comments

In this chapter, the committee provides five overarching comments on the U.S. Climate Change Science Program's (CCSP) draft Synthesis and Assessment Product (SAP) 2.4, *Trends in Emissions of Ozone Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure* (draft dated August 20, 2007). These overarching comments span more than one section of the SAP and address how responsive the authoring team was to their prospectus. Therefore, a number of important major comments that are technical and specific to a single chapter are not mentioned in the overarching comments below. In some cases, the comments on the separate sections of the draft SAP (see Chapter 3 of this report) offer detailed suggestions on how to address the overarching comments presented here.

RELATIONSHIP BETWEEN OZONE AND CLIMATE

Although the prospectus for SAP 2.4 (CCSP 2007) does not address the effects of ozone on climate, radiative forcing from ozone-depleting substances (ODSs) and their substitutes is covered in the SAP. The authoring team has indicated that changes in their approach to the SAP were made after the prospectus was finalized. Thus, the authoring team should explicitly acknowledge that this aspect of the SAP goes beyond the scope of the prospectus. The authoring team decided not to go further than radiative forcing in terms of climate effects, and they stated that the reason was that the 2006 climate change assessment (IPCC 2007) was not yet available when this SAP was developed. The committee appreciates this reasoning. However, the two-way coupling between ozone and climate is becoming increasingly important for attribution and, hence, for policy, and scientific research is evolving more and more in this direction. That was indeed a major rationale behind IPCC/TEAP (2005), a key reference document for this SAP. In Chapter 4 of the draft SAP, the authoring team brings up the issue of the effects of ozone on climate without fully pursuing this topic. The authoring team should include a more complete discussion that clearly states that ozone is part of the climate system and explains the ozone-climate connection at the process level. The authoring team should also clearly state that separating the treatment of the ozone depletion, ozone recovery, and climate change problems is scientifically artificial, and that from a scientific perspective, an integrated approach is called for in the future. The committee realizes that this SAP can make only a tentative first step in this respect, but it is nevertheless an important step.

As detailed in the committee's major comments for Chapter 4 of the draft SAP (see Chapter 3 of this review), the authoring team should include additional discussion of climate change scenarios, rather than just sticking to the single baseline Special Report on

Emissions Scenario (SRES) A1B. Use of a single scenario represents a major uncertainty for future ozone projections. Although the single baseline SRES A1B scenario is the only scenario that the Community Climate Model (CCM) modelers ran for the 2006 WMO/UNEP ozone assessment, the 2D models were run on a range of scenarios. Perhaps the authoring team could select one of the interactive 2D models from the assessment, benchmark it against the better CCMs, and then use the results from the full range of scenarios to include additional discussion of climate change scenarios. The authoring team should also explicitly state that they are not considering how drastic changes that might take place in the climate system might affect the ozone problem.

ASSESSING U.S. CONTRIBUTIONS TO OZONE-DEPLETING SUBSTANCES AND RADIATIVE FORCING

There are inconsistencies in the draft SAP in estimating U.S. contributions to production, consumption, and emissions of ozone-depleting substances (ODSs). There is also inconsistency in the draft SAP with regard to the confidence to be placed on these emission estimates:

- Regarding **production and consumption**, there should not be inconsistencies in the numbers because they are obtainable from the World Meteorological Organization report, *Scientific Assessment of Ozone Depletion: 2006* (WMO 2007). Lines 590-594 of the draft SAP state that "…during 1986-1994 the U.S. accounted for 24-30% of total annual production and consumption of ODSs reported by the United Nations Environment Programme (UNEP) when weighted by the ozone depletion potentials (ODPs), since 2001 this fraction has been closer to 10%". However, the numbers are given as 25-30% and "somewhat less since" on lines 1966 and 1967; lines 938-948 state 25-30% prior to 1993 and about 10% in 2001-2005.
- Regarding **emissions**, the Executive Summary of the draft SAP does not provide any numbers (E.S.3.5, L. 288-292); lines 607-614 only state that U.S. emissions have declined by 81% since the 1980s. Lines 1978-1979 state that "…this analysis suggests that the U.S. accounted for 25% of global emissions, on average, during the 1990s, and somewhat less since". Lines 1240-1254 give a consistent number of 18-35% for the 1990s (the committee's interpretation of line 1252), but this text does not give a number for the more recent proportion of emissions from the United States. Lines 5214-5217 state that "Between 1985 and 2005, the fraction of ozone-depleting substances weighted by ODPs emitted by the U.S. relative to the total global emissions varied from about 20% in 1985 to a maximum of about 35% in the early 1990s, to a current level of roughly 20%".
- Regarding the U.S. contribution to global **atmospheric mixing ratios**, the authoring team should correct the following inconsistencies. The Key Findings section of Chapter 2 gives the U.S. emissions to total tropospheric chlorine (on L. 658) as approximately 20 (14-32)%, and 23 (19-29)% to bromine (line 680); however, line 1984 states 16-30% for chlorine and 21-26% for bromine. Line 1634 gives a number of 21 (16-30)% for chlorine for the past decade, and on line

1703, the proportion for bromine "has increased during the 2000s from 23 to 26%".

The committee is concerned with the inconsistency of the figures stated in the draft SAP and the use of "arbitrary uncertainties" applied to the development of scenarios and the quantification of future projections (e.g., P. 73, L. 1496). In estimating the U.S. contribution to past and future equivalent effective stratospheric chlorine (EESC) and radiative forcing, the method employed in the draft SAP is unjustified. Considering past contributions, the United States was a major consumer of fluorochemicals prior to 1985. Although the lack of available data presents significant uncertainty, it is reasonable to estimate that the U.S. contribution was roughly one-half of global emissions prior to 1975. To account for the uncertainty, assigning a range of error from a minimum of one-third of global emissions for each species up to a maximum of two-thirds will encompass the "rough estimate" of one-half of global emissions prior to 1975. After the publication of the ozone depletion theory in 1974 (Molina and Rowland 1974), there was a discontinuity in the use patterns of CFC-11 and CFC-12. In 1974, about 70% of global use of chlorofluorocarbons (CFCs) was as an aerosol propellant. Following the publication, there was almost a complete elimination of the use of CFCs in this application in the United States while many other countries continued use of the CFCs as propellants. Therefore, the U.S. fraction of global emissions of CFCs for 1985-1990 is not an accurate representation of the U.S. fraction of emissions prior to 1975, contrary to the approach presented in the draft SAP. Although pre-1975 information is indeed uncertain, as noted in the draft SAP, the committee judges that a more reasonable range of the U.S. fraction of emissions prior to 1975 is a lower bound of one third and an upper bound of two thirds of total global CFC-11 and CFC-12 emissions.

Based on this information and the following points, the committee recommends a different approach to calculating the U.S. fractional contribution to global EESC and radiative forcing:

- Of the anthropogenic ozone depleting substances, CFC-11 and CFC-12 are the major contributors to both EESC and radiative forcing through 1975. This is shown by the information in Table 2.2 and Figure 2.16 where these CFCs dominate contributions to chlorine, and hence, EESC and radiative forcing as well—even through 1985. To further support the point, Table 2.2 should be modified as follows:
    - Add a column for 1975.
    - Show EESC for each of the compounds for each of the years.
    - HCFC-123 and any other hydrochlorofluorocarbon (HCFC) for which data exist should be added for completeness. Table 2.2 and its footnotes do not explain why HCFC-123 is not included except that other contributions are likely to be small. Although Daniel and Velders (2007; on which Table 2.2 is based), did not include HCFC-123 in scenario A1, the numbers may be derived.

- The use and emissions of other ODSs did not undergo the discontinuity as in the case for CFC-11 and CFC-12. Thus, the U.S. fraction of global emissions of these other ODSs for 1985-1990 is a reasonable representation of the U.S. fraction of emissions prior to 1985.

The committee recommends modifying the approach to estimating the U.S. contributions to atmospheric concentrations (as shown in Figure 2.11) as described below. U.S. fractional contributions to EESC and radiative forcing can then be calculated as ranges by taking the ratios of the values calculated for EESC and radiative forcing from those concentrations to the global values.

- For CFC-11 and CFC-12:

	Proposed approach	Approach in draft SAP
pre-1975	Lower and upper bounds representing U.S. emissions as one-third and two-thirds of global emissions for each species	Lower and upper bounds representing U.S. emissions as half and twice the 1985-1990 average U.S. fraction of global emissions for each species
1975-1985	Assume two scenarios: U.S. emissions as half and two times the 1985-1990 average U.S. fraction (from Environmental Protection Agency [EPA] vintaging model) of global emissions for each species	From the lower bound, continued to 1985 and beyond using EPA vintaging model minus 20%; from the upper bound, continued to 1985 and beyond using EPA vintaging model plus 20%
1985 and beyond	Assume EPA vintaging model estimates of U.S. emissions with 20% error bars starting from each of the two points defined by the scenarios described above	

- For all other ODSs:

	Proposed approach	Approach in draft SAP
Pre-1985	Assume two scenarios: U.S. emissions as 1/1.5 times and 1.5 times the 1985-1990 average U.S. fraction (from EPA vintaging model) of global emissions for each species	Lower and upper bounds representing U.S. emissions as half and twice the 1985-1990 average U.S. fraction of global emissions for each species
1985 and beyond	Assume EPA vintaging model estimates of U.S. emissions with 20% error bars starting from each of the two points defined by the scenarios described above	From the lower bound, continued to 1985 and beyond using EPA vintaging model minus 20%; from the upper bound, continued to 1985 and beyond using EPA vintaging model plus 20%

The text should clearly describe the rationale outlined above, stating that this approach is used because no data currently exists to better define U.S. contributions and commenting that this is only a rough estimate based on plausible scenarios.

The upper and lower bounds of the combined scenarios can then be used to calculate ranges for use in Figures 2.12, 2.13, 2.14 and 2.15. The range should be shown as a band without showing a mean since a mean has no real meaning given the uncertainties. To the extent possible, all figures showing consumption (consumption to be shown instead of production) should be extended to 2020 to be consistent with Chapter 5 of the draft SAP.

As the draft SAP points out (L. 1300), there are substantial differences (factor of two) between the model estimates and estimates made from measurements over the United States during regional pollution events. Admittedly, these differences could be caused by non-representative observational sampling for the entire United States. Nevertheless, the problem is that the accuracy of the vintaging model estimates is not established and an "arbitrary uncertainty of 20% is assumed" (line 1275). The draft SAP should emphasize the uncertainty in U.S. emission estimates in the summary sections. (Also, see Chapter 3 in this report.)

Overall, the draft SAP does not provide enough discussion of uncertainties in current understanding and projections. For this document, a thorough discussion would be appropriate, as the committee has outlined above.

RESEARCH NEEDS

Regarding the overall goals of the report, the report did not "identify where research supported by CCSP agencies is critical for future assessments," as required in Section 1.2 of the prospectus for SAP 2.4 (CCSP 2007). Because the focus of the SAP is on the United States, the authoring team should begin addressing this goal by outlining the significant contributions that U.S.-funded research (e.g., satellite programs, aircraft missions, laboratory studies, modeling efforts) have made to the world's understanding of the ozone layer. To identify research needs that are evident from our current understanding, some suggestions are as follows:

- Continued observations to monitor ozone recovery and to allow an attribution of the separate effects of decreasing halogens and of climate change. The latter, for example, stresses the importance of measuring changes in the vertical profile of ozone.
- An improved ability to quantify the impact of short-lived compounds on lower stratospheric ozone.
- The development of comprehensive, self-consistent chemistry-climate models (coupled to oceans, tropospheric chemistry, etc.).
- Process-based observational studies for model validation
- Maintaining a capability to address specific uncertainties in chemical processes or atmospheric concentrations, such as chlorine peroxide (ClOOCl) photolysis rates, the abundance of atmospheric bromine, and fugitive gas emissions from new technologies.

Recently, the U.S. Climate Change Science Program hosted a workshop in Washington, DC, on the topic of future research needs for stratospheric ozone. Because this committee has not seen the workshop proceedings, it is not possible to assess the extent to which they might help meet the requirement of this prospectus. The SAP authoring team might consider ultimately attaching those proceedings as an appendix to this SAP. However, the committee regards it as important that the prospectus be addressed in a seamless manner, tying the research needs to the scientific analysis in the SAP.

POLICY IMPLICATIONS

Although the point of this product is to provide information for policymakers, the draft SAP lacks a discussion of scientific issues that have policy implications. The draft SAP presents policy-relevant information that should be stated more clearly as such and should be brought forward to the Executive Summary in a policy-neutral manner. Additional policy relevant information should be added that should also be brought forward to the Executive Summary.

Objectives of the assessment include (from the prospectus): “(ii) to provide the scientific basis for decision support to guide management and policy decisions that affect the ozone layer and emissions of ozone-depleting substances;…” “The primary users of SAP 2.4 are intended to include, but are not limited to, officials involved in formulating climate and environmental policy….” Furthermore, the questions to be addressed include, “What are the various possible emissions scenarios that can be considered for any future policy actions on emissions of ozone-depleting gases?” Thus, it is appropriate to look at current policy issues involving ODSs. These issues include:

- HCFCs: allocation of production and consumption allowances domestically and acceleration of the phaseout schedules for developed and developing countries internationally.
- Banks of ODSs: the potential for destruction of quantities of ODSs contained in equipment and products to enhance ozone protection and decrease contributions to climate change.
- Expanding uses of methyl bromide as in quarantine and pre-shipment, and continued controversy over critical use exemptions of methyl bromide.

Relevant information in the sections should be made clearer relative to these issues, this information should be expanded, and it should be carried forward to the Executive Summary, all in a policy neutral manner. The information should include the U.S. fraction of past and projected future consumption and emissions of ODSs, as well as related by-product emissions and substitutes for ODSs. These numbers should be expressed in absolute amounts, ODP weighted amounts, and GWP weighted amounts. This information will show how U.S. actions have contributed to the success of the Montreal Protocol in achieving its goals to protect stratospheric ozone and the unintended consequence of climate protection. The information will also provide a partial scientific basis for decisions on the issues listed above. Information from other chapters will provide additional basis for those decisions.

PRESENTATION AND ORGANIZATION

One of the committee's tasks is to determine whether SAP 2.4 is effectively presented to its intended audiences. Keeping in mind the intended audiences described in the prospectus, the draft SAP does not provide enough introductory information in each chapter for the key points to be understood by a non-technical audience. One suggestion is to include WMO's 20 questions document (Fahey 2007) as an appendix in the SAP, since it is written for a broad audience. The authoring team should include pedagogical introductory information and contextual language for the key issues and key findings. Chapter 3 (Review of Individual Sections) of this review provides additional suggestions for introducing essential concepts in each chapter of the draft SAP. As a matter of organization, it is difficult for the reader to correlate the key issues and key findings at the beginning of each SAP chapter to the associated discussions within the chapters. The authoring team should establish a clearer relationship among the key issues, key findings, and associated discussion for each topic.

REFERENCES

CCSP (U.S. Climate Change Science Program). 2007. *CCSP Synthesis and Assessment Product 2.4: Prospectus for Trends in Emissions of Ozone-Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure*. Washington, DC: U.S. Climate Change Science Program. Available online: http://www.climatescience.gov/Library/sap/sap2-4/sap2-4prospectus-final.pdf.

Fahey, D.W. 2007. *Twenty Questions and Answers about the Ozone Layer: 2006 Update*. Geneva: World Meteorological Organization. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/Twenty_Questions.pdf.

IPCC/TEAP (Intergovernmental Panel on Climate Change/Montreal Protocol's Technology and Economic Assessment Panel). 2005. *Safeguarding the Ozone Layer and the Global Climate System: Issues Related to Hydrofluorocarbons and Perfluorocarbons*. IPCC/TEAP Special Report. Geneva: IPCC. Available online: http://arch.rivm.nl/env/int/ipcc/pages_media/SROC-final/SpecialReportSROC.html.

IPCC (Intergovernmental Panel on Climate Change). 2007. *Climate Change 2007 - The Physical Science Basis: Working Group I Contribution to the Fourth Assessment Report of the IPCC*. Cambridge: Cambridge University Press.

Molina, M.J., and F.S. Rowland. 1974. Stratospheric sink for chlorofluoromethanes: chlorine atomc-atalysed destruction of ozone. *Nature* 249:810−812.

WMO (World Meteorological Organization). 2007. *Scientific Assessment of Ozone Depletion: 2006*. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/index.shtml.

3

Review of Individual Sections

This chapter provides detailed suggestions for revising the Executive Summary and six chapters of the draft Synthesis and Assessment Product (SAP) 2.4: *Trends in Emissions of Ozone Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure* (draft dated August 20, 2007). The review of each section begins with the committee's major comments, followed by a list of specific comments. The major comments for each SAP section highlight issues that need significant attention and may often relate to the issues raised in the "Overarching Comments" of this peer review report. In some cases, the specific comments that follow the major comments further relate to issues raised in the major or overarching comments; in other cases, these specific suggestions are relatively minor.

EXECUTIVE SUMMARY

The committee suggests many changes to the organization and content of the draft SAP and expects that many chapters of the draft SAP will be revised. Accordingly, the Executive Summary should be revised to reflect the major issues of chapters 1-6 in the final SAP. Regardless of which suggestions may be adopted by the SAP authoring team for the SAP chapters, the Executive Summary should be rewritten with consideration for the following comments.

Major Comments

The purpose of an Executive Summary is to highlight the major points of the document in a style that is organized and accessible for a variety of audiences. The Executive Summary in the draft SAP addresses specific and minor points while failing to highlight the major points. It is also not written for the intended audiences identified in the SAP prospectus:

> "The audience for SAP 2.4 includes decisionmakers in the public (Federal, State, and local governments) and private realms (chemical industry, transportation and agriculture sectors, and climate policy and health-related interest groups), scientists, the international community, and the general public (CCSP 2007)."

The "Key Findings" section of the Executive Summary is organized as a partial outline of the chapters with a selection of bullets that have been taken—sometimes verbatim—from key findings throughout the chapters of the SAP. Within the context of the SAP chapters, these bullets are understandable; but, as presented in the Executive

Summary, they are not. Subheadings in this section are confusing. Fewer bullets should be presented in the Executive Summary (ES), and each bullet should be a synthesis of the separate but related key findings from the chapters. The key findings also include a lot of repetition; for example key findings on global and polar ozone based on observations are provided under ES.3.2 and repeated under ES.3.4. The authoring team should consider focusing on key statements of the future only in ES.3.4.

The ordering of the key questions appears to be random. The Executive Summary should present the key questions in a logical order. For example, in Section ES.3.1 (beginning on SAP P. 6), the production bullet (L. 136-140) should come before the bullet on abundances (L. 123-135). In addition, the bullet on radiative forcing (L. 141-146) would seem to belong in Section ES.3.4 (beginning on SAP P. 9).

To fulfill its purpose, the Executive Summary should be restructured to accommodate its intended audiences. One restructuring option is to present each key question directly followed by an associated finding, as in the "Summary for Policymakers" in the Intergovernmental Panel on Climate Change/ Technology & Economic Assessment Panel (IPCC/TEAP) Special Report, *Safeguarding the Ozone Layer and the Global Climate System: Issues Related to Hydrofluorocarbons and Perfluorocarbons* (IPCC/TEAP 2005). With this option, each finding is more closely linked with the key question it addresses, as recommended in Chapter 2 of this review for all SAP sections. Another restructuring option is to organize the Executive Summary in three parts to address three audience categories: (1) "Recent Major Findings and Current Scientific Understanding" to inform the international community and the public in general, (2) "Additional Scientific Evidence and Related Information" to inform scientists, and (3) "Implications for Policy Formulation" to inform decision-makers. An example of this option is the Executive Summary in the World Meteorological Organization's (WMO) report, *Scientific Assessment of Ozone Depletion: 2006* (WMO 2007).

In addition to restructuring, the Executive Summary of the draft SAP would benefit from an improved presentation of background information. The authoring team should consider defining key terms related to stratospheric ozone, as presented in SAP Chapter 1, for example. The Executive Summary does not define technical terms, making it confusing for a non-technical audience to read. Some specific examples of such terms are listed in the "Specific Comments" section below. An example of text to omit from the Executive Summary is the discussion in the first few pages about the organization of the report and the context of the report. This text could be moved into the preface when it is eventually written for the final SAP.

Although the Executive Summary of the draft SAP is consistent with the findings of the SAP, it is not as effective as it could be. At a minimum, the Executive Summary should address the objectives and questions as presented in the SAP prospectus (specified in Sections 1.2 and 1.5 of the prospectus).

Specific Comments

- P. 7, L. 136-138, "Total global production and consumption of ozone-depleting substances (ODSs) and substitute chemicals have declined substantially since the late

1980s…", and P. 7, L. 141-143, "The combined radiative forcing (energy that can increase temperature) from ODSs and substitutes including hydrofluorocarbons (HFCs) is still increasing, but at a slower rate than in the 1980s": In the Executive Summary, no distinction has been made among the original ODSs, the HCFC substitutes that have lower ODP, and the HFC substitutes that are not ODSs but have radiative forcing effects. The fact that ozone and ODSs and all substitutes have radiative forcing effects is not explained in the Executive Summary. Some examples of unexplained questions include: Which of these categories have declined since the late 1980s? All of them combined? A subset of combinations of these? Why do we care if the substitutes are declining? In the background information presented in the Executive Summary, these distinctions should be made clearly before presenting these findings.

- P. 7, L. 153: There are significant ozone losses in the tropical stratosphere, as discussed in the WMO report (2007) (see also Randel and Wu 2007).
- P. 7, L. 154: This statement refers to ozone depletion in the upper stratosphere, but P.3, L. 51 says the stratosphere is from 15-35 km (missing the upper stratosphere). These statements should be checked for consistency.
- P. 8, L. 160: The term "stabilized" should not be used, as explained in the section on SAP Chapter 3 comments.
- P. 8, L. 161-165: While "ozone hole area" may be in the realm of common knowledge, the phrases "ozone mass deficit" and "Equivalent Effective Stratospheric Chlorine" are not. "Below average" and "more intense meteorological conditions" are not defined, and the use of "higher minimum" may seem contradictory to the non-technical reader. The language should be simplified here, and any terms that are important but not common knowledge should be defined before use.
- P. 8, L. 170: Either define the term "vortex" in the background information of the Executive Summary, or use more common language in place of this technical term.
- P. 9, L. 180-181: This statement is repeated almost verbatim on P. 13 (L. 283-284). It probably only needs to appear once, and P. 13 may be the best place for it. In addition, the authoring team should consider rephrasing the confusing wording in the phrase "increased by about 7% at the minimum ozone in 1993".
- P. 9, L. 184-185: In addition to ozone-depleting substances, ozone is also a greenhouse gas (see P. 10, L. 208-209). The authoring team should consider adding a bullet up front on the importance of ozone as a climate gas.
- P. 9, L. 186: The statement that the ODS contribution to radiative forcing is 20% of that from carbon dioxide (CO_2) is separated from the statement on P. 7, L. 145 that it is about 14% of the radiative forcing from CO_2, methane (CH_4), and nitrous oxide (N_2O). This discussion should be merged to avoid confusion. Also, radiative forcing is expressed here as a percentage of CO_2, differing from P. 10 (L. 208) where radiative forcing is expressed in units of watts per square meter (W/m^2). Both units should be given or choose only one consistent expression of radiative forcing throughout the report, else these facts fail to give the larger picture to the reader.
- P. 9, L. 187-189: This discussion should be merged with that on the radiative forcing from the substitutes because they are not really separable from a climate point-of-view (i.e., it makes no sense to talk about the radiative forcing of the ODSs in isolation).
- P. 9, L. 188: Change "their" to "ODS".

- P. 9, L. 197-198: This sentence is meaningless. What is meant by "region"—height, latitude, geography? What is meant by "extent of climate change"—the magnitude? The type of change? Where it occurs? The authoring team should consider deleting this sentence.
- P. 9, L. 199: At all altitudes?
- P. 9, L. 199-201: The combination of these two sentences read like increasing methane has contributed to *decrease*, instead of *increase*, in water vapor. The authoring team should revise this text.
- P. 10, L. 208: Delete "of climate change".
- P. 10, L. 220 - P. 12, L. 260: Three sections of bullets describe model results for three latitude regions. There is no reason given for the importance and distinction of these three regions, and some bullets within each section are very similar except for small details. The authoring team should synthesize these bullets about recovery into one single section that explains the reason for the latitude distinctions. Also, why is there no discussion of the dynamical super-recovery in the midlatitudes (especially in the Northern Hemisphere), and the associated permanent sub-recovery in the tropics (as evident from the update to SAP figure 5.2 in Eyring et al. [2007])? This would seem to be an important result.
- P. 10, L. 220: The term "model" has not been explained. Consider using a descriptive phrase, such as: "Three-dimensional chemistry-climate models designed to project future ozone changes…"
- P. 11, L. 226: "up to 15 years earlier" is based on one model; it does not seem to be a representative assessment of the overall Community Climate Model (CCM) results, which seem to suggest recovery will largely follow effective equivalent stratospheric chlorine (EESC) over 60°S-60°N (see the update to SAP figure 5.2 in Eyring et al. [2007]).
- P. 11, L. 227: "The assumed scenario for greenhouse gases" is meaningless without an explanation of this "scenario" and its source.
- P. 11, L. 228: 5% only applies if you look at subregions; over 60°S-60°N, the value is more like 2%. Moreover, this is not super-recovery, as suggested by the text, as the CCMs were also about 2% higher than 1980 values in the 1960s (see the update to SAP figure 5.2 in Eyring et al. [2007]).
- P. 11, L. 233-235: For the audience to whom this report is directed, the contrast between minimum ozone values not starting to increase until 2010 whereas the ozone mass deficit recovers earlier is hard to understand. The authoring team should explain this more clearly.
- P. 11, L. 236-237: The statement is correct, but it needs context: for many of the models, Antarctic ozone follows EESC and so the point is that EESC recovers to 1980 values before 2060-2070. This may be partly realistic (decreasing age of air), and partly unrealistic (young age of air bias).
- P. 11, L. 243: Change "at 2050" to "in 2050".
- P. 11, L. 243-244: This assertion depends on the scenario. The authoring team should be more specific here.
- P. 11, L. 245-247: The text implies that one model predicts worsening Arctic ozone depletion. However, this model cannot be taken seriously, as its EESC is

physically bizarre. (The WMO report did not take it seriously, so their expert judgment can be relied on in this respect.) In an assessment such as this SAP, good information should be sifted from the bad. To be fairly considered, this information should be presented with an assessment of its reliability.

- P. 12, L. 248-260: These three bullets should either be expanded upon, moved, or eliminated. The first bullet uses the term “EESC” and mentions “different scenarios” again without explaining these concepts. In fact, the first sentence of the first bullet is incorrect because changes in CH_4 and N_2O and climate will dominate the future UV trend; furthermore, using the term “more dominated” is confusing and raises the question of more dominated in comparison to what? The second bullet talks about “the new method”, but it is not clear what this is or why it is important or different from a previously used method. The third bullet appears to be a partial restatement of information on P. 7 and 9.
- P. 12, L. 260: “The (Special Report on Emissions Scenario) SRES A1B scenario” is not helpful without an explanation of this “scenario” and its source. Why not give a range of outcomes beyond just one scenario?
- P. 12, L. 268 - P. 13, L. 278: How can ozone in midlatitudes increase if it is influenced by Arctic springtime total ozone values that have been lower than 1980 values? These two bullets seem to present a contradiction.
- P. 13, L. 274-275: What is the basis for the statement that a significant part of the midlatitude ozone decreases over the U.S. have come from the Arctic?
- P. 13, L. 280-284: This sentence is repeated almost verbatim from P. 9, L. 176 - P. 10, L. 181.
- P. 13, L. 286: Are the effects “masked”, or is it just that the signal is too hard to discern from the noise and instrument uncertainties?
- P. 13, L. 288-292: This bullet should include not just U.S. emissions but also production of ODSs by U.S. companies.
- P. 13, L. 289-291: Perhaps the intended meaning of this sentence is better conveyed by rewriting it as follows: “The U.S. has also contributed to … attenuating surface UV changes, and mitigating the radiative forcing of the climate”.
- P. 14, L. 293-295: The accuracy of this statement depends on how much of the bank gets released.
- P. 14, L. 301-306: This bullet should also discuss radiative forcing.
- P. 14, L. 304: Change “would have had” to “would have resulted in”.

CHAPTER 1: INTRODUCTION

In Chapter 1 of the draft SAP, the background information on ozone should be expanded and revised to accommodate readers who may not have technical knowledge in this area. The committee provides some suggestions below for improving the presentation of this background information.

Major Comments

The committee thinks the structure of SAP Chapter 1 would work better by first laying out the role of ozone in the climate system (including ultraviolet [UV]), discussing processes, and then move on to the scope of the report and the logic behind its structure.

Since the general public is one of the audiences of this report, the authoring team should clarify some key issues that can be a source of confusion to the lay person. Otherwise, some people may just conclude that some issues are not well understood. Examples include: carbon dioxide warms the troposphere but cools the stratosphere; lower temperatures increase ozone in the upper stratosphere, but decrease ozone (for current halogen loading) in the polar lower stratosphere; ozone depletion and climate change are distinct issues, but they are related both in terms of physical processes and policy (e.g., trade-offs).

Along the same lines, the authoring team should consider merging some of the important background given in the individual chapters into SAP Chapter 1. The authoring team should consider starting with chlorofluorocarbons (CFCs) and hydrochlorofluorocarbons (HCFCs), the transport of these species to the stratosphere, the differences between CFCs and HCFCs with respect to ozone depletion, their importance as greenhouse gases (GHGs), etc. Then move on to the chemistry and chemistry-climate interactions (as mentioned earlier). More than just gas-phase chemistry is important here. Therefore, the authoring team should consider moving the background information on polar chemistry to SAP Chapter 1, describing the difference between polar and mid-latitude chemistry up front. In any case, this background information comes far too late in Section 3.2.3.2.1 as polar ozone is already discussed in Section 3.2.1.2, for example.

Section 1.2 of the prospectus states that the SAP 2.4 “will explore the interactions between climate change and stratospheric ozone changes”. The committee understands that a quantitative assessment of the impact of stratospheric ozone changes on climate is beyond the scope of the report, but nevertheless, some qualitative discussion is called for. Chapter 1 should provide a broad context for the climate issue to set the stage for a more detailed climate discussion in Chapter 4.

Specific Comments

- P. 15: Instead of stating that the ozone layer “contributes to changes in climate,” the authoring team should simply state that it “plays a significant role in the natural climate system”. L. 321-323: The wording “as well” makes it sound as if “the influence of stratospheric ozone changes on the temperature and its structure in the stratosphere” is different from Point Number 3 in L. 317-318, but they look the same.
- P. 15, L. 315: Instead of the term “harsh”, the authoring team should choose a different, clearer, adjective—such as “damaging”.
- The first figure appearing in SAP Chapter 1 is on P. 16; however, this figure is identified as “Figure 1.2” instead of “Figure 1.1.” Rename this figure as “Figure 1.1” and then renumber the rest of the figures in Chapter 1.
- P. 16, L. 329: “Majority” could be anything over 50%—be up front and say the stratosphere has about 90% of the ozone and that most of the rest is in the troposphere.

- P. 16, L. 339: Depending on the hemisphere, the contribution of tropospheric ozone to total ozone may be around 10% in the northern hemisphere and just a few percent in the southern hemisphere.
- P. 17, L. 345-349: This text repeats what was stated on the previous page. The authoring team should consider rewriting this paragraph.
- P. 18, Figure 1.3: This figure does not fully represent all of the ozone removal processes in the stratosphere. As a remedy, the title of this figure could begin with "A simplified representation of…". In the line for ozone destruction in non-polar regions, there is an oxygen atom without a label, and the label should be added. In both of the lines for ozone destruction, "2 oxygen atoms" should be "2 oxygen molecules" instead. An alternative approach for the authoring team to consider is deleting Figures 1.3 and 1.4 altogether. Rather than focusing on the chemical mechanisms, a single figure could be inserted instead that is more useful for policymakers, simply illustrating that anthropogenic halogens cause stratospheric ozone depletion (perhaps in a top panel) and illustrating that anthropogenic nitrogen oxide (NOx) and hydrocarbons (not volatile organic compounds [VOCs], a lot of which come from trees) cause ozone formation (perhaps in a bottom panel). If the new figure could also indicate that shortwave UV light (which only penetrates the stratosphere) causes a breakdown of CFCs, and that visible light (which can penetrate to the surface) helps produce tropospheric ozone, then we would have a figure that conveyed the essential chemistry to policymakers. Figures 1.3 and 1.4 do not seem helpful for policymakers.
- P. 18, L. 370: Mentioning fire extinguishants seems like far too much detail in this context.
- P. 19, L. 374-376: This sentence repeats what was stated earlier. The authoring team should consider omitting or rewriting this sentence.
- P. 19, L. 377: Transport is indeed very important! It's a zeroth-order effect, in fact. Therefore, a 2D figure (latitude-height cross-section of ozone), such as the Figure in Box 1.2 of the IPCC/TEAP report (2005), would be very helpful to the reader.
- P. 19, Figure 1.4: This figure and the corresponding text are misleading. Both methane and carbon monoxide are examples that are important but not included in this figure. Anything that can convert RO to RO2 or OH to HO2 will have the same effect, not just the VOCs that dominate this process in urban areas. In the reaction equation, ozone should be labeled as "ozone molecules". In any event, everything in Figure 1.4 can be stated in less space in the text, calling the necessity of Figure 1.4 into question.
- P. 19, L. 385: Should this statement read "near UV and visible radiation"?
- P. 19, L. 386: After "UV", add "(shorter wavelength)" to parallel the sentence structure in the previous line, and replace harsh with a clearer adjective as above..
- P. 19, L. 387: Add "stratospheric" before "ozone layer."
- P. 20, L. 393: Up to this point in the chapter, the description of stratospheric chemistry is incomplete. Before introducing CFCs in the next paragraph, this would be a good spot to expand the discussion of stratospheric chemistry, as highlighted above in the "Major Comments" for SAP Chapter 1.
- P. 20, L. 410: Add "in" after "subsequently."
- P. 21, L. 420: Change "long lifetimes" to "lifetimes of many years."
- P. 22, L. 439: Replace "involvement" with "interactions."

- P. 22, Figure 1.1: Discussion of this figure could be expanded to include much more about the role of ozone in the climate system and vice versa. Figure 1.1 lacks methane and N_2O components.
- P. 24, L. 483: Not all CFC substitutes are "climate friendly."
- P. 24, L. 491: After "Perfluorocarbons", add "and referenced therein."

CHAPTER 2: OZONE-DEPLETING SUBSTANCES

Chapter 2 of the draft SAP is comprehensive in its discussion of the production, consumption, emissions, and atmospheric abundances of ODSs and total equivalent chlorine. Although the committee was impressed with the completeness of this review of ODSs, which includes all the information required, it is not presented in an understandable way. Chapter 2 is a long chapter, and its organization was difficult to follow. To aid other readers, the authoring team should consider including an introduction to the chapter and a brief discussion about the organization of the chapter. Chapter 2 should include more thorough explanations throughout, should synthesize information on production, consumption, emissions, and banks, and should explain the various measures of ozone before presenting the key findings. Making the detailed suggestions in the following comments will improve the readability and the accuracy of the synthesis presented in Chapter 2. Chapter 2 of the draft is written at level only understood by halocarbon experts rather than a level accessible for professionals who work at the interface between science and policy.

Major Comments

Background information should be presented at the beginning of the draft SAP Chapter 2, perhaps defining concepts and incorporating parts of the key issues section, before presenting the key findings. As part of this background information, the authoring team should add a box in either SAP Chapter 1 or the introduction to SAP Chapter 2 to explain the life cycle of fluorochemicals, define consumption, and explain the relationship between production, consumption, emission, atmospheric concentrations, and EESC or radiative forcing. Elements of that box should include the following:

- A cartoon similar to Figure SPM-1 of the IPCC/TEAP Special Report on Ozone and Climate (IPCC/TEAP 2005).
- A definition of consumption, in agreement with the Clean Air Act and the Montreal Protocol: Consumption = Production + Imports – Exports. Include a statement that consumption equals production at the global level. Under the Montreal Protocol, the primary control is on consumption and not emissions. This means that there is no control of "banks under the Protocol." Probably not to be stated in the SAP, an issue of note is the growing movement in the United States to control HFCs through control of consumption. The justification and implication of this definition are the basis for focusing on consumption in Chapter

2. Because of the focus on consumption, all SAP figures showing production should show consumption instead.

- A simple explanation of how a vintaging model works to calculate emissions from consumption. This might be done with a link to Box 2.1, with information in that box being generalized to how a vintaging model works with a focus on the Environmental Protection Agency (EPA) model for estimating U.S. contributions. As already recommended in Chapter 2 (Major Comments) of this review, Box 2.1 should be extended to 2020.
- A simple explanation of how atmospheric concentrations are calculated from emissions.
- A simple explanation of how radiative forcing is calculated from concentrations.
- A link to Box 2.2 for EESC.
- A simple explanation of the "bottom-up" method of estimating concentrations vs. the "top-down" method of estimating emissions. It would be important to demonstrate, for one or a few compounds, that the two approaches to estimating atmospheric abundances (i.e., bottom-up/top-down) converge. This could be accomplished with material reported in Chapter 2 or with references to previous work. Perhaps a figure could be used as well. This can then be used to emphasize in the text, and perhaps in the key points, the importance of continued atmospheric concentration measurements (top-down) because they will be needed in the future if the reporting of global emissions of some compounds becomes incomplete.

In addition to the explanation of these concepts, the authoring team should synthesize information on production, consumption, emissions, and banks—perhaps all together in one chart.

The authoring team should add a plot showing the consistency between the measured and expected mixing ratios for compounds where the production numbers are well established. The graphical display of this agreement could be used to indicate that continued measurements are needed in order to have good emissions estimates (in particular, for species where reporting is poor).

The issue on methyl bromide is not brought forward. This issue is the one example where the U.S. is not doing well. Methyl bromide is unique in that it not only has substantial natural contributions, but also has significant unregulated emissions. The committee recommends that Chapter 2 include a "box" focusing on methyl bromide. A figure in the box (perhaps a pie chart) could indicate the sources of methyl bromide (critical use exemption, quarantine and preshipment [QPS], natural, etc.), and how those sources have changed over time. The importance of critical use exemptions and QPS should be highlighted in a concise key finding bullet. The rising contributions (from the United States and other countries) due to these uses have policy implications.

The key findings section should be reworded so that each one is short and concise (a few sentences maximum) with fewer numbers. To summarize the numbers supporting the findings, perhaps a table or figure could be inserted instead. As part of the synthesis of the draft SAP, the authoring team should include a simplified summary statement for the whole chapter.

Specific Comments

- P. 26-28: the Key Issues section should reflect the policy issues described above; P. 29-34: Key Findings Section should address the key issues.
- P. 27, L. 526: As written, the text suggests that HFCs are ODSs. To correct this, add “and their substitutes” after “substances”.
- P. 28, L 554-555: Rewrite this question to read, “What were, what are, and what will be the contributions of the United States to production and emissions of ODSs and substitute chemicals in the past, in the present, and in the future?”.
- P. 29-31, “Key Findings” section: In the first four “Key Findings” bullets, the authoring team should consider removing the portions related to global warming potential (GWP) to the last bullet of this section, where these ideas can be presented with some context and then synthesized.
- P. 29, L. 574 & 577: “ODP-Tons” and “CO_2-equivalents” have not yet been defined at these points in this chapter.
- P. 29, L. 576: Replace “to applications” with “regarding applications” instead.
- P. 30, L. 600-614: In this key findings section, L. 601-602 give proportions of global production in the form of CFCs, HCFCs, and HFCs. The authoring team should also state the values in ODP-Tons and supply the corresponding numbers for the United States.
- P. 33, L. 663-681: This paragraph (and associated text, P. 44 to 47) provides a particularly good explanation of U.S. emissions of methyl bromide, but it is only mentioned in passing in Chapter 6. The authoring team should consider highlighting the importance of this text.
- P. 34, L. 688-689: Replace “EESC calculated” with “calculated EESC” instead.
- P. 36, L. 736: Since fluorine is also a halogen, change “halogen” to “Cl and Br”.
- P. 37, L. 753-757: The differences between the two data sets almost certainly are dominated, according to the supporting information supplied here and subsequently, by the increasingly limited coverage of reporting to Alternative Fluorocarbons Environmental Acceptability Study (AFEAS). This point should be made more clearly in the first sentence.
- P. 37, L. 755-756: Define “Article 5” countries where this is first mentioned.
- P. 38, L. 781-782: A clear distinction should be made here between where the figure caption ends and the chapter text resumes.
- P. 38, Figure 2.2: The U.S.-global ratio going negative, and the underlying information in the chapter, is very difficult to understand. Given the definition of consumption = production + import – export, the only way U.S. consumption can go negative is if inventories are decreased so that export is the dominant factor. This seems very unlikely. Also, what are the baselines used? Changes made to Figure 2.2 should also be reflected on P. 46, L. 939 & 942.
- P. 39, L. 782: Clarify which “substitutes” these are (i.e., whether HFCs are included).
- P. 39, Table 2.1: Here and elsewhere in the chapter, the focus appears to be more on production than consumption. Consumption is the more relevant measure of U.S. contribution to emissions reductions and should be the focus, and the weighted GWP

values should be available. CFC and HCFC data are reported to EPA, and the EPA vintaging model should be able to provide estimates of HFC consumption. Also, here and elsewhere in the chapter, the GWPs used must be specified; those listed in the appendix or those from the IPCC Second Assessment Report (SAR), the values the U.S. uses for purposes of reporting under the United Nations Framework Convention on Climate Change (UNFCCC).

- P. 39, L. 798: Is it correct to state that HCFC production is decreasing instead of increasing? The authoring team should consider including a graph of compound class vs. year, similar to Figure 2.16 (P. 94), except showing production instead of GWP.
- P. 40, Figure 2.3: Explain how carbon tetrachloride (CCl_4) makes ozone (negative ODP). Also, what is the source of the HFC data? It is not the United Nations (UN), as production of HFCs is not reported under either the Montreal or Kyoto Protocol.
- P. 42, Figure 2.4 and elsewhere in the chapter where HFC-23 is discussed, consider the following points:
    - The assumption that production = emissions is wrong for HFC-23. In the U.S., Europe, and Japan, significant amounts of HFC-23 are destroyed. Additionally, some HFC-23 is captured for use as a specialty low temperature refrigerant and as a fire extinguishant.
    - In the U.S., HFC-23 emissions and total HCFC-22 production are reported annually to the EPA in aggregate.
    - The EPA vintaging model should capture the refrigerant and fire extinguishant HFC-23 emissions.
    - Information from the two sources listed above should be used to show trends in U.S. HFC-23 emissions both on an absolute basis and as a percentage of total.
    - Can HFCs other than 23 and 134a be estimated? On P. 43, an estimate is provided. The authoring team should consider carrying this estimate through and including it on Figures 2.3b and 2.4.
    - These comments about HFC-23 also apply to: P. 45 (L. 918-919), P. 58 (L. 1171-1173), and P. 59 (L. 1200-1207).
- P. 42, Figure 2.4 caption: In L. 851, change “Global trends in production” to “Global production”; in L. 852, delete “to these trends”. The figure does not plot trends (although, trends can be inferred from the time series).
- P. 44, L. 897: This is the first use of the term “feedstock” in this chapter, so this should be defined.
- P. 44, L. 903: Is 0.5% a proportion of production for ODSs?
- P. 44, L. 904: HCFC-22 should be added to the list of ODSs used as feedstock.
- P. 44, L. 909: Does an increase of only 9% mean that methyl bromide (MeBr) is a small contributor to ODS?
- P. 46, L. 942: It seems possible that negative consumption for carbon tetrachloride could occur in a single year (errors in the individual contributions or short term lags), but the committee doesn’t see how it can occur for several years.
- P. 46: Should the U.S. contribution be compared to the population?
- P. 46, L. 949: where did the 62% come from? No supporting information could be found for a figure this large.

- P. 46, L. 958-959: This sentence raises an important point that needs to be brought forward to the Executive Summary.
- P. 47, L. 972-975: This section should be rewritten for consumption rather than production. The U.S. EPA should have all information required to report GWP weighted U.S. consumption of ODSs and HFCs.
- P. 48, L. 985-987: This sentence conveys an important point that needs to be brought forward to the Executive Summary.
- P. 48, L. 989-990: As already stated in these comments, the focus should be on consumption. EPA should have the data available from their vintaging model.
- P. 49, L. 1022: Isn't the assumption fairly robust?
- P. 50, L. 1038: Although bottom-up and top-down estimates of the global emissions of the HCFCs overlap, the authoring team should note that the top-down estimates (i.e., from atmospheric observations) possessed smaller uncertainties. The authoring team should consider including a figure from the WMO report (2007) comparing bottom-up and top-down estimates of emissions for CFC-11, CFC-12, and HCFCs.
- P. 51, L. 1055: After the word "decline", add ", respectively".
- P. 53, L. 1098: Be clear that this "additional small contribution" is from the unintended byproduct HFC-23 emission.
- P. 55, Figure 2.7: This figure shows an unusually large decrease in global emissions of CFCs from 2004 to 2005. Because the atmospheric observations do not support such a decrease, and because of the long lifetime of CFCs, the data in this figure should be reconsidered.
- P. 56-57, L. 1154-1157: What about the 90% of the bank that is not accessible? The amount of the CFC bank that may be recoverable is an important policy-relevant conclusion that should be brought forward to the Executive Summary.
- P. 58, L. 1186-1188: Estimates of U.S. consumption of HFCs should be available from the EPA vintaging model.
- P. 59. L. 1200-1202: This sentence is confusing. Is HFC-23 from overfluorination regulated?
- P. 60, Section 2.2.5: Is this section a summary, or new material, or some of both? It is unclear what the point of this section is. Perhaps this section could be moved up in the chapter as part of the introductory material. "Vintaging model" should be defined at the outset of this section.
- P. 61, L. 1242: At this point in the text and in all other instances in this chapter where the phrase "fully revised and amended Montreal Protocol" appears, replace this phrase with "adjusted and amended Montreal Protocol" instead.
- P. 61, L. 1248-1249: This sentence conveys an important conclusion that should come forward to the Executive Summary.
- P. 62, Section 2.2.6: Is this the vintaging section? Is vintaging needed just to get the U.S. emissions?
- P. 64, L. 1317: Instead of stating "a small number", be more specific.
- P. 64, L. 1319: Take out this reference to Figure 2.10, and move the placement of Figure 2.10 from P. 65 so that it appears after the appropriate figure call-out on P. 66 (L. 1353).

- P. 71, L. 1458: Specifically, how were the ODS lifetimes altered—was there an increase or a decrease?
- P. 73-74, Box 2.1: This box does not show how a box model works. The authoring team should consider including something like a cartoon of a box model with a simple equation. Also, this box derives atmospheric mixing ratios of ODSs owing to U.S. emissions, but P. 47 seems to say these data are not available. The authoring team should check for consistency.
- P. 73, L. 1496: Some justification for "we have arbitrarily assigned an error of ±20%" should be given. Also, what confidence interval does this represent? (Also, see Chapter 2 [Overarching Comments] of this review.)
- P. 75, Figure 2.11: The y axes should be labeled. Also, what is the confidence interval for the upper and lower bounds? The HFC-125 emissions estimate should be checked; it is unlikely that the U.S. fraction of the world is that high a since the U.S. is slower than other developing countries in adopting the ODS replacement blends containing HFC-125.
- P. 76-77, L. 1540-1562: This background information should appear much earlier in the chapter, perhaps in the chapter introduction.
- P. 76-81, Section 2.4.1: Total chlorine needs to be defined. What exactly is it? Also, this section seemed to go back and forth between organic and inorganic chlorine, making it confusing to read. Is total chlorine the sum of all CFCs, HCFCs, etc.? And somehow measurements of hydrochloric acid (HCl) are used to check total chlorine? Not clear.
- P. 79: Considering the two statements, "62% in 2004; Table 2.2" (L. 1609) and "In 2004 …" (L. 1616), the results for 2004 do not, in fact, appear in Table 2.2. The text here should be changed from "2004" to "2005", which is the year given in Table 2.2 (P. 69).
- P. 79, L. l614-1616 and elsewhere in the chapter: To state that the substance "declined fairly steadily at -1 ppt" implies an increase, a double negative that should be corrected.
- P. 80, L. 1631: Add "chlorine containing" before "replacements".
- P. 81, Figure 2.12 and P. 88, Figure 2.13: The style of the graphs should stay consistent throughout chapter. For example, make the main line thick red, the error lines light red, etc.
- P. 83, L. 1691-1692: Although this states that halons and CH_3Br are increasing, text on P. 88 states that MeBr is decreasing. The authoring team should check for consistency.
- P. 84-87: Box 2.2 is overly complicated, and the authoring team should consider simplifying it.
- P. 85, L. 1739-1745: This text should be moved to a figure caption, and this figure caption should be applied to the unnumbered figure on P. 86, and a reference to this figure should be inserted in the text.
- P. 87, L. 1760: Replace "as" with "and" instead.
- P. 88, Figure 2.13: Why are yellow and orange lines used rather than faint red?
- P. 90, Figure 2.14: Depending on audience, people may not think in "derivatives."
- P. 91, L. 1853-1855: This sentence should be edited.

- P. 97, L. 1970: The authoring team should consider deleting the clause "aided by U.S. decreases." This point has already been made in the text.
- P. 98, L. 1992-1995: This text states that the U.S. GWP-weighted emissions of ODSs have declined by 74% through 2004. However, section 2.1.4.2 (P. 47, L. 973-975) states that U.S. production and consumption data are not available in GWP-weighted form. Both sentences may strictly be true, but U.S. GWP-weighted emissions are only obtainable from detailed knowledge of the production.

CHAPTER 3: OZONE AND UV

Chapter 3 of the draft SAP represents a good effort at assembling the necessary material into a first draft. The questions are clearly posed, and the topics needed to address these questions are covered. Chapter 3 may be improved with a reorganization of its sections, as suggested in Chapter 2 (Overarching Comments) of this review, and as suggested below.

Major Comments

As suggested in the "Major Comments" for SAP Chapter 1, some of the background information provided in the introduction of SAP Chapter 3 should be moved to SAP Chapter 1 instead. In addition, there could be some useful restructuring within SAP Chapter 3. While recognizing that this suggestion is a preference, it is motivated by the belief that the intended audience will then benefit more from the data. For example, the chemistry to understand processes in Section 3.2.1.2 (P. 121) is not defined until later in Section 3.2.3.2.1, and the text should not have to refer the reader ahead in the text to understand the discussion, which is now necessary in at least two places (e.g., L. 2390 and 2570). At a minimum, the background information should be presented at the beginning of the draft SAP Chapter 3, or in Chapter 1, perhaps defining concepts and incorporating parts of the Key Issues section, before presenting the key findings.

Chapter 3 of the draft SAP is the place to distinguish between the two types of stratospheric ozone loss, including the different photochemical mechanisms (catalytic cycles) associated with the loss processes, for example, causing (1) small ozone loss (in absolute terms relative to column ozone, but large fractional ozone loss) in the upper stratosphere largely at midlatitudes, and (2) the large polar ozone losses in the lowermost stratosphere, as well as the relative roles of transport and chemistry in causing ozone changes in the mid-latitude lower stratosphere. Some mention should be made of the different methods of observing/monitoring ozone loss at these two altitude regimes. Note that the agreement among measurements of ozone trends in the upper stratosphere is better than Figure 3.8 shows (some of the differences are due to differences between trends at fixed pressure and at fixed altitude). Instead of showing a cumulative trend per decade, the authoring team should show the trend as a variation over time (similar to Figure 3.7). While the time series of ozone loss at the poles is shown (Figures 3.5 and 3.7), a similar time series of 40 km ozone amounts is needed (e.g., see Steinbrecht et al.

2006). The upper stratosphere is where recovery is likely to be first detected, since ozone changes in the upper stratosphere are the most well understood.

The first half of the UV-B section seems unnecessarily pessimistic, in the sense that it basically concludes that not much is known about what is "really" going on with surface UV-B trends, but can only estimate what ought to be going on from surrogate satellite estimates. (This same point applies to the ozone section to a lesser degree, as far as 2-D models not matching observations. That should not be a surprise, so we will just have to see what develops with 3-D models.) The committee is concerned that such a negative tone will only amplify misinterpretation, such as the recent editorial in the *Washington Times* on the 20th anniversary of the Montreal Protocol concluding that the ozone depletion scenario predicted in the 1980s was all overblown, and that it has recovered (if indeed, much outside the tropics ever went away, according to the article) as a result of natural processes. In Chapter 3 of the draft SAP, it states that midlatitude ozone has been essentially flat (or perhaps increased nominally) starting about 1994, a few years *before* the maximum in EESC. Only later does it explain *why*. To the casual reader, one could make an incorrect assumption that ozone increased from natural processes, without reading many more pages to find out that the ozone loss processes are moderated by (but not wholly controlled by) dynamics. Similarly, one could argue by a casual (mis)reading that the lack of correlation between ground-level UV-B observations and varying column ozone amounts indicates that there was never any serious risk. Only many pages later is it stated that clouds have a larger impact on day-to-day variations in surface UV-B than fluctuations in the column ozone abundance. This separation of effect-and-cause by several pages could lead to serious misinterpretation of the conclusions, especially when taken out of context. One possible way to fix this is to place section 3.2.3 ("Processes That Effect Ozone") much earlier in the text. The authoring team should state explicitly the associated observational limitations and uncertainties in the context of Figure 3-16. Perhaps it should also be clearly stated what the original projections of increased UV-B in the absence of the Montreal Protocol were, and how the surface observations (and surrogate satellite observations) are at least reasonably consistent with the fact that this drastic ozone loss did *not* occur. That, in and of itself, could help the authoring team relate its findings to human activities, such as the international regulation of halogenated compounds under the Montreal Protocol, as required in the SAP prospectus.

The treatment of UV measurements in the draft SAP has two main problems. First of all, surface UV measurements are shown, but they provide no useful information regarding ozone trends. Chapter 3 of the draft SAP should show how the 305 nm UV measurements compare or agree with the measurements of ozone. Secondly, not enough detail is given for the reader to understand that the global estimates of surface UV are derived primarily from satellite estimates of column ozone. The committee recommends the following specific changes:

- For the background section (P. 144-149, L. 2860-2970):
    - L. 2862: Add the table from the viewgraph titled "Additional Findings (1)" somewhere in/near this paragraph to indicate that clouds and pollution are as/more important than ozone in determining surface UV-B.

The authoring team could add two columns for the magnitude of typical impact on UV-B and maximal impact (i.e., clouds typically reduce UV-B by -30%, but can vary it from +12% to -100%; typical ranges of ozone allow a much narrower range of UV-B attenuation—easy to calculate using total column ozone $\pm 1\sigma \times$ radiation amplification factor (RAF) for typical, ozone $\pm 3\sigma \times$ RAF for maximal impact; similar estimates can be obtained from EPA for impact of typical and maximal ground-level haze/smog on surface UV-B). This would be a great table that would simplify the issue for many folks once and for all.

- L. 2866: Delete "above the surface". All ozone is above the surface.
- L. 2887-2888: What is the reference for the statement that ozone depletion is the dominant contributor to long-term and zonal-averaged UVB changes? This statement seems to contradict other statements in the document, e.g., L. 2895-2900.
- L. 2888: Delete "-term".
- L. 2899-2900: Make it clear that the aerosols and trace gases are the "pollutants" that are discussed later.
- L. 2901: What is the maximal range of expected UV-B (305 nm) changes caused by a "typical" ozone column variability? +x% to -y%; then, what expanded range of UV-B changes might have resulted from having not implemented the Montreal Protocol? (Look at the models from the 1970s.)
- L. 2907-2912: Topics in these two paragraphs are confused. The committee suggests the following revision: "…conditions as clouds pass over a site. Reflectivities for typical midlatitude cloud covered scenes are 30-50% and can reach 90% over high altitude tropical clouds." And, "Satellite observations of reflected UV indicate that aside from scenes with snow or ice cover or high tropical clouds, reflectivities near 90% rarely occur. Under…" Is "rarely" the right word, or should it be "never"?
- L. 2907: Add: "Clouds can therefore cause an increase in UV-B up to about 12% caused by cloud edge reflections, as well as decreases of -100%, when the cloud cover is exceedingly dark."
- L. 2913: Revise: "reaching a maximum of about 10%".
- L. 2916: Define "R".
- L. 2928: Revise: "Figure 3.14 shows that a decline in atmospheric pollutants can cause increases of up to 10% per decade in the surface UV-B."
- L. 2928: Replace "affect all UV" with "reduce UV at all".
- L. 2934: Revise: "However, global estimates…"
- L. 2936: For "UV irradiance estimates (UVest)", use "UVest" throughout the next few pages when referring to surface UV estimated from satellite observations to distinguish it from actual surface UV-B observations.

  - L. 2955: This is a great point, but it is buried in the text. Repeat this both earlier and later in the summary. How large would these changes have been? Would they have been large enough so we could have (in a world without the Montreal Protocol) more easily distinguished between ozone, cloud and pollution-induced changes in surface UV-B? (Yes.)
  - L. 2958: Not "alternatively", since this is exactly what was just explained on L. 2936.
  - L. 2966-2968: Omit the sentence "Therefore… of UV." The measurements are not "superior", but qualitatively different. (Actually, they are less realistic, since they cannot account for UV absorption near the ground.)
  - L. 2967-2968: If the UV is inferred from ozone directly, then it is a **prediction**, not a measurement, and it cannot be used to confirm UV changes due to ODSs.

- Begin the discussion of surface UV measurements (L. 2973) with a statement that long-term surface UV measurements must be carefully made to preclude variations due to clouds, aerosols, and air pollution, perhaps adding approximate percent variations in UV caused by each of these. The other point to make is that making accurate surface spectral UV measurements is extremely difficult since the surface irradiance spectrum increases by several orders of magnitude over the UVB-UVA-visible spectral region and very small errors in wavelength alignment can caused large errors in amplitude. No figures are required since these effects are extensively discussed in the background section, thus eliminating Figures 3.13 and 3.15 and the text associated with these figures. There is no point to showing such poor long-term records in the SAP. To correlate UV measurements with changes in ozone, clouds, aerosols, and pollution should be controlled as opposed to what was done for Figures 3.13 and 3.15.

- Highlight Figure 3.14 as an example of what can be accomplished by good cloud-free surface measurements. Figure 3.14 should be changed so it is clear that the UV305 is, in fact, decreasing in response to the long-term trend in ozone. To convey this point, the authoring team should consider the following options:

  a) Top panel, departures 305 nm; middle panel, ozone column departures; bottom panel, the logarithm of the uv305/uv324 ratio. This line should then be departing negative. This may require normalizing the 305/324 differences since they are of a bit of a different magnitude. Or,

  b) Keep the panels as they are, but use the 324 nm trend to retrend the 305 nm and then add a panel at the bottom showing the difference of the 305 nm measured and retrended 305 nm. This will again go negative as would be expected if ozone is increasing.

c) In either case, add the average ozone 1990-2000 to the ozone column panel. It is understood that the value will be 375 Dobson Units (DU).

The authoring team should also make a point about the anti-correlation between irradiance at 305 nm and ozone column departures. This clearly shows the sensitivity of 305 nm radiation to ozone.

To provide a better explanation of the radiation amplification factor and its use, the authoring team should provide answers to the following questions: Where did it come from (references?), how is it used, what is the value of α, and how is it that the product of the three factors (α, Ω, and sec(θ)) has units of percentage per decade?

- For Section 3.3.2.2, the authoring team should include an introductory paragraph to explain that we must rely on satellite estimates of surface UV to obtain surface UV throughout the Earth in the absence of wide coverage of excellent cloud/aerosol/pollution-free surface UV measurements. If the purpose is to see how UV is changing, then the observation of all effects (ozone, clouds, aerosols, pollution) represent real-world conditions; if the purpose is to detect and attribute UV changes to changes in ozone, then all other factors should stay constant. This explanation should be followed by a discussion of exactly how the surface UV is derived. For example, what are the weighting factors for ozone column, surface reflectance, backscattered UV, cloud reflectivity, aerosol, etc.? What is/are the primary controlling variable(s)? Perhaps the authoring team should show a plot of good agreement between clear-sky observations of surface UV-B (using Brewers, Czech data?) and that estimated from Total Ozone Mapping Spectrometer (TOMS) data, clearly highlighting the importance of measurements beyond ozone in forming the UV estimates. What satellite estimates of UV can and cannot be reproduced? The draft SAP leaves the impression that Figure 3.16 may be just ozone columns over the United States in a different guise. The draft SAP suggests the controlling property for the UV irradiance estimates is total ozone, with differences between UV305 nm and erythemal irradiance only dependent on how each of these scales with total ozone. Noting that the top and two bottom panels of Figure 3.16 are symmetrical, which would be expected, how important are the additional inputs from the satellite data?

  - L. 3052: “For UV-B at 305 nm (see…”
  - L. 3053: “The column ozone amount…”

Section 3.3.4 (“Human Exposure to UV”) is weak in addressing the title of the section. The discussion is only about where UV exposure is maximized, not what happens to people who live in these areas. It seems skin cancer rates in regions of high UV exposure (e.g., Australia, Himalayas, etc.) could be mentioned in comparison to less harsh locations. In the legend for Figure 3.17, the yellow shaded area should be defined as the effect of the Antarctic ozone hole. The authoring team should reconsider whether

Figure 3.18 is helpful here, as it does not match with the discussion of human exposure. If this figure is judged to be helpful, then another quantity more directly related to human health should be shown, e.g. maximum UV index . Also, in terms of UV, there is more difference between the Northern and Southern Hemispheres than just clouds. There are differences in Earth solar distance, aerosol content, air pollution, tropospheric ozone, and the effects of Antarctic ozone loss (McKenzie 2003). Comparisons of nearly antipodean summertime measurements indicate that Southern Hemispheric surface UV is on the order of 50% higher than Northern Hemispheric surface UV (Seckmeyer 1995). Comparisons between Barrow and Palmer show similar differences. (See Figure 3.17 of SAP 2.4.)

For the issue of ClOOCl photolysis (P. 138-140 & 143, L. 2733-2774), the draft SAP does not provide much detail, and there is no synthesis of the importance of this issue. The authors should consider highlighting the significant uncertainties surrounding the ClOOCl photolysis rates and the amount of atmospheric Br before presenting what the current measurements/analyses indicate. Once the reader understands the scientific uncertainty, then the presentation of conflicting pieces of information is more reasonable. The authoring team should consider moving text from the paragraph on L. 2770-2774 to the beginning of this section as an introduction, something like the following three revised paragraphs excerpted from the text beginning at L. 2734:

> The basics of polar chemistry leading …for the rapid catalytic ozone loss in the spring (see WMO 2007). Thus, stratospheric chlorine levels provide the fundamental driver for polar ozone loss, since chlorine is involved in both of the catalytic cycles responsible for polar ozone loss. Beyond this basic understanding, however, the ~~C~~calculated chemical loss rates of polar ozone are still quantitatively uncertain. Questions remain to be resolved on the photolysis rate of the chlorine dimer and the balance between ClO and ClOOCl in the Antarctic stratosphere and the atmospheric abundance of bromine. Higher levels of … uncertain.
>
> ~~It has been suggested~~ From *in situ* aircraft measurements, ~~(~~Stimpfle et al.~~,~~ (2004) suggested that the chlorine monoxide (ClO) dimer cycle may be a more efficient process for polar ozone loss than previously thought (Frieler et al., 2006), and ~~.~~ ~~G~~good overall consistency between *in situ* observations of ClO and the ClOOCl and model calculations can be achieved if it is assumed that ClOOCl photolyzes faster than assumed in WMO (2003). However, recent laboratory measurements of the absorption cross-section of the chlorine monoxide dimer (ClOOCl) do not support this. They indicate that ClOOCl may ~~actually~~ photolyze slower than previously understood (Pope et al., 2007). The slower photolysis rate would result in ~~a~~ smaller depletion rates and hence poor representations of the severity of polar ozone losses in ~~three dimensional (3-D)~~ models. Despite this uncertainty, 3-D models currently reproduce the basic features of the Antarctic ozone hole and Arctic ozone losses using previous laboratory recommendations for photochemical parameters (*e.g.*, WMO, 2003; WMO, 2007). Clearly more work will be required to understand this discrepancy.

> Recent measurements show that bromine exists in the stratosphere at higher concentrations than is found in most 3-D models (WMO, 2007 and references therein). ~~Hence, bromine plays a more important role in polar ozone depletion than previously thought.~~ Profiles of bromine monoxide (BrO) measured in the Arctic vortex suggest that inorganic bromine levels may be 3 to 8 parts per trillion (ppt) by volume larger than the amount of bromine carried to the stratosphere by methyl bromide (CH_3Br) and halons alone (Canty et al., 2005 [not in the reference list]; Frieler et al., 2006). Although still uncertain the additional 3-8 ppt of bromine is probably derived from very short lived (VSL) species containing bromine that enter the stratosphere at the tropical tropopause (WMO, 2007). Considering that the BrO + ClO cycle is now estimated to contribute up to half of total chemical loss of polar ozone, using the more efficient ozone loss by the ClO dimer cycle,~~T~~ this observation indicates the BrO + ClO catalytic cycle is likely to be a more efficient ozone loss process than considered in WMO (2003). Hence, bromine may play a more important role in polar ozone depletion than previously thought.~~The BrO + ClO cycle is now estimated to contribute up to half of total chemical loss of polar ozone, even considering the more efficient ozone loss by the ClO dimer cycle. The additional 3-8 ppt of bromine is a probably derived from very short lived (VSL) species containing bromine that enter the stratosphere at the tropical tropopause (WMO, 2007).~~

In reference to the statement above about the BrO+ClO cycle estimated to contribute up to half of total chemical loss of polar ozone, the authoring team should clarify whether this applies to the Arctic only (i.e., not to both polar regions).

In addition to these revisions, the authoring team should further explore the ramifications of the results of Pope et al. (2007). In the committee's view, it is too simplistic to state that the findings of Pope et al. lead to poor model representations of ozone depletion. Pope et al. are certainly not claiming to have the full story with their results, making it quite clear that if their results are correct, then some other mechanism for closing the cycle and returning the dimer to ClO must be involved. A key and highly policy-relevant point should be highlighted here (and revisited in SAP Chapter 6). Our understanding of polar ozone chemistry does not just hang by the ClO dimer photolysis thread alone. There is a huge body of observational evidence accumulated over the past three decades that provides considerable confidence in our understanding of the chemistry responsible for polar ozone depletion.

Specific Comments

- In Chapter 3 and the other sections of the SAP, the authoring team should define acronyms where they first appear in each chapter. For example, insert "(ODS)" after the phrase "ozone-depleting substances" on P. 108, L. 2149.
- The notion of catalysis is crucial to understanding how a small amount of halogens released from CFCs and related compounds can cause a disproportionately large ozone loss, especially over the Antarctic. The authoring committee should include a Krebs-cycle-like bubble diagram to show how halogens potentiate significant ozone loss

by the continual cycling of the halogen on each round of the catalytic cycle (about 100,000 ozone molecules destroyed per chlorine atom released into the atmosphere for polar ozone loss, before some other chemical reaction takes the chlorine out of the cycle). For examples of lucid descriptions of ozone loss directed at an appropriate level audience, the committee suggests looking at first-year general chemistry textbooks in common use.

- P. 109, L. 2167: This question should be stated as, "What is the current state of ozone in the stratosphere in the Earth's midlatitudes and over the polar regions?" Since the mechanisms are distinct, it is important to show that the processes responsible for them and for future recovery are indeed distinct, although connected through the concept of EESC.
- P. 109, L. 2182: Why was there a minimum in Northern midlatitude ozone in 1993, and why has it begun to increase (somewhat) since then?
- P. 110, L. 2194-2202: The committee disagrees with using the word "stabilized", especially in the context of the last sentence of this paragraph ("2006 ozone hole was one of the most severe on record"). This last sentence looks like it was tacked on to a pre-2006 paragraph. Perhaps this could be rewritten to state something to the effect that since *all* the ozone within a specific region of altitude and geography is completely destroyed in most years (moderated by dynamical factors), the ozone loss is effectively maxed out or saturated. The point here is to be clear that significant additional losses cannot occur because ozone cannot decrease below zero. In this same paragraph, the issue of "higher minimums" and "lower maximums" comes up, confusing the reader. The last half of this paragraph has this problem in each sentence: "higher minimum column amounts were observed to be below average;" "higher levels of dynamical forcing" (increased forcing, or forcing at higher altitudes?—remember, this is being read by non-experts); and "more normal conditions, the 2006 ozone hole was one of the most severe on record." "More normal" is not the same as most severe. When in doubt, break up the sentences into shorter, more declarative sentences, even if it looks somewhat repetitive (at least the reader will not come to the opposite of the intended conclusion).
- P. 110, L. 2198: "Observed to be below average" is confusing. The ozone hole is below average because it is not as severe as normal? Some might say this is above average in terms of depletion. The authoring team should consider the following revision: "In some recent winter years, the observations indicated a smaller ~~O~~ozone hole area, lower ozone mass deficit, and higher minimum column ~~amounts~~ozone ~~were observed to be below average in some recent winter years~~."
- P. 110, L. 2202: To make a clear distinction from Arctic ozone loss and to avoid any potential misunderstanding, the term "Antarctic" should be added each time the ozone hole is mentioned.
- P. 111, L. 2212: Was there really a discussion of acute health effects of UVB exposure? There is an indication that ozone can be extremely low at the southern tip of South America, but the health affects of this were not discussed.
- P. 111, L. 2215: Suggest rewriting the phrase "Trends in UV exposure changes".
- P. 111, L. 2220: Because the 7% minimum in 1993 is relative to 1979 as well, the sentence should be revised to convey this point more clearly.
- P. 112, L. 2237-2249: This text should be revised and expanded. The language needs to be more precise, and a clearer separation needs to be made between the catalytic

cycles operating at low and middle latitudes in the mid-stratosphere and those responsible for the severe ozone loss in the polar lower stratosphere. The discussion in Q.9 of the "Twenty Questions" section (Fahey 2007) of the 2007 WMO report can serve as a template, as can the more detailed description in Chapter 3 of the book *The Chemistry and Physics of Stratospheric Ozone* by Andrew Dessler (2000). The latter could also be used as a reference in the text. The following, although probably too long to be inserted in its entirety, provides some guidance for the information that should be incorporated in the revised text:

> In the middle latitudes (between the tropics and the polar regions), ozone (O_3) is catalytically destroyed by halogens and other chemicals to form molecular oxygen (O_2):
>
> $$\begin{aligned}&O_3 + X \rightarrow O_2 + XO\\&\underline{XO + O \rightarrow X + O_2}\\&\text{Net:}\quad O_3 + O \rightarrow 2\,O_2\end{aligned}$$
>
> In this cycle, X represents the catalysts chlorine atoms (Cl), bromine atoms (Br), and the oxides of nitrogen (nitric oxide, NO) and hydrogen (hydroxyl, OH). Note that all of these catalysts are highly reactive free radicals, meaning they have an unpaired electron, which tends to attach to other molecules in order to form a chemical bond. Since these reactions have an initial energy barrier to reaction, warmer temperatures will speed up this catalytic cycle, and cooler temperatures (as predicted to occur by recent climate models) will slow down this ozone loss cycle.
>
> Increased use of the halogenated source gases (CFCs, halons, etc.) over the past few decades has resulted in the release of the halogens chlorine and bromine into the stratosphere, where the more intense ultraviolet sunlight photochemically liberates Cl and Br atoms, which are then available to catalytically destroy stratospheric ozone. While NO and OH radicals are present in the unperturbed atmosphere (although their abundance has also been altered by human activity), catalytic reactions involving Cl and Br released from halogenated source gases represent the dominant ozone loss processes in the mid-stratosphere outside the Earth's polar regions. These same gases, Cl and Br, are also responsible for polar ozone loss but at a lower altitude and through different chemistry.
>
> Since oxygen atoms (O) are also involved in this midlatitude ozone loss cycle, this cycle operates most rapidly in the mid-stratosphere, since concentrations of oxygen atoms increase with increasing altitude, as more intense ultraviolet light photolyzes ozone and molecular oxygen (O_2) to release additional oxygen atoms:
>
> $$O_3 + h\nu \rightarrow O + O_2 \qquad\qquad O_2 + h\nu \rightarrow O + O$$
>
> in which hν represents the absorption of solar ultraviolet light to photolyze (photochemically break a chemical bond) ozone and molecular oxygen.

Maximum halogen catalyzed ozone loss at midlatitudes occurs around an altitude of about 40 km, where oxygen atoms are more abundant.

While *fractional* ozone loss peaks near 40 km for a stratosphere unperturbed by cold temperatures (about 8-10% of the naturally-occurring ozone at that altitude), the *absolute* amount of total column ozone loss at 40 km is much smaller in the unperturbed Antarctic, since ozone concentrations fall off rapidly above 20 km, and are much smaller than in the mid latitudes at 40 km. In mid-latitudes, the bulk of the ozone layer is centered around 25 km.

In the Earth's polar regions, a different set of halogen catalyzed reactions is responsible for the dramatic seasonal ozone losses observed over Antarctica during August through October each year (with more than 50% of the total column ozone depleted) and to a much smaller extent over the Arctic during February through March each year. The difference in hemispheres has to do with the presence of polar stratospheric clouds which provide particle surfaces for the heterogeneous chemistry converting inactive chlorine in reservoir molecules to active chlorine to participate in reactions with ozone. The extent of these stratospheric clouds is much greater in the Antarctic due to colder stratospheric temperatures than in the Arctic. Thus, molecules to participate in the following two catalytic cycles involving chlorine and bromine atoms are much more abundant in the Antarctic. These reactions account for all but a few percent of the polar ozone loss, which occurs in the lowermost stratosphere (12-24 km altitude):

$O_3 + Cl \rightarrow O_2 + ClO$	$O_3 + Br \rightarrow O_2 + BrO$
$O_3 + Cl \rightarrow O_2 + ClO$	$O_3 + Cl \rightarrow O_2 + ClO$
$ClO + ClO + M \rightarrow ClOOCl + M$	$BrO + ClO + M \rightarrow BrOOCl + M$
$ClOOCl + h\nu \rightarrow ClOO + Cl$	$BrOOCl + h\nu \rightarrow Br + ClOO$
$\underline{ClOO + M \rightarrow Cl + O_2}$	$\underline{ClOO + M \rightarrow Cl + O_2}$
Net: $2\,O_3 + h\nu \rightarrow 3\,O_2$	Net: $2\,O_3 + h\nu \rightarrow 3\,O_2$

in which M represents any air molecule, typically nitrogen (N_2) or oxygen (O_2), which carries away the excess energy of the reaction.

One could also break out the above text into two boxes, one labeled "Midlatitude Ozone Depletion" and another "Polar Ozone Depletion."

- P. 113, L. 2256: Provide a year for the McPeters reference.
- P. 113, L. 2258-2260: Revise this sentence as follows: "~~Hadley cell~~The Brewer-Dobson circulation carries air upwards across the troposphere and into the stratosphere in the tropics near 16 km." Also, add a reference at end of sentence.
- P. 114, L. 2264: Revise the text to read, "downward by the ~~Hadley cell mean stratospheric~~Brewer-Dobson circulation." Also, add reference(s).
- P. 114, L. 2268: Revise the text to read, "the top panel. The density of ozone is highest at altitudes between 12 and 24 km."
- P. 114, L. 2294: Depletions are neither strong nor weak, but can be quantified. Consider phrasing this as "substantial depletions…" instead.
- P. 115, L. 2300-2302: Too much of the SAP is written in a "clear only if known" manner. One example is the use of the phrase "ozone reached a minimum in 1993",

which is stated over and over without the explanation that this minimum corresponds to the time of maximum stratospheric aerosol loading that followed the eruption of Mt. Pinatubo and resulted in enhanced ozone loss due to anthropogenic halogens. At this point in the text, the description of this process should include mention of the important role of NOx in this chain of events.

- P. 115, L. 2305: What is meant with this phrase "important for biological processes"? Which biological processes? Beneficial or detrimental? For example, the authoring team should note in this particular chapter the beneficial effects of UV exposure in terms of vitamin-D production and the health consequences of insufficient UV exposure.
- P. 116, L. 2307-2308: Rewrite these lines as follows: "and aerosols (Mie scattering), but is not absorbed or otherwise attenuated by ozone. UV-B, however, is absorbed by ozone, as well as scattered by molecules and aerosols. Ozone absorption in the UV-B increases…"
- P. 117, L. 2332-2333: These lines could be misread to mean ozone stabilized before EESC maximized, as if ozone loss were independent of EESC. The authoring team should rewrite this text.
- P. 117, L. 2336: Figure 3.2 used Dobson Units (DU), but the text uses percent. This is a wording issue endemic to the field, switching back between percent and DU ozone loss, between total cumulative ozone loss and decadal ozone loss, etc. Whichever term the authoring team chooses, it is generally better to use the same term consistently throughout.
- P. 117, L. 2336: Following from the previous comment, the authoring team should consider showing both DU and % scales in Figure 3.2 using, say, 292 DU=100%. While the % deviations are preferable in Figure 3.2 (bottom), the DU scale makes more sense for Figure 3.2 (top panel) where ozone values are shown.
- P. 117, L. 2346: Figure 3.2 should have a title, such as "Global Ozone Trends, 60°S-60°N". Does the zero-line for ozone change (bottom panel) start at 1965, or should it start at 1980 as elsewhere in the draft SAP? (But then, it has to be acknowledged that the fit to EESC in Figure 3.2 has reached about 40% of full depletion by 1980.) Or the 1964-1980 average, as in the text? If the latter, the bottom panel looks like zero ozone loss starts in 1964 (when EESC was very small). The authoring team should use the same baselines in all chapters.
- P. 117, L. 2348-2354: The caption should define DU and QBO. The authoring team should include at least one reference.
- P. 118, L. 2350: Insert the word "cycle" after "solar".
- P. 119, Figure 3.3: Make the font size of the text labels larger so they will be legible.
- P. 119, L. 2363-2366: The top panels should be labeled "Northern Hemisphere" and "Southern Hemisphere"; and the bottom panel should be labeled "USA". In the figure legend, SBUV, NIWA, and GOME should be defined and explained in terms of what sort of data they are providing.
- P. 119, L. 2369: The authoring team should consider updating the figure from the 2006 assessment (WMO 2007) rather than the previous assessment (WMO 2003).
- P. 120, L. 2372: Delete the second use of "normalization".

- P. 120, L. 2374: The box on estimating ozone trends is not very clear because the references to the figures cannot be understood, e.g. "3-.2.1.1.-1". Also, the box should have a number, similar to other boxes in the report.
- P. 120, L. 2378: Insert "(Figure 3.3, bottom)" after "processes".
- P. 121, L. 2382: "ozone changes are similar to ozone over…"
- P. 121, L. 2398: "Vortex" is not defined as a meteorological "containment vessel" that keeps low ozone inside and high ozone outside (both with and without the ozone hole!) but as a sharp north-south gradient in temperatures near the edges of the polar region.
- P. 122, L. 2405: "Antarctic is more stable because the Antarctic polar vortex is more stable."
- P. 122, L. 2407: The correct section number is 3.2.1.2.2.
- P. 122, L. 2410: Again, ozone is increasing but the EESC is not going up (much), or at least it appears that ozone increases before EESC reaches a maximum. This could be misunderstood since it is not explained until pages later; also restated in L. 2429.
- P. 122, Figure 3.4: The years in the figure do not match the figure caption.
- P. 123, Figure 3.5: This figure should be extended to include the size of the 2007 ozone hole.
- P. 123, L. 2436: WMO should not be used as a primary reference (use scientific papers), but could be used as a secondary reference.
- P. 124, L. 2440-2446: The committee suggests the following revision: "Although NH polar column ozone averages ~~is~~ are a general indicator of Arctic ozone depletion and trends (WMO, 2003), the March average in 2004/2005, reflecting~~s~~ the strong influence of dynamics (*e.g.*, vortex fragments moved outside the 63°-90° region during March, see Figure 3.4), ~~and~~ is ~~consequently~~ high relative to ~~those of~~ other recent cold winters even though ~~the magnitude of~~ chemical ozone loss in the lower stratospheric vortex in mid winter 2004/2005 was as high as or higher than ~~in those years~~ozone loss in other recent cold winters. During…"
- P. 124, L. 2447: Consider if the plot were shifted by two weeks (15 February to 15 March). Would this show more loss relative to dynamical effects?
- P. 124, L. 2450-2460: This paragraph should be improved, in particular with regard to the interpretation of the total ozone values between 2000 and 2005. Chapter 6 of the WMO report (2007) makes a clear statement in this regard, which is missing here: "Inside the Antarctic vortex, the interannual variations in ozone depletion observed from 2001 to 2005 have not been caused by changes in EESC. At current EESC concentrations, nearly total loss of ozone occurs in the lowermost stratosphere inside the ozone hole in September and October and EESC concentrations often exceed those necessary to cause total loss. The Antarctic ozone hole, therefore, has low sensitivity to moderate decreases in EESC and the unusually small ozone holes in some recent years (e.g. 2002 and 2004) are strongly attributable to a dynamically driven warmer Antarctic stratosphere."
- P. 124, L. 2451: Add "Antarctic" before "ozone hole".
- P. 124, L. 2452: Replace "line" with "panel".
- P. 124, L. 2452-2453: What is the reason for the increase?

- P. 124, L. 2459: Is something missing from Figure 3.6? The "last two decades" are not covered by the bottom row.
- P. 125, L. 2471: Perhaps add a phrase for clarity, e.g., "… minimum, which also shows the dynamically perturbed years 2002-2004 but without as much amplitude."
- P. 126, L. 2489: The authoring team should consider updating the figure from the 2006 assessment (WMO 2007) rather than the previous assessment (WMO 2003).
- P. 126, Figure 3.7: Add the low data point for 2006. The casual reader will wonder why 2002-2004 is so high and might assume that for these years the data does not match the quadratic fit. The point here is that these were warm years, and are indeed explainable as being comparatively warm years. Perhaps a good way to modify the plot is to also run the same fit using whatever temperature was used on the original fit ±2σ in the temperature, and then have a grayed-out band, which should provide a better range over which all of the data points, including 2002-2004, should fit. The plot should also have a label for "warm winters" and "cold winters". Can this figure be extended to include the size of the 2007 ozone hole? Also, this figure replicates half of Figure 3.5—could they be consolidated? In the caption for Figure 3.7, NCEP/NCAR data is credited for the plot. Why is this data set required to generate the data plotted in the figure, which is only total column ozone data? Instead of stating "data courtesy of Greg Bodeker (NIWA)", the authoring team should cite the source publication for the data (Bodeker 2005).
- P. 126, L. 2485: Box 8.1, Chapter 8—to which document does this refer?
- P. 126-128, Section 3.2.2.1: The authoring team should mention somewhere that the stratospheric ozone column in the tropics has declined substantially (WMO 2007; see also Randel and Wu 2007). This is important in order to place the earlier statement about the lack of a trend in total column tropical ozone (L. 2342-2344) in context.
- P. 126, L. 2493: Surely, the clearest evidence of ozone destruction due to chlorine compounds is the ozone hole, not the upper stratosphere?
- P. 126, L. 2493: Add "global" before "ozone".
- P. 127, L. 2497: It is not net ozone decrease (not a column integrated amount), but only the "cumulative ozone decrease in the upper stratosphere from 1979 to 1995 was ~10-15%..." Why aren't the upper stratospheric ozone data shown? It seems they would be quite appropriate.
- P. 127, L. 2503-2507 and P. 128, L. 2518-2525: Based on the text in these two places, it would be expected to see two figures for trends in profile ozone: (1) 1979-1995, and (2) 1996-2004. Figure 3.8, which is taken from the WMO report (2007) and combines the earlier decreasing trend with the later increasing trend, provides no support for the discussion here which would be of more interest than combining all the data onto one plot. Another possibility is to change the figure to total change since 1979 instead of change/decade. Please reconcile the discussions with the figure. There is a section in the WMO report (2007) about the EESC fit, why the trend should be expressed in percent per decade, and how the percent per decade trend is related to the cumulative decline. Because a similar discussion appears to be lacking in SAP Chapter 3, the trend numbers are confusing. The authoring team should provide clarification on the EESC fit and cumulative ozone loss here.
- P. 127, L. 2507: Where are the 10% declines? They are not obvious in Figure 3.8.

- P. 127, L. 2508: Figure 3.8 needs degree symbols in latitude titles of both panels; also, a time series of upper stratospheric midlatitude ozone as a plot might be helpful. Also, the trend is shown in the figure as percent per decade, while the text describes cumulative ozone loss over the entire 1979-1995 period… confusing. Pick one or the other. I prefer cumulative ozone loss relative to some well-defined baseline value.
- P. 128, L. 2525: "on the column because most of the ozone resides in the lowermost stratosphere."
- P. 128, Section 3.2.2.2: In this section on polar ozone, the authoring team may want to include mention of the work of Yang et al. (2005).
- P. 128, L. 2529 and P. 129, L. 2537: The correct spelling is "Hofmann", not "Hoffman". (The authoring team should check entire document to be sure this name is spelled correctly.)
- P. 129, Figure 3.9: Fix the order in the legend so it is chronological. The caption does not match the legend in the figure. Can this figure be redone to include 2005 and 2006?
- P. 129, L.2541: Replace"1996" with "1966" instead.
- P. 130, L. 2553: Annual final warming is not a major warming? If so, how can this be the first? "Major warming" should be defined.
- P. 130, L. 2558 and following: This discussion is out of place. In fact, it should be included in the discussion of processes that affect ozone. Furthermore, the wording in the first sentence contradicts the previous paragraph.
- P. 130, L. 2562: Change to: "Compared to the Antarctic, the Arctic is generally warmer with fewer polar stratospheric clouds (PSCs) (Fromm et al., 2004, ~~see~~ Figure 3.12~~in Section 3.2.3.2.1~~)."
- P. 130, L. 2564: Revise this sentence as follows: "…regions are generally not concentric ~~around the pole~~with the Arctic polar vortex, but are frequently centered roughly in the region between Greenland and Norway."
- P. 130, L. 2564: "Thus o~~O~~zone levels in the Arctic low stratosphere exhibit a large amount of variability…"
- P. 131, Figure 3.10: This figure, which compares relative rates of Arctic and Antarctic ozone depletion, disguises the fact that, for cold Arctic winters, chemical loss of column ozone in the Arctic is comparable to chemical loss of column ozone in the Antarctic. That is because Figure 3.10 disguises the situation that the Antarctic starts out with so much less ozone than the Arctic. The committee suggests that two panels should be given: one that shows fractional loss, the other that shows ozone density (concentration) loss.
- P. 132, L. 2594: What is the source or reference for this statement?
- P. 132, L. 2596: Add "as a percentage ozone loss at each altitude layer."
- P. 132, L. 2601-2602: Revise as follows: "… analyses (Rex et al., 2004; WMO 2003; WMO 2007)), and are significantly less than in the Antarctic."
- P. 132, L. 2604+: Should 3.2.3 be placed earlier, along with general discussion of ozone chemistry and other processes?
- P. 132-133, L. 2606-2616: This section needs a better discussion of how temperatures are influenced by dynamics. Perhaps even changing the title to "transport and dynamically driven temperature changes". As it stands now, for the non expert, the

paragraph mentions only transport and temperature, skipping completely any mention of dynamics but which is in the title of the section. What may be intended is as follows: "Stratospheric ozone levels are strongly influenced by both transport and the dynamics which control temperatures ~~of~~ in the stratosphere…." Then, expand on how dynamics controls the local temperature below before seguewaying (sic) into catalytic ozone loss.

- P. 133, L. 2627: Polar ozone loss is not caused solely by PSCs, and could be misread this way. Try: "The chemical ozone loss processes precipitated by the presence of halogens are initiated by the formation of PSCs in the extremely cold polar lower stratosphere (refs.). PSCs provide a surface upon which heterogeneous (not gas-phase, but at the surface between a solid/liquid and a gas) reactions take place that convert comparatively unreactive chlorine reservoirs into ones that are exceedingly reactive in sunlight." PSCs do not generate radicals; rather, sunlight does.
- P. 133, L. 2637: V_{PSC} should use subscripts, here and throughout.
- P. 134, L. 2642: "The cooling of the lower stratosphere is much larger…"
- P. 134, L. 2647: Figure 3.11 is first cited earlier (L. 2570)—should it be placed earlier? Label the figure with Arctic ozone loss. Are some years missing?
- P. 135, L. 2664: "waves propagating into the stratosphere as measured by ???" Provide an indication as to how planetary wave activity is measured.
- P. 136, L. 2679-2680: "In the upper stratosphere, dynamically or radiatively forced temperature changes have a large effect on ozone loss rates because ???." The authoring team should not leave the reader believing that it is the same story in the upper stratosphere and then immediately shift to great detail in the lower stratosphere. The SAP text is unbalanced towards ascribing all the ozone "recovery" below 30 km to transport processes. Chapter 6 of the WMO report (Bodeker and Waugh 2007) contains a much more balanced description of the slowing of the ozone decline in the lower stratosphere at mid-latitudes since 1996. It notes that between 18 and 25 km altitude, the slowing of the decline can be attributed to atmospheric chlorine and bromine (EESC) changes and it also points out likely sources of uncertainty in the transport contributions derived from models.
- P. 136, L. 2696: Is the implicit assumption here and throughout that dynamics=temperature?? Some discussion of this might help.
- P. 136, L. 2697: It seems odd in this discussion of global mid latitude ozone to lead off the difficulties with the polar vortex. This does not seem to be the primary difficulty.
- P. 137, L. 2705: "Near zero ozone air" might not be understood; "the mixing of air from the tropical upper troposphere (with very low ozone amounts) into…"
- P. 137, L. 2708: How much is a "large fraction"? This could be taken by some to mean that there is no such thing as photochemical ozone loss, and that it is all the result of "natural" processes. Certain phrases might easily be taken out of context and misunderstood. Also, the phrase "observed inferred" is confusing.
- P. 137, L. 2718: Are chemical transport models (CTMs) reliable for attributing changes in ozone from transport? The discrepancies between CTMs and observations in terms of interannual variations are so large that it is difficult to place much weight on CTM results to attribute long-term changes.

- P. 138, L. 2723: If this is true, why do the ozone loss vs. EESC plots fit so well? This is the same issue of misunderstanding as in L. 2708.
- P. 138, L. 2726-2727: Redundant.
- P. 138, L. 2733-2738: As already suggested, the basis of polar chemistry should be explained much earlier in the report. Again, a lot of repetition in this section can be avoided through improvements of the structure.
- P. 138, L. 2735: Heterogeneous processes do not directly convert chlorine reservoirs to ClO. The text can be made more precise here by explicitly showing just a few reactions:

> "Heterogeneous reactions on PSCs convert the comparatively unreactive chlorine reservoirs hydrochloric acid (HCl) and chlorine nitrate ($ClONO_2$) first to chlorine gas (Cl_2) in the long, dark polar night:
>
> $ClONO_2 + H_2O$ ---on PSCs→ $HOCl + HNO_3$
>
> $HOCl + HCl$ ---on PSCs→ $Cl_2 + H_2O$
>
> As soon as the Sun first appears over the horizon in the Antarctic spring in August each year, the chlorine gas photolyzes (breaks apart into chlorine atoms in the presence of sunlight) and reacts with ozone to make chlorine monoxide (ClO):
>
> Cl_2 –sunlight→ 2 Cl
>
> $2\times(Cl + O_3 \rightarrow ClO + O_2)$
>
> The sum of all the reactions up to this point is often called "chlorine activation," since the chlorine compounds are converted from comparatively unreactive forms to much more photochemically reactive forms. At high concentrations of ClO, it reacts both with itself (to form the so-called ClO dimer, ClOOCl, a reaction that actually proceeds *faster* at lower temperatures) and with the analogous bromine monoxide, BrO. Catalytic cycles formed from the reaction of ClO with itself and with BrO are responsible for almost all of the rapid ozone loss in the Antarctic spring (refs)."

- P. 139, L. 2749-2754: The one paper published to date that has examined the implications of the Pope et al. cross section on our understanding of polar ozone loss is von Hobe et al. (2007).
- P. 140, L. 2770: This statement is too negative. Be quantitative here. We can certainly get to better than 20%, perhaps as good as 10%, depending who you read and believe. That is relatively good agreement. Although there is good quantitative agreement, it is not perfect.
- P. 140, L. 2774: While "the exact sources of the extra bromine" may be "somewhat uncertain", there is ample evidence that CHBr3 and CH2Br2 can supply the full burden of the higher levels of bromine.
- P. 140, L. 2776: Chlorine activation is never defined (see L. 2735 text above); denitrification is never defined. For a "critically important" process (and it is), it should have been defined by now in the text.
- P. 140, L. 2777-2783: What is the point of this paragraph? The topic sentence indicating the importance of PSCs is disconnected with the rest of the paragraph which is just a recap of what satellites are available to measure PSCs. Is this helpful, are these the only measurements? Why the special mention? All of this would be in Fromm et al. 2003

anyway. The authoring team should eliminate the recap of satellites and either discuss the importance of denitrification, which should probably be defined, or merge the topic sentence here with the next paragraph and eliminate the paragraph.

- P. 140, L. 2789: "…Figure 3.12 (Fromm et al., 2003)."
- P. 141, Figure 3.12: Define "entire winter season"
- P. 141, L. 2800: The variabilities are not different between the Arctic and Antarctic (i.e., the variabilities of PSC frequency ±10% in both hemispheres is about the same), just the means are different (in the Southern Hemisphere, the variability is about a mean of 70% versus a mean of 10% for the Northern Hemisphere).
- P. 141, L. 2802: The committee suggests the following revision: "…However, even in the coldest Arctic winters PSCs are only about 25% of PSC frequency ~~not nearly as frequent as~~ in the Antarctic."
- P. 142, L. 2806: The committee suggests the following revision: "…As in the polar regions, halogen increases … decades in the midlatitudes…"
- P. 142, L. 2819-2820: Suggest, … "The sulfur ~~formed~~ enhanced stratospheric sulfate aerosols ~~with~~ providing significantly more surfaces that could support heterogeneous chemical reactions, thus converting …"
- P. 142, L. 2821: "Catalytically active forms".
- P. 143, L. 2829-2837: Similar discussion appears in the work of Salawitch et al. (2005).
- P. 143: The discussion of ozone profile changes needs more references and needs to be expanded. The text suggests that there is a major puzzle in our understanding. In fact, CCMs seem to get upper stratospheric ozone losses that are quite consistent with SBUV(/2)—at fixed pressure levels.
- P. 143, L. 2829-2837: What is the point of this paragraph and is it related to the previous discussion concerning the missing ozone loss in southern hemisphere, which is what the reader may be expecting? It starts with a specific discussion of very short-lived species (VSLS) bromine, but then becomes quite general about mid latitude ozone loss and high aerosol and then indicates that aerosol isn't important for bromine and odd-hydrogen ozone loss cycles. If it is only about bromine then it should start a little more generally.
- P. 143, L. 2839: "Profile shape" is redundant; delete "shape".
- P. 143, L. 2840:." … cause of declining ozone, …"
- P. 143, L. 2844-2846: What is the difference between "non-interactive and interactive" models? What is the significance of "in altitude coordinates"?
- P. 144, top: References are needed. Is this a discussion of CCMs or CTMs?
- P. 151, L. 2999-3000: It is stated that the impact of aerosol and pollution on UV may be different at 305 nm and 324 nm, yet the assumption made is that they are equal in making the residual calculation on P. 153.
- P. 152, L. 3005: Cite original sources as well as WMO 2007.
- P. 156, L. 3062: Are the calculated UV changes positive or negative? "as about +20%".
- P. 157, L. 3069: This needs to be worded much more carefully: the expansion was in the 1980s, not the 1990s, and was it actually the *vortex* that was expanding rather than the ozone minimum? See Bodeker 2002.

- P. 157, L. 3070: Has the Antarctic polar vortex really persisted into late December?
- P. 157, L. 3079-3086: In the context of summertime UV changes, it is pertinent that summertime high-latitude ozone trends are attributable to springtime losses over the entire extratropics (and hence to ODSs). See Figure 3-18 of the WMO report (2007).
- P. 157, L. 3084-3086: These low-ozone episodes are not ODS-related.
- P. 157, L. 3086: List these references.
- P. 158, Figure 3.17: The figure caption and the ordinate axis label should agree. Perhaps no data are available for a northern hemisphere site closer to the latitude of Palmer, which would make a nicer comparison. There is a 50% difference in summer UV between Barrow and Palmer that is more than just a reflection of the latitude difference. Also, include a reference for the data or for the plot.
- P. 159, L. 3102: At beginning of this section, elaborate (briefly) on the lost next-to-last “key finding” about “acute health effects of UV-B exposure.” They are nowhere discussed or even listed, and they should at least be enumerated with adequate reference to other sources. The authoring team could look at (Lloyd 1993); this short review article also has many of the standard references (which should be updated). The authoring team may also refer to the WMO’s 20 questions document (Fahey 2007), specifically the question on skin cancer. Keep in mind that the differences between the hemispheres are not solely clouds, but also the larger column ozone in the NH, aerosol differences (more in NH), etc.
- P. 159, Figure 3.18: Change the color bar to indicate two sets of units: kW/m^2sec and the maximum UV index at each location in each month. The latter is more familiar to the average reader. .
- P. 161, L. 3142: If this really is the bottom line, the authoring team should contrast what is currently observed as far as UV-B changes (not much) versus what would have happened, had the Montreal Protocol not been implemented.
- P. 161, L. 3148: Why? If EESC is still rising in 1993, it must be temperatures, dynamics, etc.
- P. 161, L. 3153: Replace “7%” to “8%” to be consistent with P. 160.
- P. 165, L. 3316: Complete this reference.

CHAPTER 4: OZONE EFFECTS ON CLIMATE

The purpose of Chapter 4 of the draft SAP is to examine the relationships between human impacts on ozone and the changes occurring in the climate system. Overall, the chapter does a good job of capturing the main issues, but the presentation of these issues should be improved. In particular, as discussed in the overarching comments of this peer review report, there needs to be a better framing of the two-way interaction between ozone and climate. Several major and specific comments are made below.

Major Comments

Unfortunately, this chapter does not discuss the relationships between ozone and climate in a historical context. For example, in discussing the attribution of stratospheric temperature trends to ozone, carbon dioxide, and water vapor, the references are only the recent studies, ignoring previous work that laid the groundwork on this issue.

In the introduction on P. 174, the authoring team should include a discussion of the importance of ozone to the climate system both through its absorption of solar radiation and as a greenhouse gas through its absorption of infrared radiation. Ozone is not only a greenhouse gas, but is the third most important greenhouse gas in the natural climate system after water vapor and carbon dioxide. An additional sentence after the first two sentences could segue into ozone's overall importance on the climate system. The authoring team should consider including a diagram or schematic such as the figure presented by David Fahey (NOAA) at the committee's meeting (Fahey et al. 2007).

The issue of climate change and ozone represents an area where SAP 2.4 is severely lacking. There are much more comprehensive accounts of these processes in Chapter 1 of the IPCC report (2005) and Chapter 5 of the WMO report (2007). The authoring team should provide an equal balance between how climate change might affect ozone (which is treated well in SAP 2.4) and how ozone changes may affect climate (which is lacking in SAP 2.4). In the WMO report, one of the key findings linking climate change to ozone is Figure 4-3 (WMO 2007); not only is this figure lacking from the SAP, but so is any discussion of this key new finding.

On P. 175, there is discussion of ODPs and GWPs with no introduction as to what those concepts are or where they come from. While a largely inadequate explanation of GWPs (e.g., why integrate 100 years?) can be found in the glossary, there is no explanation of ODPs. Perhaps this can be handled by referring to SAP Chapter 2, but it would also be good to have ODPs included in the Glossary.

Somewhere, perhaps as early as Section 4.2, there should be a breakdown of the radiative forcing contributions from different ODSs.

Because the reader may not know what a "chemistry climate model" is, Section 4.3 should include a sentence (or perhaps more) about these numerical models representing the coupled physical and chemical processes of the climate system. Figure 4.4 should also reflect this change.

The authoring team should include a discussion of the general sign of the effects of changes in ozone with altitude and latitude on climate. The discussion on the top of P. 186 only mentions that decreases in stratospheric ozone cool the stratosphere, but there is no discussion of the complex interactions between the solar absorption and infrared effects of ozone on affecting climate (one does not need to go back to the Lacis et al. study but should at least provide the more up-to-date representation of those findings such has been done in papers by Jim Hansen and by Forster and Shine).

Figure 4.9 discusses the effects of one IPCC scenario only (A2). It is not balanced to just cite one scenario that has particularly large relative methane to nitrous oxide increase and promote this scenario as the basis for future effects of non-ODSs effects on ozone. Also, A2 is high for ozone effects, but is not the highest IPCC climate effects scenario (which is A1fi).

Section 4.6.1 on the relevance to the United States (P. 198, L. 4063-4069) seems like it was written as an afterthought, and it does not say much. This is an opportunity to discuss the policy implications related to the interactions between ozone and climate (e.g., how future ozone could be affected by climate policy in the United States and throughout the world). As an example, future growth in methane and nitrous oxide could have important ramifications to the ozone layer. Other comments related to elimination of the banked ODSs and to HCFCs relative to replacements for these compounds (some like HCFC-123 are more energy efficient and have less effects on climate than alternatives) are saved to the review of Chapter 6, but are also relevant here.

In some parts of this chapter, there is a tendency for the authors to particularly focus on their own research rather than providing a balanced overview of the literature. Some specific suggestions for further discussion are provided below.

Specific Comments

- P. 170, L. 3488-3489: Chemical processes do not transport ozone.
- P. 171, L. 3505-3506: Start with a bullet that discusses the importance of ozone as an absorber of solar radiation and as a greenhouse gas, and that ozone is one of the most important gases (after water vapor and carbon dioxide) in affecting the natural climate system.
- P. 171, L. 3508: This question about models does not fit among the rest of the scientific questions here.
- P. 171, L. 3513: Chapter 5 says we do not need to worry about volcanic eruptions because their effects are short-term, so why is this question being asked (especially within the context of climate change)?
- P. 171, L. 3519: Along with providing the 20% number, the bullet should also give the actual radiative forcing values to help place the 20% into context.
- P. 171, L. 3520-3523: While the ODS contribution to radiative forcing will decline, it is being largely offset by that from the ODS substitutes. The two need to be considered together, to avoid confusion.
- P. 171, L. 3524: There are other ways of showing ozone and climate effects; consider replacing “given” with “represented, for example,”.
- P. 172, L. 3532-3534: Add in discussion of how the sign (positive, negative) of changes in stratospheric ozone with altitude can affect the sign of forcing on climate.
- P. 172, L. 3538: Just saying “chemistry climate models (CCMs)” is likely jargon to a general audience. The authoring team should consider including a statement like “coupled models of the Earth’s atmospheric chemistry and climate processes (called chemistry climate models, CCMs)”.
- P. 172, L. 3541-3542: Since there were few measurements of stratospheric temperatures before the 1960s, what is the justification for saying “temperatures have decreased beginning in the 1960s”? How do we know it didn’t start before that?
- P. 172, L. 3548-3549: Revise this statement as follows: “The ~~principal~~main greenhouse gases changing as a direct result of human activities are carbon dioxide…” The principal greenhouse gases in the Earth’s atmosphere are water vapor, carbon dioxide, and ozone.

- P. 173, L. 3550-3555: First, the decreasing water vapor is only in the lower stratosphere—be clear. Secondly, the increasing methane will explain the increasing water vapor, but not the decrease. Explain the decrease (see Bill Randel's paper). Thirdly, discuss the trends in the upper stratosphere that basically are continuing to increase as a result of the increasing methane.
- P. 173, L. 3566-3569: Although the impact of ozone depletion on tropospheric climate is outside the scope of this report, it is listed here as a key finding. (In fact, it does not seem to have support anywhere in the chapter.)
- P. 174, L. 3574-3579: The authoring team should also discuss the effects of volcanic eruptions on ozone through chemical processes.
- P. 179, L. 3675: Revise: "the RF on climate".
- P. 184, L. 3774: The last sentence in this paragraph starts a new idea and should be the beginning of the next paragraph.
- P. 185, Figure 4.5: This figure is missing a discussion of the basis for the zero reference level. The observations in this figure seem not to exhibit enough interannual variability. (Also, to say the observed temperature evolution is captured "reasonably" by the models is a bit misleading, since the temperatures are normalized.) A much better figure (with more recent model results, and from a large group of models, not just one [moreover, one from a member of the author team]) would be Figure 4(a) of Eyring et al. (2006)—which was Figure 5-11(a) of the WMO report (2007) and uses CCMs.
- P. 187, Figure 4.6: These models are not CCMs according to the SAP's definition; rather, they are atmosphere-ocean GCMs (with no ozone chemistry)—or, "AOGCMs".
- P. 189, L. 3869: The wording is a bit tendentious. Do we really think that Arctic temperatures will decrease in the future? The available CCM results suggest if anything the opposite.
- P. 189, L. 3876: Change "will" to "would"; this is a conditional statement.
- P. 189, L. 3878: What is meant by "further" decreases in temperature?
- P. 190: Section 4.3.3 on water vapor is entirely missing a discussion of the trends in water vapor above 22 km, e.g., from HALOE measurements, that show water vapor has been increasing at higher altitudes for several decades, including beyond 2000, largely as a result of methane oxidation.
- P. 192: The reference to Wennberg does not provide proper credit to what has been known about HOx catalytic effects on ozone for a long time.
- P. 195-197: Discussion in Section 4.5 on volcanic eruptions does not adequately discuss the effects on ozone resulting from chemical interactions on the sulfate aerosols, especially that since this effect is related to the ODS induced chlorine and bromine in the stratosphere, this effect may disappear by mid-century.
- P. 197, L. 4032-4033: Do we really need a model to show that volcanic eruptions, which by their nature are intermittent, will not threaten the long-term recovery of ozone? This seems rather obvious. The only way it would be threatened would be if we had persistent volcanic eruptions, but we cannot know that.
- P. 198: The authoring team should include some discussion of how reliable the CCMs are—at the very least, a reference to Eyring et al. (2006).

- The authoring team should say somewhere that for a fully self-consistent treatment of chemistry-climate interactions, CCMs will need to be coupled to ocean models, and to tropospheric chemistry.

CHAPTER 5: THE FUTURE AND RECOVERY

The purpose of Chapter 5 of the draft SAP is to synthesize information on the future behavior of ozone, UV radiation at Earth's surface, and ODSs. To improve this synthesis, Chapter 5 would benefit from a more critical discussion of scenarios and models, as well as a presentation of the updated model results that were prepared by the scientific community for the WMO's *Scientific Assessment of Ozone Depletion: 2006* (WMO 2007). The model results were not completed in time to be included in the WMO report, but have since been published in a paper by Eyring et al. (2007).

Major Comments

Overall, SAP Chapter 5 would benefit from two additional sections: one that discusses the various phases of recovery, and another that discusses the rather considerable, emerging literature on empirical studies that have addressed whether ozone has begun to recover. The more empirical studies of ozone recovery that are incorporated into the SAP's message (done in a manner that is coordinated with SAP Chapter 3), the stronger Chapter 5 will be.

Among the questions to be addressed by the SAP is, "What are the various possible emissions scenarios that can be considered for any further policy actions on emissions of ozone-depleting gases?" The only reference to this is a bullet in Chapter 6 stating that the hypothetical cessation of all future emissions of ODSs starting in 2007 may be considered by Parties to the Montreal Protocol. This bullet is carried forward to the Executive Summary. Such a hypothesis seems highly unrealistic. Table TS-5 on P. 34 of the IPCC/TEAP report (2005) provides a less aggressive mitigation scenario for drawing down the banks that might be more possible. The committee strongly supports the idea of including an emission scenario between no action and an unrealistic extreme case, since the emission scenario depends on future emissions from new production as well for the HC.

The authoring team should include some critique of the ODS scenarios, which could be incorrect. At some point, the modeling community is going to have to move to emission-based (not concentration-based) modeling of ODSs, to allow for a full feedback of changes in ODS lifetimes on ODS abundances. The current lack of emission-based modeling represents a major uncertainty in the current projections, which should be flagged.

Chapter 5 of the draft SAP relies too much on 2D models, and the authoring team should include an expanded discussion of how 2D models should be interpreted: the committee disagrees with the apparent equal weight placed on 2D models and CCMs. The CCMs are now at least as reliable as the 2D models (it could be argued that they are actually more reliable) for midlatitude changes, to say nothing of polar changes (and it is known that the polar changes influence the midlatitude changes, especially in the

Southern Hemisphere). Two-dimensional, non-interactive models should not be considered projections, as they are physically unrealizable. Furthermore, the circulation changes predicted by 2D interactive models could be of dubious value. Two-dimensional models are probably best used to look at the differences between scenarios.

Chapter 5 is uncritical of CCM results that are deemed unreliable by WMO. For example, in many places, recovery dates or depletions are quoted from models explicitly designated “unreliable” by WMO (2007) (those with the dashed lines in the plots), yet they seem to be given full weight in this report. The two models in question (AMTRAC and MRI) had Cl_y that was clearly physically wrong (and stated as such in the WMO report). So, why even consider them?

In general, Chapter 5 of the draft SAP lacks an adequate “assessment” concerning the models: results are presented without enough context or discussion of what the authors believe. Where there are discrepancies between different models, the authoring team should provide at least some value-added expert opinion. (Much of this can actually rely on the WMO report.)

The authoring team should include some update from the WMO report, since 18 months have passed from when it was coming into final form. At the very least, the CCM discussion should have been based on Eyring et al. (2007), not the WMO report, given that the WMO report had none of the “reliable” models going past 2050, whereas Eyring et al. (2007) show two of them out to 2100 (one of which is a U.S. model). The results published in the Eyring et al. paper represent a straightforward update of the results in the WMO report, but some of the simulations were not completed in time to be included in the WMO report. Nevertheless, the Eyring et al. paper represents the current consensus of the CCM community, and so the relevant figures should be taken from it. In fact, the authoring team would be justified in including additional model simulations beyond those shown in the Eyring et al. paper, especially from the U.S. models, for the most complete picture.

The authoring team should include more discussion of the differences between midlatitude and tropical changes, since much of the discussion in Chapter 5 is for the 60°S-60°N aggregate. This point is especially pertinent in light of the recent paper by Randel and Wu (2007)—the results of which were highlighted in the WMO report—noting significant decreases in the stratospheric column in the tropics (in agreement with the CCMs). In particular, the models are pointing to a strong dynamical (transport-induced) super-recovery in the Northern Hemisphere midlatitudes, which could see total ozone returning to 1980 levels before 2020 (evident from the update to SAP figure 5.2 in the Eyring et al. paper). This should be discussed in SAP Chapter 3, and the implications of this certainly have relevance for the United States and pertinence for modeling and attribution of climate change (i.e., observed ozone changes cannot be imposed and reversed in line with EESC as a scenario to force climate models in the 21st century).

For P. 238, 239, and 240, Figure 5.7 and Table 5.2, two additional scenarios should be added here and in supporting information: (1) a scenario that shows zero emissions of EPA estimates of CFCs and halons that could be economically recovered within the United States (and globally, if available from IPCC or EPA estimates) and destroyed, and (2) a scenario that reduces HCFC production consistent with U.S. proposals to the Montreal Protocol (as on the ozone secretariat web site) for acceleration of the HCFC phaseout in developed and developing countries; information in the U.S.

submission to the ozone secretariat can be generalized to develop a representative scenario. Both of these scenarios are required for decision support of policy issues being considered by the United States.

The reference list in Chapter 5 includes almost no journal papers. The authoring team should consider expanding their literature review for this chapter, including original studies rather than just citing the assessment, where appropriate.

Specific Comments

- P. 204, L. 4262: Why only consider column ozone? This seems overly narrow.
- P. 204, L. 4266-4270: The wording needs to be tightened up. What non-halogen emissions are discussed here? And how is this different from climate change (it says "also" on L. 4268)? Does the text intend to mean **changes** in solar cycle and volcanic eruptions, or just those phenomena themselves?
- P. 204, L. 4275: All ODSs will not be gone by mid-century. ODSs in 1980 were well above zero. More generally, the authoring team should discuss the selection of 1980 as the baseline year.
- P. 205, L. 4284: "Will show" is strange wording.
- P. 205, L. 4284-4292: It is really the combination between climate change and halogens that matters, so this should be discussed more generally. These lines read as if ozone is determined by halogens until 2050, and afterwards ozone is determined by climate change. That is too simplistic.

  Why is there no discussion of the dynamical super-recovery in the midlatitudes (especially in the Northern Hemisphere), and the associated permanent sub-recovery in the tropics? This would seem to be a big story!
- P. 205, L. 4286-4287: In fact, the CCMs suggest that circulation changes will be of comparable importance in many regions (tropics, Northern Hemisphere midlatitudes, possibly even the Arctic). In fact, the tropical changes in the stratospheric column observed today may well be mainly due to climate change (though this has yet to be demonstrated).
- P. 205, L. 4289-4291: Not just then, but already.
- P. 206, L. 4317: "Up to 15 years earlier" is based on one model; it does not seem to be a representative assessment of the overall CCM results, which (the committee would say) rather seem to suggest recovery will largely follow EESC over 60°S-60°N (see the update to SAP figure 5.2 in the Eyring et al. paper).
- P. 206, L. 4320: Five percent only applies if you look at subregions; over 60°S-60°N, it is probably more like 2%. And this is not super-recovery, as suggested by the text, as the CCMs were also 2% higher than 1980 values in the 1960s.
- P. 207, L. 4329: The statement is correct, but it needs context: for many of the models, Antarctic ozone follows EESC, and so the point is that EESC recovers to 1980 values before 2060-2070. This may be partly realistic (decreasing age of air), and partly unrealistic (young age of air bias).
- P. 207, L. 4338: The text implies that one model predicts worsening Arctic ozone depletion. However, this model cannot be taken seriously, as its EESC is physically bizarre. (The WMO report did not take it seriously, so their expert judgment can be

relied on in this respect.) In an assessment such as this SAP, good information should be sifted from the bad. To be fairly considered, this information should be presented with an assessment of its reliability.

- P. 207, L. 4343: Isn't it virtually tautological to say that halogens will have a negligible effect on ozone when there are no halogens left? Is the text actually saying anything more than this?
- P. 210-211, L. 4412-4414: That the indirect radiative forcing due to ozone loss falls to zero before the direct RF does is merely a point of definition; that is how the indirect RF is defined (rather arbitrarily). In fact, the ozone loss attributable to ODSs began before 1980 (see Figure 3.2, P.118).
- P. 212-213, Section 5.1: This is almost a verbatim repetition of the key findings. A proper introduction should be included.
- P. 213, L. 4454-4456: What is the reference for this statement? Halogen effects are changing slowly, and there is increasing evidence for circulation changes (and, in the upper stratosphere, effects of stratospheric cooling).
- P. 213, L. 4461: Replace "should be" with "are".
- P. 214, L. 4473-74: The uncertainty in these scenarios should be discussed.
- P. 215, L. 4508-4509: Check the consistency of these numbers with Figure 4.9.
- P. 215, L. 4509: Change "0.5%" to "1.0%".
- P. 215, L. 4510: Change "1.0%" to "0.5%".
- P. 217, Section 5.2.2: If 2D models are to be shown, then the authoring team should include a more critical discussion of why, and what aspects can be trusted of the projections. For example, if non-interactive results are included, they cannot be considered part of the projections, but only to look at the difference between interactive and non-interactive as representing the effects of climate change. So, it is for attribution rather than projection purposes. (This is how the non-interactive 2D models were used in WMO [2007], apart from Figure 1(c) of the Executive Summary, which seems to have included the non-interactive 2D models from Figure 6-9 of WMO [2007] [displayed in SAP 2.4 as Figure 5.1] in constructing its range of model projections.)
- And along the same lines, are the interactive 2D models trustworthy? To include CO_2 cooling is fine, but circulation changes from a 2D model surely cannot be trusted as they are not physically based (e.g., they can be induced by Rayleigh drag or strong diffusion). The committee is not sure what is meant by "calculating the residual circulation from heating rates"—this makes no sense. What is the role of circulation changes in these models?
- In the WMO report (2007), the 2D models were included to be able to consider different scenarios, which the CCMs were not able to consider because of computational limitations. Arguably, the differences between scenarios from the 2D models would be of value. But for the baseline projections, the 2D models need to be discounted. Do we really believe a projection of 60°S-60°N ozone being 5% above 1980 values in 2100 (Figure 5.1), when it is not seen in the CCMs (Eyring et al. 2007)?
- P. 217, L. 4542: CCMs are now better able (than 2D models) to represent global ozone, too.
- P. 219, Figure 5.1: Why is the super-recovery different in the two hemispheres?

- P. 220, L. 4584: "Most" CCMs (at least for the future projections) do *not* include a QBO.
- P. 220, L. 4585: How can the QBO make it difficult to "define recovery"?
- P. 220, L. 4586: This one model projection is actually two, and they are both not to be trusted for this purpose. The WMO authors explicitly said so (WMO 2007). So, the results should not be highlighted.
- P. 220, L. 4588: Need to update!
- P. 220, L. 4590: Climate change is evident much earlier, in certain regions.
- P. 220, L. 4591: This value is from AMTRAC, which is also 2-5% above the 1980 values in the 1960s. So this wording is very misleading.
- P. 221, Figure 5.2 (and associated discussion): There is no discussion at all of possible circulation changes, yet they are quite evident from this figure. The authoring team should at least replace this figure with the one in Eyring et al. (2007). The authoring team should include some critique of AMTRAC and MRI, if the results are to be shown. And note that there is a lot of uncertainty associated with the 1980 baseline. (This does not refer to the fact that ozone loss begins earlier, but rather that the use of the 1980 baseline requires statistical acrobatics which lead to unreliable estimates of the ozone depletion.)
- P. 221, L. 4621: Yes, but for many of the models the EESC recovery date is earlier, too. This might be partly true (faster Brewer-Dobson [BD] circulation, so decreasing age of air), but also partly wrong (young age of air bias in models). There needs to be an *assessment* here.
- P. 222, L. 4625: Why even mention this one model (thus giving credence to it), if the authoring team does not believe its projection?
- P. 224, Section 5.2.3: This section needs more quantification (and some references).
- P. 224, L. 4658: It seems tautological to say that climate change will dominate over the effects of ODSs when there are no ODSs left.
- P. 225, L. 4665: "Preoccupied" is an inappropriate word to use here.
- P. 225, L. 4666-4667: This long-term, world-wide decrease contradicts what is said in the middle paragraph on P.145.
- P. 225, L. 4682: Sure, but these AMTRAC results are not trusted. The midlatitude Cly is unphysically high, for numerical reasons that are understood.
- P. 239, L. 4921: This point needs some explanation.
- P. 241, L. 4956: To avoid confusion, the reference should be to section 5.5.1.2, not just section 5.5.1, to make clear that the differences that are being talked about are between Newman et al. (2006) and the WMO report, in each region (polar and midlatitudes), and *not* between polar and midlatitudes.
- P. 242, Figure 5.8: Why duplicate a figure that is already in Chapter 2? (In Chapter 2, the figure is even referred to!)
- P. 243, L. 4995-4996: For a discussion of life cycle analysis, a reference to the IPCC/TEAP report (2005) would be good.
- P. 244, L. 5003: Change "5.7%" to "5.6%".
- P. 245 & 246, L. 5032-5041 & 5047-5056: Information in these sections is repetitive.

- P. 245, Section 5.5.3.2: This paragraph is repeated verbatim immediately below.
- P. 246, L. 5060: Is ozone depletion really so nonlinear (apart from the ozone hole)? Changes of less than 10% should be reasonably linear.
- So far, the report is assuming, in coming up with its estimates, that all observed ozone changes are attributable to ODSs. But, there are many studies indicating that a substantial fraction of the observed changes (~30% in NH midlatitudes) are due to changes in transport, rather than ODSs. So, the U.S. ODS-related contribution to ozone changes would be reduced accordingly.

CHAPTER 6: IMPLICATIONS FOR THE UNITED STATES

This chapter starts with a discussion of why the changes in ozone over the United States are not directly connected with the U.S. emissions of ODSs. Such a discussion is likely necessary to the purpose of this report, but the chapter largely does not capture the content it should have to be useful to policymakers. Several major comments are made below, as well as a number of more specific comments.

Major Comments

"Implications for the United States" can be viewed in three ways: in terms of accountability, impacts, and management. To address **accountability**, Chapter 6 of the draft SAP should assess the past contribution of the U.S. to ozone depletion (EESC) and climate change (radiative forcing) due to consumption of ODSs, and how U.S. actions have contributed to reducing past, current, and future contributions to those issues. To address **impacts**, the authoring team should assess potential impacts in the United States due to global consumption of ODSs. This can only be addressed in terms of the atmospheric sciences since the report was not designed to be an effects report. And finally, the authoring team should discuss the remaining **management** issues related to ODSs and what information can be provided from a scientific perspective to inform these management decisions. Each of these three implications should be addressed in Chapter 6 with summary points being carried forward to the Executive Summary.

A main problem with Chapter 6 is the lack of discussion of ODS management issues that are currently being discussed heavily by policymakers, both in the United States and internationally through the Montreal Protocol. Although the committee does not suggest that the specific bills before Congress or specific measures before the Montreal Protocol should be included, the authoring team should include a generic discussion of these issues. Several examples of the key issues to examine are provided below:

- Representative Henry Waxman of California has sponsored a bill (H.R. 3448) that would accelerate the phase-out of HCFCs, but this bill includes an exemption to allow the continued use of HCFC-123 in specific applications. The United States and the other parties to the Montreal Protocol are implementing an accelerated phase-out of HCFCs. The SAP should discuss the science potentially affecting policy issues associated with HCFCs.

- Phasing out ODSs has potential implications to climate change due both to energy efficiency issues and the potential direct climate impacts of replacement compounds. The authoring team should include a discussion about the tradeoffs between ozone and climate policy as ODSs are eliminated. (There is much material on this in the IPCC/TEAP report [2005], which could be drawn upon.)
- While there is a very general discussion about the importance of banked quantities of ODSs, there is no discussion about the size of the banks, especially the banks that could be economically recovered and destroyed, in the United States or elsewhere in the world, nor any discussion about possible policy considerations to reduce or eliminate the banks so that effects on ozone could be reduced.
- There are some additional issues associated with the Montreal Protocol that should be discussed. For example, there are ongoing considerations for the effects of emissions of short-lived ODSs such as CF3I, n-propyl bromide, and other compounds (WMO 2007). There is a need for three-dimensional modeling studies and an assessment of such short-lived compounds to meet the needs of policymakers. Another example is the ongoing considerations about methyl bromide.

There is also no discussion of the potential role of indirect GWPs (a direct connection between stratospheric ozone and climate) in policymaking. Chapter 1 of the IPCC/TEAP report included a detailed discussion of indirect GWPs, including their uncertainties and potential pitfalls, and this material could be drawn upon.

Chapter 4 discusses the effects of carbon dioxide and human-impacted greenhouse gases on ozone. The authoring team should consider including a discussion of the role that the United States plays in the production and emission of such gases, and the resulting implications on ozone policy. The SAP would also benefit by including a discussion about the interrelationships between ozone policy and climate policy, both for the United States and internationally. More specifically, the SAP does not address the fact that limiting future emissions of methane is a “win-win” situation, in that methane affects ozone depletion and climate change. A concerted effort to reduce natural gas leaks and/or trap and burn (for fuel) methane emitted by landfills would help the ozone layer (less methane means less water, which means less HOx and more mid-latitude ozone and less PSCs and more polar ozone). There is ample scientific literature that points to the beneficial effects to the ozone layer from less methane, and other scientific literature that points to beneficial effects to global climate of less methane. A similar argument could be made regarding the win-win situation if N_2O emissions could be reduced (by, for example, more use of treated livestock manure as a beneficial product, rather than waste). Charts such as Figure 1.1 that simply indicate “the intricate coupling of the issues” are not too useful for policymakers; however, a figure showing the ODP and GWP of methane and N_2O, and demonstrating the positive effect for both ozone and climate of reductions in methane and N_2O, would certainly be useful to policymakers. The authoring team should also consider highlighting these options in the SAP Executive Summary.

The discussion of surface UV measurements has problems as in Chapter 3 of the draft SAP. It is not clear how section 6.5 fits into "Implications for the U.S." Perhaps the title of the Chapter 6 could be changed.

For the section describing "a world avoided", the committee commends the authoring team's attempt to make the point that we would be facing an even more delayed recovery if the participating nations had not taken action when they did.

Specific Comments

- P. 250, L. 5171: Emissions of what?
- P. 251, L. 5197-5207: Suggest the following rewrite, or something similar:

> The UV ~~changes~~ over the U.S. ~~have~~ has been measured from ground stations. Unfortunately, as discussed earlier, surface UV measurements are strongly affected by clouds, aerosol, and air pollution, thus they are not a good reference for estimating changes in UV from long term changes in ozone. Such changes in UV, however, can be estimated from the satellite record of column ozone and backscattered UV as described in Ch. 3. ~~and calculated from satellite observations of column ozone. Trends derived from observations from the surface have large uncertainties because of high variability and influence of clouds and aerosols.~~
>
> • ~~Calculations~~ Estimates of UV based on satellite observations of column ozone and reflectivity of the surface suggest that the averaged erythemal irradiance (which is a weighted combination of UVA and UVB based on skin sensitivity) over the United States increased roughly by ~~about~~ 7% when the ozone minimum was reached in 1993 and is now about 4% higher than in 1979.
>
> ~~Direct surface-based observations do not show significant trends over the U.S. in UV levels over the past three decades because effects of clouds and aerosol have likely masked the increase in UV due to ozone depletion over this region.~~

For "ground stations," the authoring team should consider provide more detail, such as the number and location of these stations.

- P. 251, L. 5210: Why is it difficult to accurately quantify the U.S. fraction? Is this statement in conflict with the statement on the previous page?: "The contribution of emissions from the U.S. to the global burden of ozone-depleting substances can also be quantified" (L. 5176-5177).
- P. 252, L. 5214-5217: After updating the percentages as discussed in this review, also discuss the implications of these percentages. For example, since it accounts for xx% of the global emissions currently, it is clear that the U.S. needs to carefully consider its role in additional policy considerations. It would be nice to see a table that discusses the effects from individual gases to get to the percentages provided, either here or in an earlier chapter.
- P. 252, L. 5223: Should the CUEs be specified?
- P. 252, L. 5224-5227: Similarly, discuss policy implications of the banked amounts, but also try to be more quantitative in the discussion. If possible, provide information on the individual contributions to the banked amount for the U.S. and the rest of the world.

- P. 253, L. 5240-5243: Why are two different units used, one absolute (W/m^2) the other % weighted by100 year GWP? Do the %GWP apply only to U.S. or global?
- P. 253, L. 5245: Reduced the climate forcing by how much? Recall the claim earlier that this can be quantified.
- P. 255, L. 5285-5286: This sentence needs rewording so that its meaning is clarified.

REFERENCES

Bodeker, G.E., H. Shiona, and H. Eskes. 2005. Indicators of Antarctic ozone depletion. *Atmospheric Chemistry and Physics* 5:2603-2615.

Bodeker, G.E., H. Struthers, and B.J. Connor. 2002. Dynamical containment of Antarctic ozone depletion. *Geophysical Research Letters* 29(7).

Bodeker, G.E., and D.W. Waugh. 2007. The Ozone Layer in the 21st Century. Chapter 6 In *Scientific Assessment of Ozone Depletion: 2006*. Global Ozone Research and Monitoring Project - Report No. 50. Geneva: World Meteorological Organization. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/index.shtml.

CCSP (U.S. Climate Change Science Program). 2007. *CCSP Synthesis and Assessment Product 2.4: Prospectus for Trends in Emissions of Ozone-Depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure*. Washington, DC: U.S. Climate Change Science Program. Available online: http://www.climatescience.gov/Library/sap/sap2-4/sap2-4prospectus-final.pdf.

Dessler, A.E. 2000. *The Chemistry and Physics of Stratospheric Ozone*. International Geophysics Series: Academic Press.

Eyring, V., N. Butchart, D.W. Waugh, H. Akiyoshi, J. Austin, S. Bekki, G.E. Bodeker, B.A. Boville, C. Brühl, M.P. Chipperfield, E. Cordero, M. Dameris, M. Deushi, V.E. Fioletov, S.M. Frith, R.R. Garcia, A. Gettelman, M.A. Giorgetta, V. Grewe, L. Jourdain, D.E. Kinnison, E. Mancini, E. Manzini, M. Marchand, D.R. Marsh, T. Nagashima, P.A. Newman, J.E. Nielsen, S. Pawson, G. Pitari, D.A. Plummer, E. Rozanov, M. Schraner, T.G. Shepherd, K. Shibata, R.S. Stolarski, H. Struthers, W. Tian, and M. Yoshiki. 2006. Assessment of temperature, trace species and ozone in chemistry-climate model simulations of the recent past. *Journal of Geophysical Research* 111:D22308.

Eyring, V., D.W. Waugh, G. E. Bodeker, E. Cordero, H. Akiyoshi, J. Austin, S.R. Beagley, B.A. Boville, P. Braesicke, C. Brühl, N. Butchart, M.P. Chipperfield, M. Dameris, R. Deckert, M. Deushi, S.M. Frith, R.R. Garcia, A. Gettelman, M.A. Giorgetta, D.E. Kinnison, E. Mancini, E. Manzini, D.R. Marsh, S. Matthes, T. Nagashima, P.A. Newman, J.E. Nielsen, S. Pawson, G. Pitari, D.A. Plummer, E. Rozanov, M. Schraner, J.F. Scinocca, K. Semeniuk, T.G. Shepherd, K. Shibata, B. Steil, R.S. Stolarski, W. Tian, and M. Yoshiki. 2007. Multimodel projections of stratospheric ozone in the 21st century. *Journal of Geophysical Research* 112:D16303.

Fahey, D.W. 2007. *Twenty Questions and Answers about the Ozone Layer: 2006 Update*. Geneva: World Meteorological Organization. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/Twenty_Questions.pdf.

Fahey, D.W., A.R. Douglass, V. Ramaswamy, and A.-M. Schmoltner. 2007. Chapter 4: How do climate change and stratospheric ozone loss interact? PowerPoint Presentation. September 11, 2007, Boulder, CO. National Academies Committee Meeting on Trends in Emissions of Ozone-depleting Substances, Ozone Layer Recovery, and Implications for Ultraviolet Radiation Exposure.

IPCC/TEAP (Intergovernmental Panel on Climate Change/Montreal Protocol's Technology and Economic Assessment Panel). 2005. *Safeguarding the Ozone Layer and the Global Climate System: Issues Related to Hydrofluorocarbons and Perfluorocarbons*. IPCC/TEAP Special Report. Geneva: IPCC. Available online: http://arch.rivm.nl/env/int/ipcc/pages_media/SROC-final/SpecialReportSROC.html.

Lloyd, S. 1993. Health and climate change: Stratospheric ozone depletion. *Lancet* 342 (8880):1156-1158.

McKenzie, R.L., D. Smale, G.E. Bodeker, and H. Claude. 2003. Ozone profile differences between Europe and New Zealand: Effects on surface UV irradiance and its estimation from satellite sensors. *Journal of Geophysical Research* 108 (D6):4179.

Newman, P.A., E.R. Nash, S.R. Kawa, S.A. Montzka, and S.M. Schauffler. 2006. When will the Antarctic ozone hole recover? *Geophysical Research Letters* (33):L12814.

Pope, F.D., J.C. Hansen, K.D. Bayes, R.R. Friedl, and S.P. Sander. 2007. Ultraviolet absorption spectrum of chlorine peroxide, ClOOCl. *Journal of Physical Chemistry A* 111 (20):4322-4332.

Randel, W.J., and F. Wu. 2007. A stratospheric ozone profile data set for 1979-2005: Variability, trends, and comparisons with column ozone data. *Journal of Geophysical Research* 112 (D6):D06313.

Salawitch, R.J., D.K. Weisenstein, L.J. Kovalenko, C.E. Sioris, P.O. Wennberg, K. Chance, M.K.W. Ko, and C.A. McLinden. 2005. Sensitivity of ozone to bromine in the lower stratosphere. *Geophysical Research Letters* 32: L05811.

Seckmeyer, G., Mayer B., Bernhard G., McKenzie R.L., Johnston P.V., Kotkamp M., Booth C.R., Lucas T., Mestechkina T., Roy C.R.; Gies H.P.; Tomlinson D. 1995. Geographical Differences in the UV Measured by Intercompared Spectroradiometers. *Geophysical Research Letters* 22 (14):1889-1892.

Steinbrecht, W., H. Claude, F. Schönenborn, I.S. McDermid, T. Leblanc, S. Godin, T. Song, D.P.J. Swart, Y.J. Meijer, G.E. Bodeker, B.J. Connor, N. Kämpfer, K. Hocke, Y. Calisesi, N. Schneider, J. de la Noë, A.D. Parrish, I.S. Boyd, C. Brühl, B. Steil, M.A. Giorgetta, E. Manzini, L.W. Thomason, J.M. Zawodny, M.P. McCormick, J.M. Russell III, P.K. Bhartia, R.S. Stolarski, and S.M. Hollandsworth-Frith. 2006. Long-term evolution of upper stratospheric ozone at selected stations of the Network for the Detection of Stratospheric Change (NDSC). *Journal of Geophysical Research* 111:D10308.

von Hobe, M., Salawitch, R. J., Canty, T., Keller-Rudek, H., Moortgat, G. K., Grooß, J.-U., Muller, R., and Stroh, F. 2007. Understanding the kinetics of the ClO dimer cycle. *Atmospheric Chemistry and Physics* 7:3055-3069.

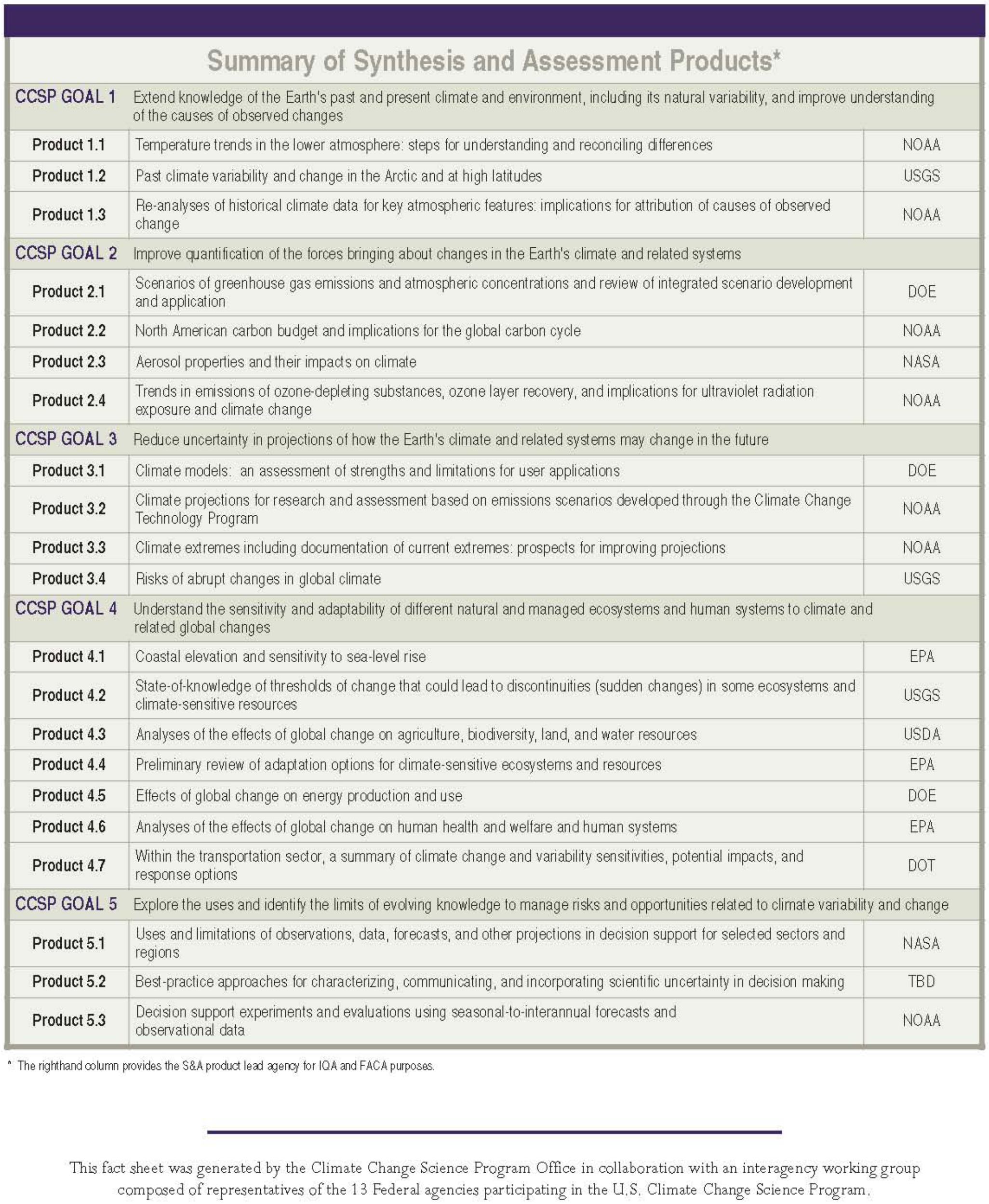
WMO (World Meteorological Organization). 2003. *Scientific Assessment of Ozone Depletion: 2002*. Global Ozone Research and Monitoring Project - Report No. 47. Geneva.

———. 2007. *Scientific Assessment of Ozone Depletion: 2006*. Global Ozone Research and Monitoring Project - Report No. 50. Geneva. Available online: http://ozone.unep.org/Assessment_Panels/SAP/Scientific_Assessment_2006/index.shtml.

Yang, E.-S., D. M. Cunnold, M. J. Newchurch, R. J. Salawitch. 2005. Change in ozone trends at southern high latitudes. *Geophysical Research Letters* 32:L12812.

Appendix A

CCSP Synthesis and Assessment Products



Summary of Synthesis and Assessment Products*

CCSP GOAL 1	Extend knowledge of the Earth's past and present climate and environment, including its natural variability, and improve understanding of the causes of observed changes	
Product 1.1	Temperature trends in the lower atmosphere: steps for understanding and reconciling differences	NOAA
Product 1.2	Past climate variability and change in the Arctic and at high latitudes	USGS
Product 1.3	Re-analyses of historical climate data for key atmospheric features: implications for attribution of causes of observed change	NOAA
CCSP GOAL 2	Improve quantification of the forces bringing about changes in the Earth's climate and related systems	
Product 2.1	Scenarios of greenhouse gas emissions and atmospheric concentrations and review of integrated scenario development and application	DOE
Product 2.2	North American carbon budget and implications for the global carbon cycle	NOAA
Product 2.3	Aerosol properties and their impacts on climate	NASA
Product 2.4	Trends in emissions of ozone-depleting substances, ozone layer recovery, and implications for ultraviolet radiation exposure and climate change	NOAA
CCSP GOAL 3	Reduce uncertainty in projections of how the Earth's climate and related systems may change in the future	
Product 3.1	Climate models: an assessment of strengths and limitations for user applications	DOE
Product 3.2	Climate projections for research and assessment based on emissions scenarios developed through the Climate Change Technology Program	NOAA
Product 3.3	Climate extremes including documentation of current extremes: prospects for improving projections	NOAA
Product 3.4	Risks of abrupt changes in global climate	USGS
CCSP GOAL 4	Understand the sensitivity and adaptability of different natural and managed ecosystems and human systems to climate and related global changes	
Product 4.1	Coastal elevation and sensitivity to sea-level rise	EPA
Product 4.2	State-of-knowledge of thresholds of change that could lead to discontinuities (sudden changes) in some ecosystems and climate-sensitive resources	USGS
Product 4.3	Analyses of the effects of global change on agriculture, biodiversity, land, and water resources	USDA
Product 4.4	Preliminary review of adaptation options for climate-sensitive ecosystems and resources	EPA
Product 4.5	Effects of global change on energy production and use	DOE
Product 4.6	Analyses of the effects of global change on human health and welfare and human systems	EPA
Product 4.7	Within the transportation sector, a summary of climate change and variability sensitivities, potential impacts, and response options	DOT
CCSP GOAL 5	Explore the uses and identify the limits of evolving knowledge to manage risks and opportunities related to climate variability and change	
Product 5.1	Uses and limitations of observations, data, forecasts, and other projections in decision support for selected sectors and regions	NASA
Product 5.2	Best-practice approaches for characterizing, communicating, and incorporating scientific uncertainty in decision making	TBD
Product 5.3	Decision support experiments and evaluations using seasonal-to-interannual forecasts and observational data	NOAA

* The righthand column provides the S&A product lead agency for IQA and FACA purposes.

This fact sheet was generated by the Climate Change Science Program Office in collaboration with an interagency working group composed of representatives of the 13 Federal agencies participating in the U.S. Climate Change Science Program.

Appendix B

Biographical Sketches of Committee Members and Staff

M. Joan Alexander (C*hair*) is a Senior Research Scientist at NorthWest Research Associates, Colorado Research Associates Division (Boulder, Colorado), an Adjoint Associate Professor in the Program for Atmospheric and Oceanic Sciences at the University of Colorado. The focus of her research is on atmospheric waves in the stratosphere. Her work involves both observational analyses and theoretical modeling to better understand the nature of these waves, their properties, their sources, and their many effects in the atmosphere. She currently serves on the Board on Atmospheric Sciences and Climate and will serve as liaison from the board for this committee. She is a fellow of the American Meteorological Society (AMS) and has served as the president of the American Geophysical Union's Atmospheric Sciences Section, as a member of the AMS's Committee on the Middle Atmosphere, and as the secretary of the International Committee for Dynamic Meteorology. Dr. Alexander received her Ph.D. in Astrophysical, Planetary, and Atmospheric Sciences from the University of Colorado, Boulder.

Derek Cunnold is a Professor Emeritus in the School of Earth and Atmospheric Science at the Georgia Institute of Technology. His research interests include remote sensing of the atmosphere, satellite observations, particularly of ozone, and atmospheric transport of trace gases, three-dimensional modeling of the stratosphere, and sources of trace gases and their accumulation in the atmosphere. He has served as a lead author and co-author on previous ozone assessment reports. He was also a contributor to International Geosphere-Biosphere Programme's Overview of the Global Emissions Inventory Activity (2000). He earned his Ph.D. from Cornell.

Terry Deshler is a Professor in the Department of Atmospheric Science and the College of Engineering at the University of Wyoming. Dr. Deshler is an expert in performing and analyzing balloon-borne measurements of stratospheric aerosol and ozone in the mid latitudes and polar regions of both hemispheres. His interests include documenting the severe ozone loss observed above Antarctica each year, investigating polar stratospheric clouds in both hemispheres, and measuring mid and low latitude stratospheric aerosol, including the effects of major volcanic eruptions. Polar stratospheric clouds are essential pre-cursors to polar ozone loss. Mid latitude stratospheric aerosol play a role in the chemical and radiation balance of the stratosphere. Dr. Deshler has published widely in these areas: author or co-author of over 100 refereed scientific publications, co-author of a chapter for the 1998 UNEP/WMO ozone assessment report, author of a chapter for the SPARC assessment of stratospheric aerosols. In 1982, he received his Ph.D. in Physics from the University of Wyoming.

Steven Lloyd is Senior Professional Staff in Atmospheric Chemistry and Radiation at the Applied Physics Laboratory at Johns Hopkins University. His research focuses on measurements of global UV effective reflectivity from the TOMS and SBUV(/2) satellite instruments, validation of these measurements, and spaceborne remote sensing of ozone. He has served as PI on a NASA project called "Modeling and Analysis of the Lower Stratospheric Radiation Field" and as a member of NASA's Solar Occultation Satellite Science Team. In 1993, he earned his Ph.D. from Harvard University. For the book entitled Ecosystem Change and Public Health: A Global Perspective, he wrote the chapter on "The Changing Chemistry of Earth's Atmosphere".

Mack McFarland is an Environmental Fellow at DuPont Fluoroproducts. Dr. McFarland received a B.S. in chemistry from the University of Texas at Austin in 1970 and a Ph.D. in Chemical Physics from the University of Colorado in 1973. From 1974 through 1983, first as a Post-Doctoral Fellow at York University and then as research scientist at the NOAA Aeronomy Laboratory, he planned, conducted, and interpreted field experiments designed to probe the cycles that control atmospheric ozone concentrations. These studies included measurements of gases and processes important to the global climate change issue. In late 1983, he joined the DuPont Company. His primary responsibilities have been in coordinating research programs and assessment and interpretation of scientific information on stratospheric ozone depletion and global climate change as a basis for policy decisions on these global environmental issues. During 1995 and 1996, Dr. McFarland was on loan to the Atmosphere Unit of the United Nations Environment Program, and in 1997, he was on loan to the Intergovernmental Panel on Climate Change (IPCC) Working Group II Technical Support Unit. The value of his contributions to DuPont has been recognized through the 2007 Pedersen Award, a C&P Flagship Award, Environmental Respect Awards, and Environmental Excellence Awards. In 1999, he was awarded an individual Climate Protection Award by the U.S. Environmental Protection Agency for his contributions in providing understandable, reliable information to decision makers. Dr. McFarland has served on the NRC Committee Panel for Chemical Science and Technology and the Committee on the Analysis of Global Change Assessments. He has participated in every major international scientific assessment on stratospheric ozone and global climate change as author, reviewer, or review editor."

Michelle Santee is the Group Supervisor of the Microwave Atmospheric Science Group at NASA's Jet Propulsion Laboratory. Her research interests include: polar processes in the lower stratosphere, such as chlorine activation and deactivation, polar stratospheric cloud formation, denitrification and dehydration, and ozone loss; processes controlling HNO3 in the upper troposphere/lower stratosphere (UT/LS); transport of pollution in the UT/LS; and the influence of solar storms on the upper stratosphere. She contributed to the polar chapter of the 2006 Scientific Assessment of Ozone Depletion. In 1993, she earned her Ph.D. in Planetary Science from California Institute of Technology.

Theodore G. Shepherd is a Professor of Physics at the University of Toronto. Stimulated by his role as PI in the development and use of the Canadian Middle Atmosphere Model since 1992, his applied research focuses on the middle atmosphere, including the ozone layer. He was a member of the Steering Committee for the 2006 Scientific Assessment of Ozone Depletion. As a part of WCRP's SPARC, he was a co-chair of the initiative on Detection, Attribution, and Prediction of Stratospheric Change. For a 2005 IPCC Special Report entitled Safeguarding the Ozone Layer and the Global Climate System, Dr. Shepherd co-authored the chapter called "Ozone and Climate: A Review of Interconnections". He received his Ph.D. in 1984 from MIT, and was just made a Fellow of the Royal Society of Canada.

Margaret Tolbert is a Professor in the Department of Chemistry and Biochemistry at the University of Colorado. Dr. Tolbert is a pioneer in understanding the chemistry and physics of particles in the atmosphere. In landmark work, she identified the role of surface reactions on solid and liquid stratospheric clouds. She proposed a new phase for some polar stratospheric clouds, and demonstrated key cirrus cloud processes in midlatitude ozone depletion. She served on the review panel of the 1994 ozone assessment report. She has also served as Associate Editor of the journal Atmospheric Environment. She is a member of the National Academy of Sciences, and she has served on the National Academies' Committee on Atmospheric Chemistry. She earned her Ph.D. in 1986 from California Institute of Technology.

Don Wuebbles is the Director of the School of Earth, Society, and Environment and a Professor in the Department of Atmospheric Science at the University of Illinois. Dr. Wuebbles spent many years as a research scientist and group leader at the Lawrence Livermore National Laboratory before returning to the University of Illinois in 1994. His research emphasizes the development and use of mathematical models of the atmosphere to study the chemical and physical processes that determine atmospheric structure. He is the author of about 400 scientific articles, most of which relate to atmospheric chemistry and global climate change as affected by both human activities and natural phenomena. He also directs a number of research projects that are primarily oriented towards improving our understanding of the impacts that man-made and natural trace gases may be having on the Earth's climate and on tropospheric and stratospheric chemistry, with emphasis on concerns about global ozone. He has played a major role in the national and international assessments of the effects of chlorofluorocarbons (CFCs) and other halocarbons on ozone. He developed the concept of Ozone Depletion Potentials used in most policymaking relative to protection of the ozone layer (e.g., the Montreal Protocol and its amendments, the U.S. Clean Air Act). Dr. Wuebbles and his research team have developed a range of computational models of global atmospheric chemistry and physics that are applied to the study of natural (e.g., volcanic eruptions; solar flux variations; solar particle effects) and human-related (e.g., emissions of CFCs, halons, other halocarbons, methane, plus nitrogen oxides and other gases and aerosols from aircraft) effects on tropospheric and stratospheric ozone. These models are also used in studies along with data from atmospheric measurements to further our understanding of the relationships between chemical and physical processes in the atmosphere. He has served as a lead author and co-author on most of the international ozone assessments. He served on the NRC Committee to Assess Fire Suppression Substitutes and Alternatives to Halon. He earned his Ph.D. in Atmospheric Science at UC Davis.

Staff:

Leah Probst is a research associate with the NRC's Board on Atmospheric Sciences and Climate and Polar Research Board. Since joining the NRC staff in 1999, Ms. Probst has led studies on the science and implementation plan for the World Climate Research Programme's Americas Prediction Project and on the proposed Global Precipitation Measurement satellite mission. She works with the U.S. National Committee on the International Polar Year 2007-2008 and with the NRC's Climate Research Committee. She has contributed to many other NRC studies, including topics such as surface temperature reconstructions for the last 2,000 years, the

Tropical Rainfall Measuring Mission satellite program, the New Source Review Program of the Clean Air Act, and cumulative effects of oil and gas activities on Alaska's North Slope. She received a B.A. in biology from George Washington University.

Katherine Weller is a Senior Program Assistant for the NRC's Board on Atmospheric Sciences and Climate and the Polar Research Board. Since joining the National Academies in 2006, Ms. Weller has worked with the Climate Research Committee, and has worked with committees to review the Climate Change Science Program's Synthesis and Assessment Products 3.3 and 5.2. In 2004, she received a B.S. in biopsychology from the University of Michigan. She is currently working toward a master's degree in environmental science and policy from Johns Hopkins University.