

Report on the WCRP/JSC Meeting

Tokyo, Japan, 13-17 March, 2000

Marie-Lise Chanin and Marvin Geller, (SPARC Co-Chairs)

The co-chairs of the SPARC Scientific Steering Group (SSG), Marie-Lise Chanin and Marvin Geller, attended the 21st session of the Joint Steering Committee for the World Climate Research Programme in Tokyo, held during March 13-18, 2000. Drs. Chanin and Geller made a joint presentation that reviewed the progress of SPARC during the past year as reported in the SPARC Newsletter N° 14, and indicated some new directions that are being considered. The presentation closely followed the SPARC thematics according to the following outline.

Stratospheric Indicators of Climate Change

1. Stratospheric Temperature Trends
2. Vertical profile of Ozone Trends
3. Upper Troposphere/Lower Stratosphere (UT/LS) Water Vapour Climatology and Trends

Stratospheric Processes and Their Relation to Climate

4. Dynamics and Transport in the UT/LS
5. Chemistry and Microphysics in the UT/LS
6. Gravity Wave Processes and their Parameterization

Modelling Stratospheric Effects on Climate

7. Climate-Middle Atmospheric Modelling: GRIPS
8. Stratospheric Reference Climatology
9. Stratospheric Aspects of Climate Forcing
10. Dynamical Coupling of the Stratosphere and Troposphere

Other Scientific Issues

11. Data Assimilation
12. Solar Forcing and Climate Variability
13. Monitoring Stratospheric Change

Other Topics

14. Review of Overall SPARC Strategy and Status of Implementation
15. Cooperation with Other International Programmes
16. Planning for the 2nd SPARC General Assembly
17. SPARC Data Centre
18. SPARC Office

The discussion on each of these topics closely reflected the Report on the Seventh Meeting of the SPARC SSG that was presented in SPARC Newsletter No. 14. The JSC felt that the SPARC project was progressing quite well, but did present some advice on various topics. These included the following.

Data Assimilation

The JSC urged that the SPARC efforts in data assimilation should be closely coordinated with those of WGNE (the Working Group on Numerical Experimentation).



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Stratospheric Aerosols

The JSC felt that consideration should be given to pursuing this aerosol effort as a joint SPARC-IGAC-GEWEX activity. IGAC should be involved given the importance of aerosol chemistry, and GEWEX because of the aerosol impact on radiation and also its role in cloud nucleation.

Next Phase of SPARC

The JSC indicated that it considered SPARC's efforts so far to be very suc-

cessful, but that it was timely to consider its future. When asked about when SPARC might consider its efforts complete, Drs. Geller and Chanin indicated not until the role of the stratosphere is properly integrated into the other WCRP projects, will it be time to end SPARC.

Solar Forcing and Climate Variability

The JSC agreed that it is desirable to pursue studies of Upper Atmosphere Trends and Solar Influences on

Climate jointly with SCOSTEP. It was also felt that GEWEX should undertake studies of how the occurrence of clouds might depend on solar activity.

Data Activities

The JSC indicated that it was desirable that SPARC and GEWEX link their data bases to one another.



Small Organic Peroxy Radicals: Big Players in Ozone Production

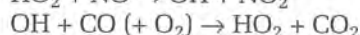
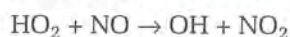
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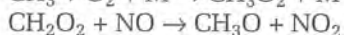
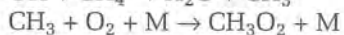
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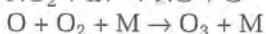
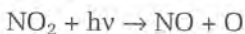
Peroxy radicals are central to ozone photochemistry in the troposphere and lower stratosphere. The simplest of the family, HO_2 , interconverts readily with OH on a time scale of seconds.



Organic peroxy radicals are formed in the oxidation of hydrocarbons and other organic molecules. For example, methane reacts to form the methyl peroxy radical, CH_3O_2 , which reacts rapidly with NO to give the methoxy radical.



The reactions of peroxy radicals with NO are the critical steps in the photochemical formation of ozone, since the NO_2 generated photodissociates at wavelengths which are available throughout the troposphere ($\lambda > 300 \text{ nm}$).



This combination of a hydrocarbon "fuel", NO "catalyst", and sun light lead to photochemical ozone production when molecular oxygen cannot be photodissociated (Figure 1)

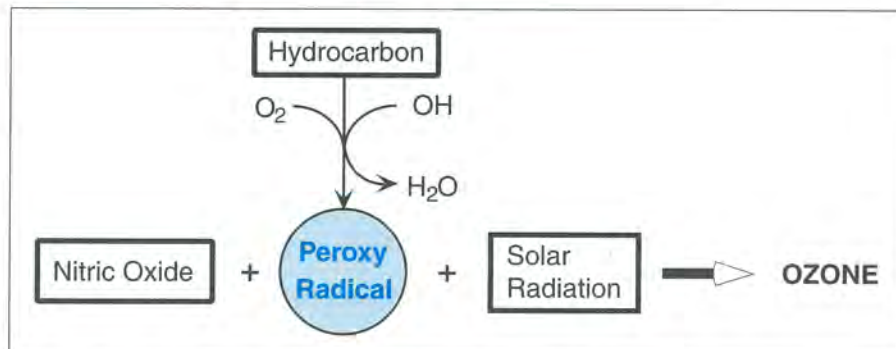
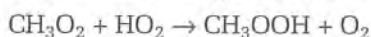


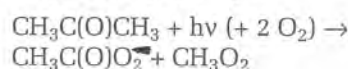
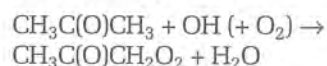
Figure 1: A simplified scheme that shows the pathway for the photochemical production of ozone in the troposphere. The key role of peroxy radical in ozone production is exemplified.

In NO_x -poor regions of the atmosphere, reactions between peroxy radicals become more important. Of particular interest are those between peroxy radicals and HO_2 , which usually lead to the formation of a stable peroxide product, such as methyl hydroperoxide.



Further physical removal of this peroxide, e.g. by rain out, can lead to permanent removal of the radicals from the atmosphere, and hence a change in the oxidising potential of the region. The interactions centred on peroxy radicals, including the routes for ozone and free radical formations are shown in Figure 2.

Many of the products formed in peroxy radical reactions (peroxides, carbonyls) can photodissociate to produce free radicals. For example, it has recently been proposed that acetone can compete with ozone photolysis as a source of HO_x in the upper troposphere and lower stratosphere. The destruction of acetone leads to the formation of acetyl peroxy radicals, $\text{CH}_3\text{C}(\text{O})\text{CH}_2\text{O}_2$, in the case of OH attack, or acetyl peroxy, $\text{CH}_3\text{C}(\text{O})\text{O}_2$, and methyl peroxy radicals if photolysis occurs.



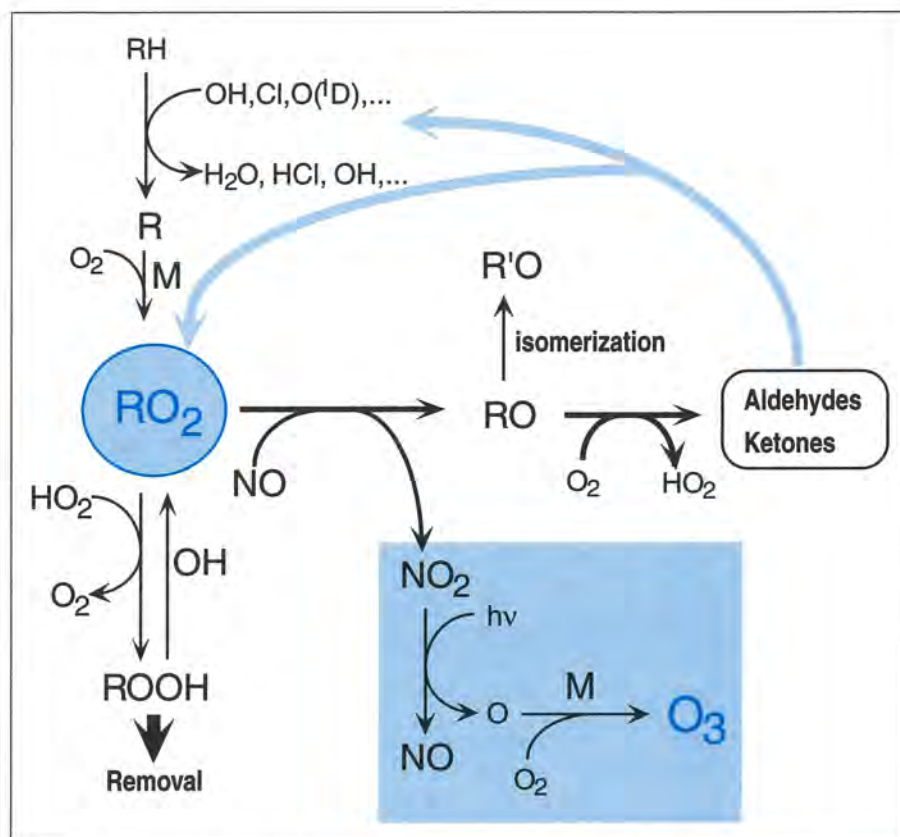


Figure 2: A schematic of the chemical processes involved in the oxidation of hydrocarbons in the atmosphere in the presence of nitrogen oxides that leads to the production of ozone, other free radicals, and stable molecules.

In light of the recent modelling activity regarding acetone and the organic hydroperoxides, it was considered timely to evaluate what is known about the reactions of the simple peroxy radicals with each other and with HO_2 , NO and NO_2 .

Reactions of peroxy radicals have been studied in the laboratory for over 20 years now. The studies have not always been definitive, since good, direct ways to detect the radicals have been slow in development. Almost all the studies have used ultraviolet absorption spectroscopy, which is not particularly sensitive, or specific to a given radical. The studies are further complicated by the fact that the products of the reactions are often other peroxy radicals which absorb in the same spectral region and undergo reactions with the peroxy radical of interest. Add to this the necessity to generate and detect two radicals simultaneously, and it is clear that great care needs to be taken in planning and executing an experiment. Furthermore, previous data was not always reinterpreted in light of new information. With all these things in

mind, an evaluation of the chemistry of some atmospherically important peroxy radicals has been prepared under the SPARC and IGAC tasks related to laboratory studies. Participants were **G.S. Tyndall** (NCAR, chair), **R.A. Cox** (Cambridge, IGAC co-ordinator for the Laboratory Studies joint project), **R. Lesclaux** (Bordeaux), **G.K. Moortgat** (MPI Mainz), **M.J. Pilling** (Leeds), **A. R. Ravishankara** (NOAA, SPARC co-ordinator for the Laboratory Studies joint project) and **T.J. Wallington** (Ford Motor Co). After an initial informal discussion in Bilbao, Spain during the Gas Kinetics Symposium, a first workshop took place at the CNRS/Service d'Aéronomie laboratory in University Paris 6, Jussieu, from 7th to 9th December, 1998 to discuss the database. A second work-shop from 29th April to 1st May, 1999, in Boulder enabled the evaluation to be finalised and a first draft of a manuscript to be assembled. All the participants have been involved in peroxy radical chemistry, and were in a position to evaluate their own and other studies critically. The aim of the exercise

was to formulate a consistent kinetics data set for use in atmospheric chemistry, and to reanalyse data from older studies using these new parameters when necessary to eliminate or reduce systematic uncertainties. The manuscript is now complete, and additionally contains the results of a 3D model (C. Granier, CNRS-SA, NOAA/CIRES-University of Colorado/ MPI-Hamburg) run with the previously and currently recommended reaction rate coefficients. A summary of the major findings of the evaluation follows.

Ultraviolet Absorption Cross Sections

All the peroxy radicals studied here have absorption spectra in the ultraviolet, below 300 nm. While this absorption does not facilitate photolysis in the lower atmosphere, the spectra are important for laboratory determinations of concentration. Two parameters are necessary for a good description of the spectra – a well-defined shape (absorption as a function of wavelength) and a quantitative measure of the magnitude of the absorption cross section. The novel approach taken in the current evaluation was to evaluate these issues separately – that is because the experimental artefacts that could lead to systematic errors in the two are not necessarily the same. Some of the difficulties encountered include absorption by multiple species, incomplete conversion of starting material into the required radical and an inadequate knowledge of the concentration of radical precursor (which was often chlorine atoms). Furthermore, advances in the understanding of interfering reactions, particularly those between different free radicals, enabled the accuracy of the actinometry to be assessed much more thoroughly than was possible at the time of most of the measurements. All the published spectra were fitted to a common functional form, which enabled outliers to be readily identified, and a best shape to be determined. Then, the experimental conditions were carefully screened to ascertain which absolute determinations were likely to be free of systematic uncertainties, and these data were used to scale the best shape spectrum.

Table 1: Summary of Recommended Rate Coefficients

Reaction	Number	k(298 K)	A	E/R (K)	Uncertainty at 298 K
$\text{CH}_3\text{O}_2 + \text{HO}_2$	1	5.2 e-12	4.1 e-13	-750±150	1.3
$\text{CH}_3\text{O}_2 + \text{CH}_3\text{O}_2$	2	3.4 e-13	9.5 e-14	-390±100	1.2
$\text{CH}_3\text{O}_2 + \text{NO}$	3	7.7 e-12	2.8 e-12	-300±100	1.15
$\text{CH}_3\text{O}_2 + \text{NO}_2$	4 (k_0)	1.3 e-30		$n = 4.0 \pm 2.0$	1.3
	4 (k_∞)	7.6 e-12		$m = 2.0 \pm 2.0$	1.2
$\text{CH}_3\text{O}_2 + \text{O}_3$	5	1.0 e-17	3.0 e-16	1000±500	3.0
$\text{C}_2\text{H}_5\text{O}_2 + \text{HO}_2$	6	7.8 e-12	7.4 e-13	-700±200	1.3
$\text{C}_2\text{H}_5\text{O}_2 + \text{NO}$	7	8.9 e-12	2.7 e-12	-350±50	1.1
$\text{C}_2\text{H}_5\text{O}_2 + \text{NO}_2$	8 (k_0)	4.9 e-30		$n = 5.8 \pm 2.0$	1.5
	8 (k_∞)	7.6 e-12		$m = 0.9 \pm 0.9$	1.5
$\text{CH}_3\text{C(O)O}_2 + \text{HO}_2$	9	1.4 e-11	4.3 e-13	-1040±500	2.0
$\text{CH}_3\text{C(O)O}_2 + \text{CH}_3\text{O}_2$	10	1.1 e-11	2.0 e-12	-500±250	1.5
$\text{CH}_3\text{C(O)O}_2 + \text{CH}_3\text{C(O)O}_2$	11	1.4 e-11	2.5 e-11	-500±200	1.4
$\text{CH}_3\text{C(O)O}_2 \pm \text{O}$	12	2.0 e-11	8.1 e-12	-270±100	1.15
$\text{CH}_3\text{C(O)O}_2 + \text{NO}_2$	13 (k_0)	8.5 e-29		$n = 6.5 \pm 2.0$	1.2
	13 (k_∞)	1.1 e-11		$m = 1.0 \pm 1.0$	1.2
$\text{CH}_3\text{C(O)CH}_2\text{O}_2 + \text{HO}_2$	14	9.0 e-12	8.6 e-13	-700±300	2.0
$\text{CH}_3\text{C(O)CH}_2\text{O}_2 + \text{CH}_3\text{O}_2$	15	4.0 e-12	7.5 e-13	-500±300	2.0
$\text{CH}_3\text{C(O)CH}_2\text{O}_2 + \text{NO}$	16	8.0 e-12	2.8 e-12	-300±200	1.5
$\text{CH}_3\text{C(O)CH}_2\text{O}_2 + \text{NO}_2$	17	6.4 e-12(1atm)	2.3 e-12	-300±300	1.5

Rate coefficients are given in units of $\text{cm}^3 \text{ molecule}^{-1} \text{ s}^{-1}$.

For bimolecular reaction, the temperature dependent expression is given by $k(T) = A \exp [-(E/R/T)]$.

For pressure dependent reactions (4,8,13) the temperature dependent expressions are $k_0(T) = A(T/298)^{-n}$ and $k_\infty(T) = A(T/298)^{-m}$.

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Reaction Kinetics and Mechanisms

The major goal of this task was to evaluate the current body of kinetic data on peroxy radicals using a consistent set of ultraviolet absorption cross sections. Reanalysis of existing data was performed whenever possible/necessary. Based on the accumulated experience of the panel members, sources of systematic errors were identified and the affected data sets were either corrected or rejected. Data pertaining to the products of each reaction were first evaluated, since an accurate knowledge of the product distribution is often required to derive the kinetic data from the experimental results. The rate coefficients were then evaluated using the recommended absorption cross sections (for absorption techniques) and product distributions. The evaluated rate coefficients are given in Table 1.

The largest changes (and most discussion) involved the chemistry of the acetyl peroxy radical. This is related to the fact that its reactions lead to a "cascade" of other radicals, all of which contribute to the measured UV absorption and to the observed kinetic decays. The reaction of acetyl peroxy with CH_3O_2 had one of the highest uncertainties of all those

considered. Kinetic data obtained using fairly similar techniques were in tolerable agreement, but the analysis had been conducted using different product distributions. A careful analysis of the available product studies indicated that the majority of the reaction at room temperature proceeds by formation of free radicals, $\text{CH}_3 + \text{CO}_2 + \text{CH}_3\text{O} + \text{O}_2$. The other channel, which produces acetic acid, $\text{CH}_3\text{C(O)OH}$, has been suggested as a source of acetic acid in the free troposphere. However, no strong evidence could be found for its occurrence. When the available kinetics studies were analysed using a consistent product distribution, the apparent agreement in the rate coefficients actually worsened! The evaluated rate coefficient, $1.1 \times 10^{-11} \text{ cm}^3 \text{ molecule}^{-1} \text{ s}^{-1}$ at 298 K, while thought to be more reliable than previously, is still subject to some uncertainty, and demands further study.

Despite two decades of study, it is clear that more studies are required, particularly at low temperatures. A major recommendation of the evaluation is that new, more specific detection techniques for the study of peroxy radical kinetics be developed. As noted earlier, UV absorption is neither sensitive nor specific, and the results obtained are often ambiguous.

Mass spectrometric or high-resolution optical techniques using a different spectral region may be amenable for this goal. Also, use of more diverse ways of producing the radicals would be beneficial. Finally, it is hoped that the evaluation will be used as a solid basis for both the design and analysis of future experiments.

Claire Granier of this group briefly assessed the impact of the evaluated rate coefficients using the IMAGES model, a 3D model developed at NCAR in Boulder. Not all the possible consequences were examined. Figure 3 shows the changes in the calculated concentration of PAN at two altitudes when the rate coefficients recommended in Table 1 are used in place of the values used in current models. This is provided only as an example, and further modelling studies in the future will elucidate the consequences of all the changes noted here.

A detailed paper describing this analysis and evaluation will be submitted to JGR-Atmosphere in June 2000. The database used in this analysis will be available at the SPARC data center in the near future. The availability of this database will be announced in a future newsletter.



The SPARC Data Center

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The SPARC Data Center (SPARC-DC) has been in operation since July 1999. Located at the State University of New York at Stony Brook, it has been established as a resource for the linkage, storage, and exchange of scientific data pertinent to the different SPARC working groups. Furthermore, it is designated to provide simple post-processing and analysis of data for SPARC projects. Funded by NASA, Marvin Geller heads the SPARC-DC as the principal investigator with William Randel and Kevin Hamilton co-investigators. Petra Udelhofen is the scientist responsible for operating the SPARC-DC and will provide an overview over the activities of the data center in its year of inception.

The SPARC-DC computer consists of a fast dual processor workstation, which serves the web at www.sparc.sunysb.edu (Figure 1). Data can be downloaded via the web site or anonymous ftp (ftp-address: [atmos.sparc.sunysb.edu](ftp://atmos.sparc.sunysb.edu), directory/pub/sparc). Several working groups have transferred data to the SPARC-DC most of which are publicly available.

In the last months, the SPARC-DC obtained data describing climate forcing parameters that influence stratospheric change (Karoly, 2000). Robert Portmann provided a combined tropospheric and stratospheric data set by merging the global stratospheric ozone trends data set by Randel and Wu (1999) with the ozone data set by Kiehl *et al.* (1999). The Randel and Wu data set can also be obtained from the SPARC-DC. Judith Lean contributed reconstruction of solar irradiance from 1610 to present (currently 1999, with yearly updates) (Lean *et al.*, 1995) and high resolution spectral estimates of solar irradiance from 115.5nm to 425.5nm during solar maximum and solar minimum based on satellite measurements (Lean *et al.*, 1997).

Steven Pawson transferred model data from fourteen groups of the GCM Reality Project for SPARC

(GRIPS) to the SPARC-DC (Pawson *et al.*, 2000). To make the data readable across platforms, the SPARC-DC converted the binary files into NETCDF. Time series of three-dimensional monthly mean temperature and geopotential height fields and zonal means of temperature and winds are available. Several GCMs provided additional model diagnostics, which vary from one model to the next.

In January 2000, the Water Vapour Assessment (WAVAS) working group decided at a review meeting to archive data sets used in the WAVAS report at the SPARC-DC. These data sets include water vapour measurements from aircraft campaigns, ground measurements, and satellite data. The

trace gas climatologies for CH₄, H₂O, O₄, HF, HCl, NO₂ (sunrise and sunset), NO (sunrise and sunset), ClO, N₂O, and HNO₃ based on UARS measurements. Web site displays of these data sets were produced at the SPARC-DC (Figure 2). In an ongoing effort for the SPARC Reference Climatology project, Randel derives monthly mean model climatologies (Table 1) which are updated and transferred regularly to the SPARC-DC. Model variables are the monthly and zonal means of temperature, wind, eddy momentum flux, eddy heat flux, EP flux divergence, and geopotential heights. Additional data sets encompass tropopause heights derived from NCEP data.

Model	Time Coverage	Months
ECMWF	01/1979-12/1993	180
NCEP-RA	01/1958-12/1999	504
NCEP-CPC	10/1978-05/1999	248
UKMO	11/1991-02/2000	100

ECMWF: European Centre for Medium Range Weather Forecast;
NCEP: National Center for Environmental Prediction; RA-Reanalyses;
CPC-Climate Prediction Center; UKMO: UK Meteorological Office

Table 1: Monthly Mean Reference Model Data available at the SPARC Data Center (Source: W.J. Randel). Variables include the temperature and geopotential heights (3D Grids) and temperature and zonal mean wind components (zonal average fields). The table summarises the data holdings as of April, 2000. These data sets will be updated periodically to extend the time coverage.

working group requested to have these data in one location to facilitate access. Originally, the water vapour data were scattered across twenty-three computer sites with eight of them password protected. Now, the SPARC-DC mirrors the data sets and provides a centralised archive. Some water vapour data are restricted in usage following the wishes of the authors. For example, restrictions apply when the original authors have not yet published their data and want to be consulted about further data usage. Directions on how to obtain a password for the restricted data sets are given on the SPARC-DC web site.

At the outset of the SPARC-DC, William Randel provided zonal mean

Another ongoing effort at the SPARC-DC is the acquisition and publication of high-resolution radiosonde data for the SPARC Gravity Wave Initiative. Figure 3 shows the station location of US upper air sites that provide radiosonde ascents with measurements every six seconds. The SPARC-DC has purchased radiosonde data from NOAA for 1998 and this data set will be extended for three more years in the near future utilising NSF research funds.

The SPARC-DC encourages the different working groups to make use of its facilities. To ensure the portability to different computer systems, these data sets should be formatted in ASCII or NETCDF. Certain binary data may be accepted and converted into NETCDF at the SPARC-DC. The SPARC-DC does not intend to duplicate data when these are available from other data centres, except under circumstances when data mirroring may alleviate access through the Internet. Generally, the SPARC-DC provides linkages to useful existing sites and collects data that are unique to SPARC projects. Typically, the data will be made available to the public. Password-protected access will be provided for data that have not yet been published. However, the SPARC-DC does not intend to distribute data sets that require more complicated data protocols.

In its year since inception, the primary efforts of the SPARC-DC have been on setting up the site, collecting data for the different working groups, and converting them into portable formats. In the future, its focus will shift towards performing data analyses particularly within, but not limited to, the framework of the SPARC Reference Climatology Project.

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Interannual Changes of Temperature and Ozone

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The strength of the wintertime vortex is reflected in polar temperature, which undergoes large variations between years. So does total ozone at extratropical latitudes. Both are regulated by the residual mean circulation of the middle atmosphere. Air descending at high latitudes of the winter hemisphere experiences adiabatic warming that offsets radiative cooling, maintaining wintertime temperatures well above radiative equilibrium. Residual mean motion also transfers ozone poleward and downward, its column abundance increasing steadily during winter until reaching its spring maximum.

Interannual changes are an important focus of stratospheric research. Understanding them is necessary to properly identify anthropogenic changes. Interannual changes of dynamical structure over the 1980s and 1990s have been studied in global analyses from ECMWF. Dynamical changes in the lower stratosphere (LS) are investigated in relation to contemporaneous changes of total ozone from TOMS, likewise concentrated in the LS. These changes of dynamical and chemical structure are then related to interannual changes of temperature and ozone in the upper stratosphere observed by the French lidar at Observatoire de Haute-Provence (OHP).

Annual Variation of Dynamical Structure

Figure 1 shows the mean annual cycle of column-averaged temperature at 100-10hPa over the Arctic T (solid), which has been constructed by averaging ECMWF analyses over the years 1979-1998. Following summer solstice, polar temperature decreases steadily until reaching a minimum shortly after winter solstice. Interannual variance of polar temperature (shaded) has similar seasonality, increasing sharply during the

disturbed months of winter. Planetary waves propagating upward from the troposphere at this time of year drive downwelling over the Arctic, which introduces adiabatic warming.

Superposed in Figure 1 is the mean annual cycle of upward EP flux at 100hPa $\bar{F}_z$ (dashed), averaged over the northern extratropics. It represents westward momentum that is transmitted upward from the troposphere by planetary waves and is largely absorbed in the middle atmosphere.

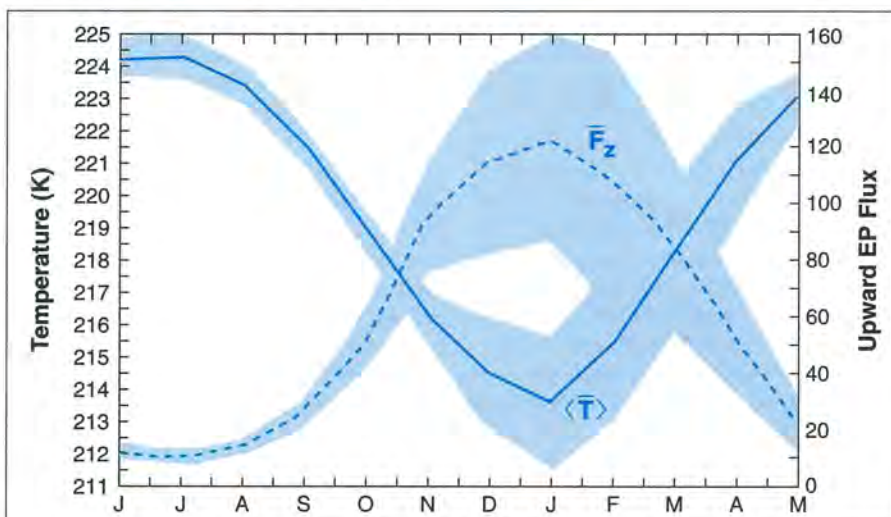


Figure 1: Mean annual cycle of column-averaged temperature above 100hPa (solid) and upward EP flux at 100hPa (dashed), normalised, each averaged over 50°N-90°N, along with the std. deviation of their interannual variability (shaded).

As such, $\bar{F}_z$ measures overall wave driving of the residual mean circulation and thus downwelling over the Arctic, where adiabatic warming offsets radiative cooling. $\bar{F}_z$ has seasonality just opposite to $\langle \bar{T} \rangle$ maximising shortly after winter solstice, when perpetual darkness over the Arctic leads to strong LW cooling. The interannual variance of $\bar{F}_z$ (shaded) has similar seasonality. Largest changes of upward EP flux from the troposphere are observed during the dynamically disturbed season.

The annual cycle of $\bar{F}_z$ implies strong adiabatic warming during winter that offsets strong LW cooling, controlling the minimum polar temperature that is achieved each year around solstice. By driving residual mean motion that transfers ozone-rich air poleward and downward, $\bar{F}_z$ also controls the spring maximum of total ozone. About their mean annual cycles, temperature, ozone, and upward EP flux from the troposphere each experience sizeable changes from one year to the next. Over the two decades under consideration, those changes define a cloud of some 20 realisations of the annual cycle, as characterised by the rms variation in Figure 1 (shaded). Each realisation of the annual cycle determines the inter-annual anomaly, defined as the deviation from the 20-year mean annual variation. In the analysis below, we explore how the anomaly during an individual year in one of these properties is related to the anomaly during the same year in the other properties.

Interannual Changes of Dynamical Structure

Interannual changes of wintertime temperature are large over the polar cap. Their vertical structure is deep. Figure 2a shows the anomalous wintertime temperature during January 1980. It's large over the Arctic, where anomalous temperature is uniformly negative (January temperatures colder than average). Anomalous temperature increases upward to the roof of

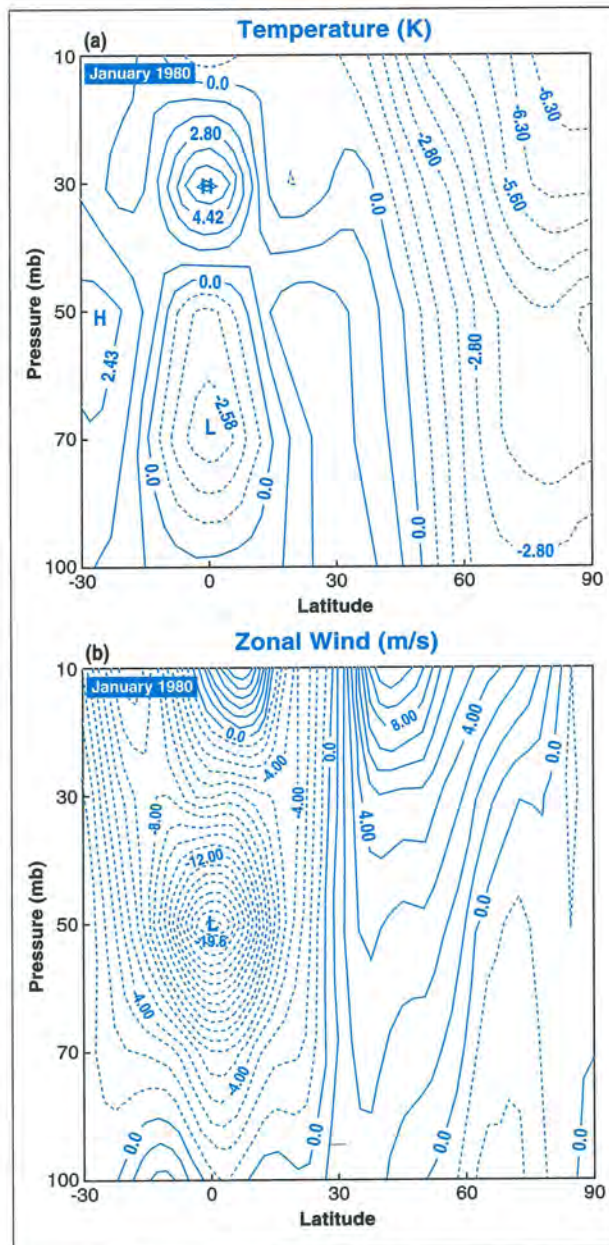


Figure 2: (a) Anomalous zonal-mean temperature, as a function of latitude and pressure, for January 1980. (b) As in (a), but for zonal-mean wind.

the ECMWF analyses, amplifying to more than 7 K by 10hPa. At sub-polar latitudes, it has reversed sign, but it's much smaller. Over the equator, values are dominated by the temperature signature of the QBO, which is out of phase between 70 and 30hPa.

Zonal-mean wind is shown in Figure 2b, also for January 1980. Anomalous wind is positive at high latitudes, westerlies increasing upward along the axis of the polar-night jet to the highest level represented in the analyses. In the tropics, QBO easterlies prevail over the entire LS. This distribution of equatorial wind displaces the critical line of planetary waves into the winter hemisphere, favouring wave absorption farther northward and a polar-night

vortex that is anomalously warm and weak (Holton and Tan, 1980; Labitzke, 1982). Yet, during this particular year, the January vortex is actually anomalously cold and strong.

During the winter in Figure 2, upward EP flux from the troposphere is anomalously weak (not shown). This implies a residual circulation during 1980 that is likewise anomalously weak. Through reduced downwelling and adiabatic warming, anomalously weak $\bar{F}_z$ also implies a polar-night vortex that is anomalously cold and strong, as is observed.

The correspondence during 1980 between anomalous temperature and wave driving by the troposphere, in fact, reflects a more general relationship. Figure 3 compares the anomalous column averaged temperature over the Arctic during January (solid), as a function of year, against the corresponding record of anomalous $\bar{F}_z$ (dashed), integrated over the northern extratropics and during the preceding months of winter. The latter represents the cumulative wave driving of residual motion leading up to the temperature minimum in January. The relationship is clear and it's strong: Interannual changes of temperature over the Arctic closely track interannual changes of westward momentum trans-

mitted to the middle atmosphere by planetary waves. With a correlation of 0.89, changes of $\bar{F}_z$ account for almost all of the interannual variance of minimum polar temperature.

Anomalous Structure of Interannual Changes

The structure of interannual changes has been composited from its spatial coherence, with 100hPa temperature over the North Pole serving as a reference time series. Figure 4 shows the anomalous January temperature corresponding to a 1-std deviation increase of T_{100}^{NP} . Like anomalous structure during 1980, it's deep – significant at the 95% level (shaded) over most of the winter hemisphere. Anomalous

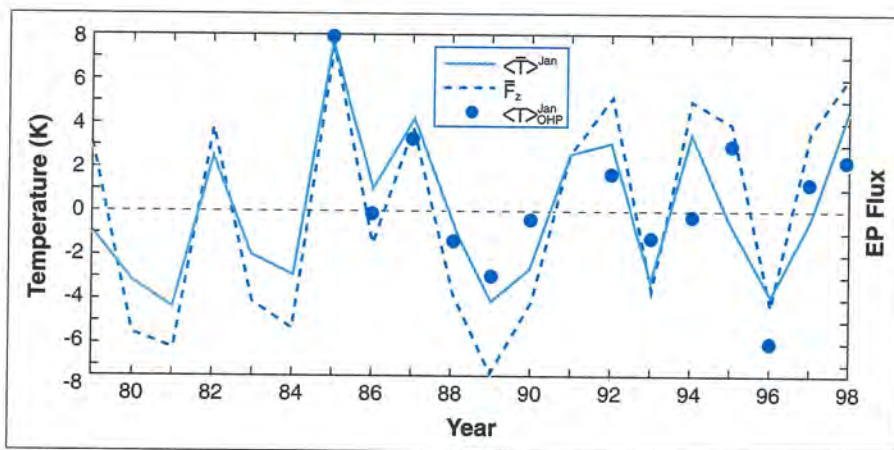


Figure 3: Anomalous column-averaged (100-10hPa) temperature during January (60°N-90°N), as a function of year, compared against the anomalous upward EP flux from the troposphere (50°N-90°N) integrated over the preceding months Jan.-Oct. (normalised). Superposed is the column-averaged temperature above 30km (circles) during January, observed at OHP.

temperature is strong over the Arctic, where positive values reflect enhanced downwelling and adiabatic warming. It increases upward, approaching 7 K near 30hPa. That's close to the overall std deviation of polar temperature in the LS, as changes operating coherently in the vertical account for nearly all of its interannual variance. South of 50°N, anomalous temperature is negative, reflecting enhanced upwelling and adiabatic cooling. However, although just as significant as changes at high latitudes, temperature changes in the tropics are an order of magnitude weaker.

The smallness of temperature changes in the tropics has been argued as evidence for no change of the residual circulation, leaving much larger changes at high latitudes to be associated with changes of ozone (WMO, 1999). However, temperature changes in the tropics operate coherently with changes at high latitudes. Their disparate magnitude reflects two basic features of the residual circulation: (1) Temperature tendencies at high latitudes are emphasised by spherical geometry, which leads to strong downwelling and adiabatic warming over the pole, but much weaker upwelling and adiabatic cooling at low latitudes. (2) In polar darkness, radiative damping is slow enough for descending air to warm adiabatically with little diabatic loss, in contrast to faster

radiative damping that operates on ascending air in illuminated regions.

Dynamical Changes in the Upper Stratosphere and Mesosphere

Interannual changes in the LS are accompanied by changes in the upper stratosphere and mesosphere. Those changes are observed by the lidar at OHP, which retrieves temperature above 30km via Rayleigh scattering (Hauchecorne and Chanin, 1980; Keckhut *et al.*, 1995). Superposed in Figure 3 is the anomalous column-averaged temperature above 30km observed at OHP (solid circles).

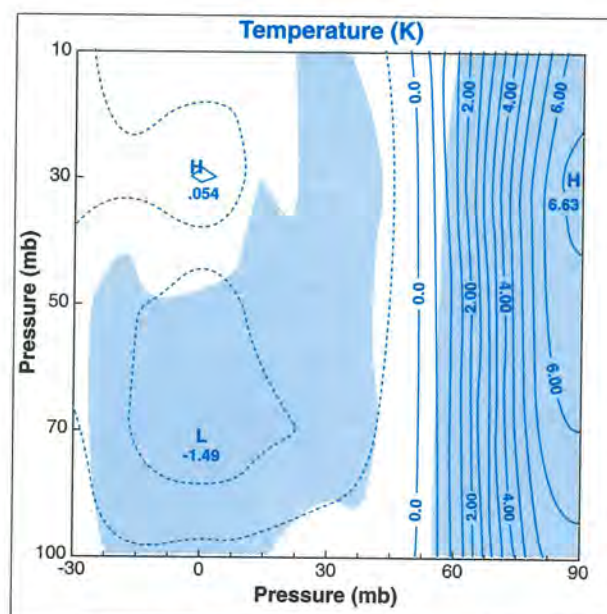


Figure 4: Anomalous January temperature, as a function of latitude and pressure, operating coherently with 100hPa temperature over the North Pole. 95%-significance level shaded.

These are local measurements, in contrast to the horizontally-averaged behaviour from ECMWF. Some years are missing due to limited sampling. Note also that OHP, situated at 44°N, lies well south of the polar cap, where temperatures from ECMWF (solid) have been averaged. Despite these limitations, the OHP record is strongly correlated to temperature changes in the LS. In years when the Arctic LS is anomalously warm, so too is the upper stratosphere above OHP. Just the reverse is observed in years when the Arctic LS is anomalously cold.

Figure 5 shows the vertical structure of anomalous temperature at OHP (solid), which has been composited from its vertical coherence. Anomalous temperature is positive from 30 to 60km (January temperatures warmer than average), where it is 95% significant and changes coherently with temperature in the Arctic LS. Above 60km, temperature also changes coherently with temperature in the LS, but out of phase.

The structure of temperature changes above 30km resembles mean thermal structure (dashed), which is superposed in Figure 5. Both increase upward to about 45km and then decrease into the mesosphere. The profile of anomalous temperature is displaced downward relative to mean temperature by ~5km, reflecting downwelling that tends to lower isotherms. Anomalous temperature reverses sign above 60km. This happens to be the level where the meridional gradient of temperature reverses: coldest temperatures found over the winter pole in the stratosphere, but over the summer pole in the mesosphere. The similarity of temperature profiles in Figure 5 implies that interannual changes at OHP result principally from changes of meridional transport, which lead to a meridional swing of isotherms: OHP is positioned equatorward of strong downwelling over the Arctic and, hence, where residual motion is dominated by poleward transport. During winters when the meridional circulation is strong, warmer isotherms south of OHP and in the stratosphere expand poleward, as the vortex recedes. This is observed at OHP as a shift of stratospheric temperatures to

warmer values. In the mesosphere, however, colder isotherms south of OHP expand poleward, shifting temperatures at OHP to colder values. During winters when the residual circulation is weak, just the reverse occurs.

Relationship to the 3D Circulation

Figure 6 plots the anomalous 100hPa height during January, which has been composited from its spatial coherence with 50hPa height over the North Pole. Anomalous height is strong and positive over the Arctic, where it weakens the polar low and upper-tropospheric westerlies during January. Positive values reflect enhanced downwelling and adiabatic warming, driven by anomalous wave absorption during winters when upward EP flux from the troposphere is enhanced. Surrounding the positive anomaly over the Arctic is negative anomalous height at mid-latitudes. It's concentrated over the north Atlantic, between Europe and North America, and over the north Pacific, between Asia and North America. Like the stronger anomaly over the Arctic, these mid-latitude changes are 95% significant, so they operate coherently with changes of opposite sign over the polar cap.

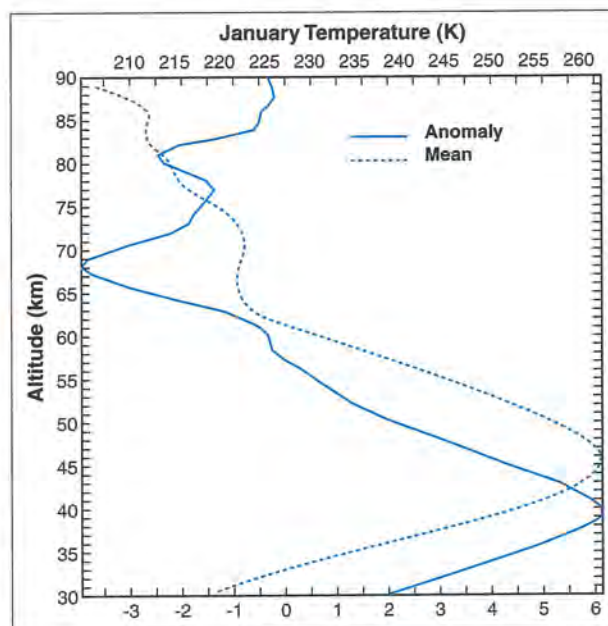


Figure 5: Anomalous January temperature at OHP (solid), as a function of altitude, composited against temperature changes at 40km. Also shown is the 20-year mean January temperature at OHP (dashed).

The anomalous structure in Figures 4 and 6 shares certain features with the so-called Arctic Oscillation (AO) (Wallace and Thompson, 1998; 2000); see the recent review by Baldwin (2000). Derived from the leading EOF of sea-level pressure (SLP), the AO involves temperature changes that maximise over the Arctic in the LS. They decrease sharply at subpolar latitudes, where anomalous temperature has opposite sign and remains comparatively small. Despite these similarities, temperature changes operating coherently with T_{100}^{NP} are deeper than those operating coherently with SLP. They remain strongly coherent to the highest level represented in ECMWF analyses. More importantly, changes in Figure 4 are 2-3 times stronger than changes operating coherently with SLP. These differences imply contributions to anomalous wintertime temperature other

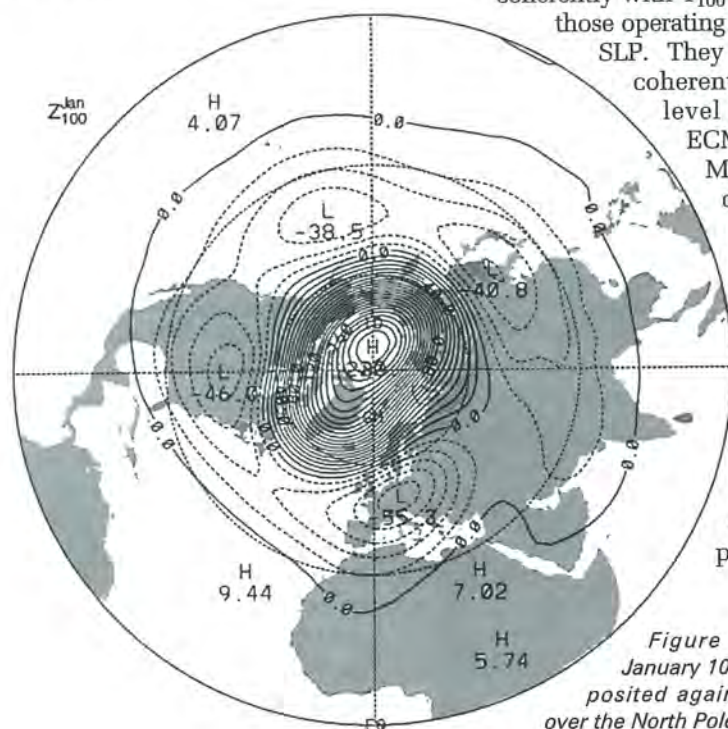


Figure 6: Anomalous January 100hPa height, composited against 50hPa height over the North Pole.

than those represented in the leading EOF of SLP. They also reflect the very high spatial coherence of stratospheric temperature and its coherence to $\bar{F}_z$ (Figure 2),

Relationship to Ozone

The annual cycle of total ozone is lagged with respect to the annual cycle of polar temperature by 3 months, reflecting the additional time for ozone to be exported poleward. Analogous to polar temperature, its interannual variance is large during winter months leading up to its maximum, which is reached shortly after spring equinox.

Figure 7 compares the record of anomalous March ozone from TOMS (solid), averaged over the northern extratropics, against the corresponding record of $\bar{F}_z$ (dashed), integrated over the preceding months of winter. Like polar temperature, interannual changes of spring ozone track changes of upward EP flux from the troposphere. In years when $\bar{F}_z$ is anomalously strong, March ozone is anomalously large. Just the reverse is observed in years when $\bar{F}_z$ is anomalously weak. It's noteworthy that this relationship holds even in years of unusually low ozone, like 1997.

While being strongly correlated to spring ozone, interannual changes of $\bar{F}_z$ do not account for all of its variance – in contrast to interannual changes of minimum polar temperature observed during early winter (Figure 2). Other mechanisms also contribute to interannual changes of spring ozone. A similar conclusion applies to spring temperature, which tracks the record of $\bar{F}_z$ is similar fashion.

Interannual changes of $\bar{F}_z$ reflect changes in overall wave driving of the residual circulation. The residual circulation is influenced as well by the QBO, which controls where wave activity is absorbed (e.g., Tung and Yang, 1994). By doing so, the QBO also affects mean downwelling over the winter hemisphere and, thus, springtime temperature and ozone. Another source of interannual variance comes from volcanic perturbations, during the years immediately following

El Chichon and Pinatubo. They alter diabatic heating and ozone photochemistry (WMO, 1995), but occupy only a small fraction of the overall record.

Like interannual changes of temperature, interannual changes of ozone are strong at high latitudes. Figure 8 shows the structure of anomalous March ozone, as a function of latitude, which has been composited from its spatial coherence. Anomalous ozone is positive in the extratropics, where it reflects enhanced downwelling of ozone-rich air during winters when $\bar{F}_Z$ is anomalously strong. It approaches 25 DU at high latitudes, where anomalous temperature is also positive and strong. In the tropics, anomalous ozone has opposite sign, reflecting enhanced upwelling of ozone-lean air during the same winters. Compensating changes at high and low latitudes also appear in zonal-mean ozone structure observed by MLS (Fusco and Salby, 1999). Like temperature changes, however, ozone changes in the tropics, where ozone is short lived, are comparatively weak – offset by photochemistry that competes with transport for control of ozone mixing ratio surfaces. Changes in the tropics are nevertheless significant at the 95% level, so they operate coherently with the stronger ozone changes at high latitudes.

Ozone Changes in the Upper Stratosphere

Anomalous total ozone in Figure 8 reflects interannual changes in the LS. Those changes are accompanied by interannual changes in the upper stratosphere. Lidar measurements at OHP, in addition to temperature, also retrieve ozone between 25 and 45km (Godin *et al.*, 1985; Guirlet *et al.*, 2000). Superposed in Figure 7 is the anomalous ozone column abundance above

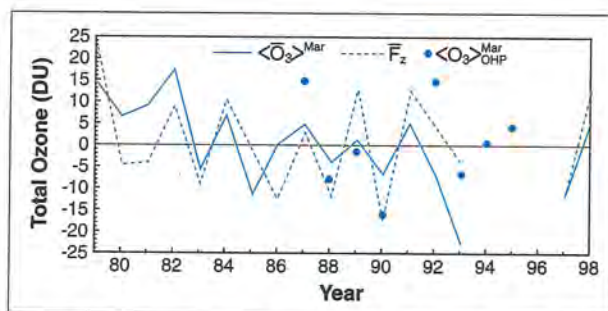


Figure 7: Anomalous March total ozone (10°N - 90°N), as a function of year, compared against the anomalous upward EP flux from the troposphere (55°N - 90°N) during Dec.-Feb. (normalised). Also shown is the anomalous column abundance above 30km during March observed at OHP (normalised).

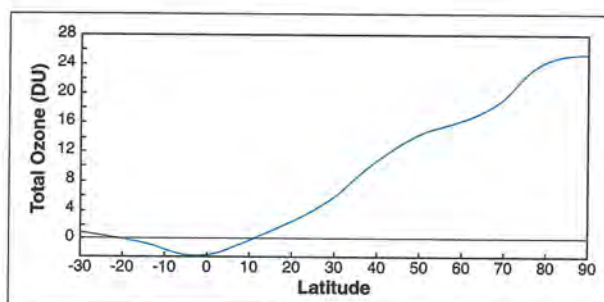


Figure 8: (a) Anomalous total ozone during March, as a function of latitude, composited against total ozone at 50°N .

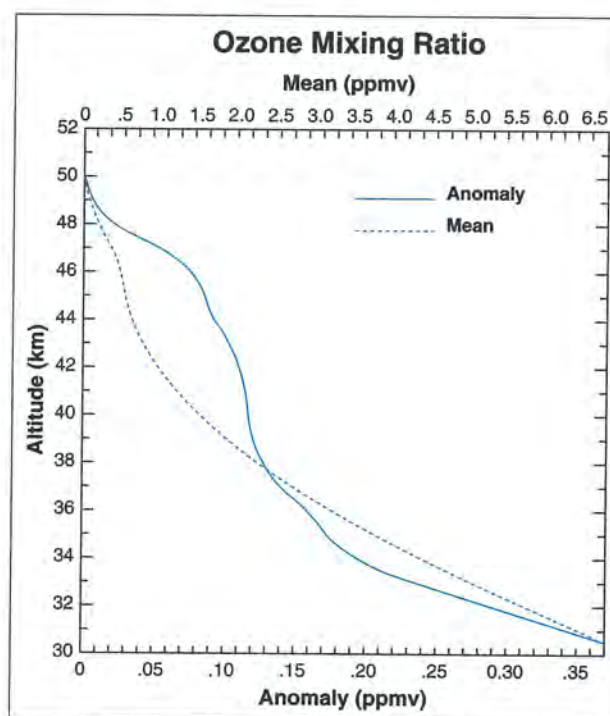


Figure 9: Anomalous ozone mixing ratio at OHP during March (solid), as a function of altitude, composited against ozone changes at 40km. Also shown is the 20-year mean ozone mixing ratio at OHP during March (dashed).

30km observed at OHP (solid circles), after 1986. It misses a couple of years, due to limited sampling. Nevertheless, the available data from the upper stratosphere and mesosphere are strongly correlated to TOMS

ozone fluctuations in the LS. In years when TOMS ozone is anomalously high, so too is OHP ozone. Just the reverse is observed in years when TOMS ozone is anomalously low.

Figure 9 shows the vertical structure of anomalous ozone mixing ratio at OHP (solid). Owing to the limited number of years available, March ozone has been composited by grouping high and low years and then differencing the means of each group. Anomalous mixing ratio is positive at all levels, decreasing upward from a maximum at 30km. Changes in the upper stratosphere thus operate coherently with changes in the LS, characterising ozone changes that are deep. When TOMS ozone (concentrated below 30km) is anomalously large, so too is ozone mixing ratio above 30km. Just the reverse is observed in years when TOMS ozone is anomalously small.

The structure of ozone changes above 30km mirrors the mean structure of ozone mixing ratio during March (dashed). Both peak near 30km and decrease upward in very similar fashion. The resemblance of anomalous ozone to mean ozone implies that much of the interannual change observed at OHP follows from a meridional swing of ozone mixing ratio surfaces: During winters when the residual circulation is strong, enriched mixing ratio surfaces expand poleward, as the vortex recedes. Conversely, during winters when the residual circulation is weak, those mixing ratio surfaces recede equatorward, as the vortex expands.

Discussion

Observed changes of ozone in the LS have structure consistent with interannual changes of the residual circulation. So do contemporaneous changes of temperature. Interannual changes of ozone operating coherently in polar and tropical regions imply changes between neighbouring years (e.g., between

± 1 -2 std deviations of the residual circulation) of 50-100 DU. These are as large as observed changes that have been ascribed to chemical depletion.

Changes of temperature and ozone in the upper stratosphere and mesosphere operate coherently with changes in the LS. They evidence an expansion of ozone-rich air at sub-polar latitudes during winters when the residual circulation is strong, compensated by reversed changes of ozone-lean air inside the polar vortex.

Changes of the residual circulation introduce changes of photochemistry, which can change ozone as well. Changes of meridional transport alter ozone production inside air that drifts poleward during the disturbed season. Such changes also influence chemical destruction mechanisms. Winters when the residual circulation is anomalously weak lead to temperatures over the Arctic that are anomalously cold. They provide conditions more favourable for heterogeneous processes, like those operating in the Antarctic LS. Conversely, winters when the residual circulation is anomalously strong lead to temperatures over the Arctic that are anomalously warm, making such processes unlikely.

Through this mechanism, interannual changes of the residual circulation can regulate ozone depletion over the Arctic. While heteroge-

neous chemistry can augment changes introduced directly by changes of transport, it's unlikely that it plays a central role in observed changes of temperature and ozone. Those interannual changes have structure that is deep, extending upward coherently through the entire stratosphere. They also operate coherently across much of the globe, changes at high latitudes being compensated at low latitudes by changes of opposite sign.

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The lower arctic stratosphere in winter since 1952

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Temperature information on the lower arctic stratosphere is now available for 45 winters, and information on the main state of the stratosphere since 1952 when Scherhag discovered the *Stratospheric Midwinter Warmings*. It is well known that the interannual variability of the stratospheric circulation during the arctic winters is very large, that very cold as well as very warm winters are observed and that it is very difficult to identify temperature trends.

In this short report we want to summarise the observations during the last 49 winters, based mainly on the historical daily analyses of radiosonde observations made at the Freie Universität Berlin (FUB) which is the longest data set available. We are using the monthly mean 30hPa temperatures over the North Pole as a key parameter to describe the general state of the Arctic since the winter of 1955/56, Table 1. Here, **RJ** is the monthly mean of the sunspot num-

bers in January, in the column marked **QBO** the phase of the Quasi-Biennial Oscillation is indicated (determined using the winds between 50 and 40hPa in January-February), and **FW** gives an indication of the timing of the Final Warmings which are the transitions from the winter to the summer circulation. **CW** stands for Canadian Warmings and * indicates the occurrence of a *Major Midwinter Warming*. **C** stands for a cold monthly mean (about half a standard deviation

below the average) and the definition for this is based on the 30-year monthly means, also given in the table. The values of the linear trend (K/decade) for the whole period and the confidence level in % are given at the bottom of Table 1.

Early Winter: The clearest change in temperatures and circulation took place in November. Starting with the winter 1981/82, almost all temperatures in the Table are marked with a C which is based on the 30-year mean, but the average of the last 15 years is with -71.5°C almost 3 degrees colder than the 30-year mean, and the C must be newly defined. At the same time, the *Canadian Warmings* which in November/December took place in 18 of the 31 years until 1981/82, are missing during the last 18 winters, except for 2 cases. This hints to a change in the meteorological conditions of the troposphere, as *Canadian Warmings* are connected with the intensification of the stratospheric Aleutian high (see insert p.14) which is again connected with the Aleutian low in the troposphere. The last 15 Decembers have been sometimes very cold, but also 2 *Major Mid-winter Warmings* took place, a feature not observed before, and in total for 45 winters the monthly mean average for December remains the same as for the 30 years before.

Mid-winter: In the 34 years from 1952 to 1985, 6 *Major Warmings* took place in January and 8 in February; there were no major warmings in December. During the last 15 years, we observed only 1 *Major Warming* in January - but 2 in December (!), and 3 in February. It is tempting to speculate that there were significantly fewer *Major warmings* in mid-winter during the last 15 winters, but if one counts these events for 3 winter months, the result is not convincing. Really striking is the fact that there was a period of 7 winters without *Major Warmings*.

Factors influencing the state of the stratospheric circulation are the Southern Oscillation (SO) and the North Atlantic Oscillation (NAO), not dealt with here, and the QBO and the 11-year solar cycle (Labitzke, 1987; van Loon and Labitzke, 2000). The QBO determines the character of the early winter, leading to a colder and more stable polar vortex in December and January during the west phase of the

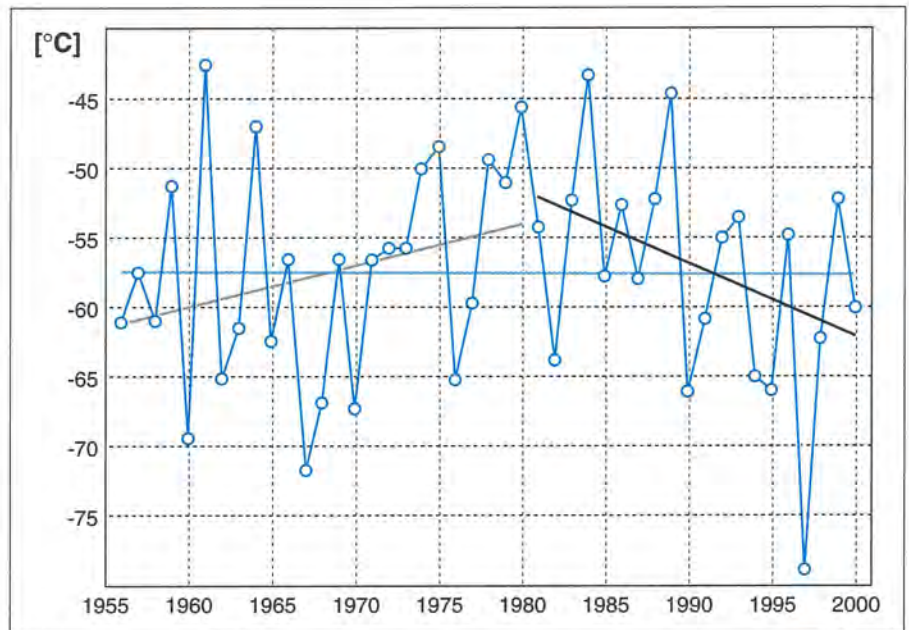


Figure 1: Time series of the 30hPa monthly mean temperatures at the North Pole in March, 1956-2000. Linear trends have been computed for three different periods. (Labitzke and van Loon 1999, updated).

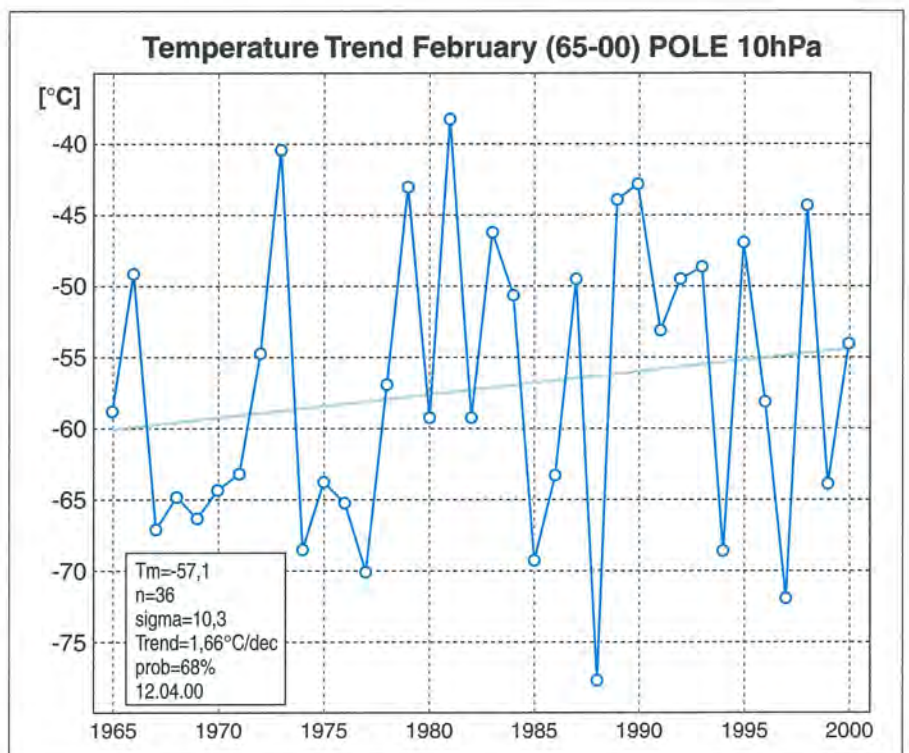


Figure 2: Time series of the 10hPa monthly mean temperatures at the North Pole in February, 1965-2000. The linear trend is $+1.66^{\circ}\text{C/dec}$ with a probability of 68%.

QBO and a more disturbed and warmer Arctic during the east phase of the QBO. The solar cycle influences the latter part of the winters when a clear difference is observed between periods of high and low solar activity. During high solar activity the winters in the west phase of the QBO tend to be disturbed and are often connected with *Major Midwinter Warmings*.

If one compares the last 15 winters with the earlier years since 1958, one cannot find a convincing sign of mid-winter temperature change; but, although January 2000 was the coldest of them all, one may identify a positive temperature trend, if one starts, e.g., in 1979 ($+0.7$; 20%). One feature is, however, worth mentioning: the late winters from 1993 to 1996, i.e. 4 winters belonged to the

MONTHLY MEAN 30hPa-NORTHPOLE TEMPERATURES (°C)

YEAR	RJ	Nov	Dec	Jan	QBO	Feb	Mar	Apr	FW
1951/52	41	CW			east	*			late
1952/53	27	CW			west				late
1953/54	0				west		*FW		early
1954/55	23		CW	*	east				late
1955/56	74	-68	-7 -79C	-76C	west	-71C	-61C	-45	late
1956/57	165	-68	-73	-74	east	-38*	-58	-53C	late
1957/58	203	-70C	-79C	-71	west	-49*	-61C	-57C	late
1958/59	217	-58CW	-67CW	-74	east	-73C	-51*FW	-43	early
1959/60	146	-71C	-69CW	-60	west	-70C	-69C	-41	early
1960/61	58	-64	-65CW	-65	east	-72C	-42*FW	-47	early
1961/62	39	-67	-80C	-79C	west	-67	-65C	-57C	late
1962/63	20	-69CW	-74	-74	east	-52*	-61C	-54C	late
1963/64	15	-72C	-81C	-78C	west	-77C	-47*FW	-44	early
1964/65	18	-70C	-77C	-76C	west	-73C	-62C	-45	late
1965/66	28	-68	-59CW	-76C	east	-60*	-56	-48	late
1966/67	111	-65	-73CW	-80C	west	-78C	-72C	-48	late
1967/68	122	-71C	-75	-58*	west	-68	-67C	-52C	late
1968/69	104	-63CW	-62	-72	east	-74C	-57	-47	late
1969/70	112	-71C	-73	-49*	west	-62	-67C	-47	late
1970/71	91	-71C	-78C	-54*	east	-66	-56	-51C	late
1971/72	62	-72C	-78C	-79C	west	-70C	-56	-44	early
1972/73	43	-69CW	-79C	-73	east	-44*	-56	-52C	early
1973/74	28	-71C	-78C	-78C	west	-79C	-50*FW	-44	early
1974/75	19	-65CW	-74	-65	east	-68	-48*FW	-46	early
1975/76	8	-71C	-77C	-80C	west	-78C	-65C	-34*FW	early
1976/77	16	-63CW	-68	-60*	east	-69	-60C	-40	early
1977/78	52	-68CW	-77C	-74	e / w	-66	-49	-44	early
1978/79	167	-74C	-74CW	-75C	west	-60*FW	-51	-49	early
1979/80	162	-64CW	-72	-80C	east	-70C	-46*FW	-45	early
1980/81	114	-68CW	-82C	-81C	west	-56*	-54	-54C	late
1981/82	111	-71C	-70CW	-71	east	-69	-64C	-38	early
1982/83	86	-74C	-79C	-79C	west	-62	-52	-46	late
1983/84	57	-70C	-77C	-80C	w / e	-65	-43*FW	-50	early
1984/85	17	-71C	-73	-53*	east	-66	-57	-46	late
[T] n=30		-68.6	-74.1	-71.5		-65.7	-56.8	-47.0	
sigma		3.6	5.6	9.1		9.9	7.8	5.3	
C		≤ -70	≤ -77	≤ -75		≤ -70	≤ -60	≤ -51	
1985/86	3	-73C	-81C	-76C	west	-74C	-53*FW	-41	early
1986/87	10	-76C	-78C	-60*	east	-49	-58	-53C	late
1987/88	59	-66	-56*	-77C	west	-80C	-52*FW	-44	early
1988/89	161	-71C	-77C	-82C	west	-57*FW	-45	-52C	early
1989/90	177	-71C	-79C	-80C	east	-63	-66C	-52C	late
1990/91	137	-74C	-76	-70	west	-57*	-61C	-45	early
1991/92	150	-72C	-78C	-66	east	-59	-55	-44	early
1992/93	59	-70C	-77C	-80C	west	-68	-54	-49	late
1993/94	58	-70C	-70CW	-66	west	-77C	-65C	-38	early
1994/95	24	-71C	-81C	-70	e / w	-57	-66C	-49	late
1995/96	12	-71C	-79C	-81C	west	-75C	-55	-48	late
1996/97	7	-66CW	-69	-79C	east	-83C	-79C	-56C	late
1997/98	32	-73C	-66	-67	west	-64	-62C	-48	early
1998/99	62	-72C	-63*	-67	east	-69*	-51	-55C	late
1999/00	90	-75C	-81C	-84C	west	-74C	-60C	-46C	late
Trend	K/dec	-1.2	-0.2	-0.7		-1.0	-0.03	+0.1	
conf.	%	99	20	54		60	3	12	

group of the QBO westerlies. This did not happen before when never more than 2 consecutive winters fell into the same group. In addition, during these winters the solar activity was low, and this combination agrees with the fact that we observed cold winters, all through February. During this period of the winter the interannual variability is, as discussed above, extremely large and therefore no significant trends can be determined, see Table 1, bottom line.

Spring: March is an important time of the winter, as a long lasting undisturbed cold vortex is a meteorological situation in which ozone can be destroyed, while earlier breakups of the vortex are connected with ozone transport into the Arctic. The spring of 1997 was the coldest within our series of 45 winters. One should not imply a clear trend here, as the overall trend in March in the Arctic is almost zero, Figure 1. This Figure may serve as a warning: if one uses the complete series since 1956 to calculate a linear trend, the trend is zero. If one uses the first 25 years the trend is positive and statistically significant, whereas it is negative and, again, statistically significant from 1981 to 1997. In other words, the trend depends on how one cuts the cake (Labitzke and van Loon, 1999). If one combines the temperature changes of March and April during the last 15 years, one ends up with no or a slightly positive trend for this period, although only 2 *major Final Warmings* took place during these years in contrast to 9 during the earlier 34 years. The timing of the *Final Warmings* during the last 15 years was evenly distributed. During the earlier years it happened that in 7 cases in a row the transition was late (in 1965-1971), followed by 9 years when it was early (in 1972-1980). The reason for this must be sought in the troposphere.

The temperature trend described above for the 30hPa level is similar for the lower stratosphere, e.g. the 50hPa level. But if one goes about 7km higher, to the 10hPa level where the FUB-data are available since 1965, the trend appears to change, and February is shown as an example, Figure 2. This agrees with positive temperature trends during winter reported from rocket-

Classification of Stratospheric Warmings

Major Midwinter Warmings occur mostly in January-February. In addition to warming of the north polar region and reversal of the meridional temperature gradient, they are also associated with a breakdown of the polar vortex, which is replaced by a high. That is, the definition of a *Major Midwinter Warming* requires not only the warming but also a total change of circulation. The definition of a breakdown of the polar vortex is that the usual westerlies in the Arctic at 10hPa are replaced by easterlies so that the centre of the vortex moves south of 60-65°N. The vortex is either displaced entirely or split into two. This type of warming has not been observed in the Antarctic.

Minor Warmings can indeed be intense and sometimes also reverse the temperature gradient, but they do not result in a reversal of the circulation at the 10hPa level. They are found in the Antarctic as well.

Canadian Warmings often happen in early winter. They take place when the Aleutian stratospheric high intensifies and moves poleward. The *Canadian warmings* can reverse the meridional temperature gradient and sometimes briefly change the zonal wind direction over the polar cap, but nevertheless they do not lead to a breakdown of the cyclonic polar vortex.

Final Warmings mark the transition from the cold cyclonic vortex in winter to the warm high centred on the pole in summer. Their intensity varies much and they can be divided into major and minor *Final Warmings*. The time when the *Final Warmings* take place - when the westerlies of winter are replaced by the easterlies of summer - also varies a good deal, so they are further divided into early and late *Final Warmings*. Naturally, there are also *Final Warmings* in the Southern Hemisphere.

sonde data (Chanin and Ramaswamy, 1999) and with model results which indicate only weak cooling in the middle stratosphere above a cooling in the lower stratosphere in connection with the observed ozone decrease (Langematz, 2000). But caution is recommended, as the variability is, as stressed before, very large and any trend can be derived, depending on the beginning and the end of the time series.

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Long and short term variability in the dynamical attributes of the Arctic polar vortex and its effect on ozone

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The meteorology of the Arctic polar vortex has been recognised as an important influence on the severity of ozone depletion in the Arctic region. The area (or volume) which is at temperatures below that of the formation of the Polar Stratospheric Clouds (PSC) and the duration of the period of the PSC formation control the amount of the active chlorine available to destroy ozone. Pawson and Naujokat [1999] showed the effect of decreasing polar temperature during the 1990s on active chlorine concentrations. The relationship between the temperature in the stratosphere and the quasi-biennial oscillation (QBO) and solar activity has been studied previously [e.g. Labitzke, 1987]. In this short report, trends and variability in intensity, duration, radius and stability of the Arctic polar vortex are reported [Nakane *et al.*, 2000a]. In addition, we briefly report a preliminary estimation of the effect of the chlorine loading on the ozone depletion in the vortex based on ozone sonde observations over Japan under similar dynamical conditions but different chlorine loadings [Nakane *et al.*, 2000b].

Long and short term variability in Arctic polar vortex characteristics

Maps of Ertel's potential vorticity (PV) distributions on isentropic surfaces are known to be useful for viewing the dynamical processes related to the polar vortex because PV behaves as a quasi-conserved tracer. PV can be calculated using temperature and wind fields from 3D global analysis data from ECMWF, NCEP/NCAR, etc. The edge of the polar vortex, the barrier to meridional transport, can be defined as the region where the gradient in PV is large. To quantify the intensity of this barrier, the first derivative of PV with respect to equivalent latitude ϕ_e is calculated, $dPV/d\phi_e$ [Nash *et al.*, 1996]; the equivalent latitude

is defined as the geometrical latitude enclosing the same area as the PV isoline. To eliminate the dependence on potential temperature and ϕ_e , the normalised gradient of PV is defined as $NGPV=(dPV/d\phi_e)/PV$.

The NGPV as a function of equivalent latitude and time for the 39 winters from 1959 to 1997 in the Northern Hemisphere is shown in Figure 1. Here, "winter" refers to winter/spring and "1959" refers to 1958/1959. In each winter, the vortex onset, growth, and shrinking due to stratospheric warmings and dissipation can be seen in the maps of NGPV. Large NGPV corresponds to a strong vortex. The location of the meridional maximum in NGPV, ϕ_e^{max} , defines the edge of the polar vortex. Therefore, $(90 - \phi_e^{max})$ is the radius of the polar vortex in units of degrees. Strong vortices

with large NGPV were formed in 1976, 1989, 1990, 1993, 1995, 1996 and 1997. Stable vortices, characterised by smaller fluctuations in radius of the vortex, were in 1967, 1976, 1982 and 1997. The strong stratospheric warming in February 1963 is clearly identified by the sudden shrinking of the vortex. In most cases, polar vortices persisted after disappearance of PSCs, and low ozone concentrations continued as long as the vortices were stable.

In Figure 1, positive trends in the strength and stability of the polar vortex can be qualitatively seen. A quantitative analysis of the vortex characteristics [Nakane *et al.*, 2000a] concluded that:

1. There were positive trends in the strength, duration, radius and stability of the vortex.

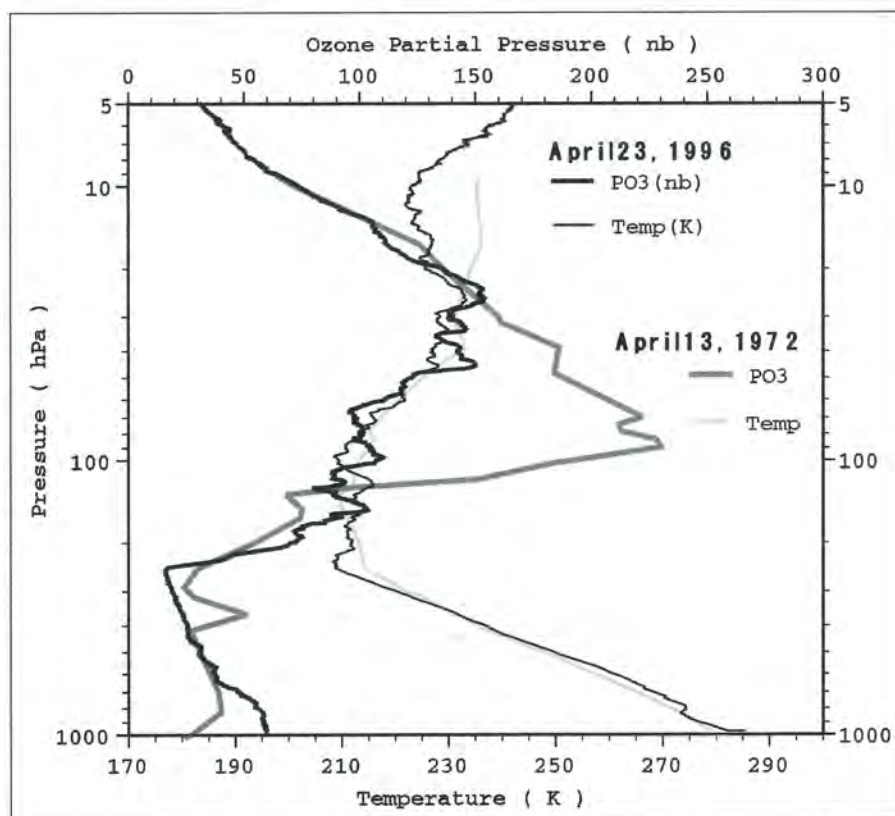


Figure 3: Vertical profiles of ozone and temperature observed by ozone sondes at Moshiri (44°N, 142°E) by NIES and STEL of the Nagoya University on April 23, 1996 (black) and by Japan Meteorological Agency (JMA) at Sapporo (43°N, 121°E) on April, 13, 1972 (gray). The parts of the polar vortex covered the measurement sites in the middle and lower stratosphere in both cases.

2. Short-term variability in the vortex indices was correlated with the QBO.

3. A coupling of the 11-year solar cycle and the QBO generated long-term variability. Positive correlations were found between all four characteristics of the vortex (especially strong correlation in the case of vortex strength) and the QBO during solar inactive phases; during solar active phases, the correlations were weak or weakly negative.

A case study of the relative importance of chlorine loading and the dynamical condition of the Arctic polar vortex on ozone depletion

Vertical profiles of ozone have been measured at Moshiri (44°N, 142°E) in Hokkaido, Japan since 1996. Episodic low ozone events were found on April 14 and 23, 1996 when the station was inside the decaying Arctic polar vortex. Significant chemical ozone loss was found in the Arctic vortex in 1996 by the MATCH technique (Rex *et al.*, 1997). The ozone profile obtained at Moshiri on April 23, 1996 was compared with the profile measured at Sapporo on April 13, 1972 when Moshiri and Sapporo (43°N, 121°E) were also thought to be inside the Arctic polar vortex (Figure 2). The dynamical conditions of the vortex in 1972 were similar to those of 1996 except that the vortex was a little weaker in 1972 (Figure 1) and the temperature inside the vortex was higher in 1972. The ozone profiles at Sapporo in 1972 and at Moshiri in 1996 were similar except between 150hPa and 20hPa; at around 70hPa, the ozone mixing ratio was 55% smaller in the 1996 Moshiri case compared to the 1972 Sapporo case (Figure 3).

To understand the effects of the differences in chlorine loading and in the dynamical features of the Arctic polar vortices on chemical ozone loss, preliminary photochemical multi-trajectory-box model simulations inside the vortex were carried out for both cases. The chemical box-model includes gas phase reactions, heterogeneous reactions and diabatic descent in

the polar vortices. Thirty-six trajectories were placed inside the vortex and were run from December 1, 1995 to April 23, 1996 using the zonal, meridional and vertical winds from the ECMWF data. The diabatic descent rate was calculated by averaging the potential temperature levels of all trajectories, and its uncertainty was given by the standard deviation. The potential temperature of the air mass descended from 625K on 1 December, 1995 to 460K on 23 April, 1996. The same descent rate was assumed for the 1972 case because the behaviour of the polar vortex in 1972 was similar to that in 1996 (see Figure 1); the uncertainty in the descent rate seems to be larger than its variability. First, backward trajectories from Moshiri on April 23, 1996 and from Sapporo on April 13, 1972 were run and it was confirmed that both series of trajectories came from inside the polar vortices. Then, the forward trajectories were run, with the photochemical model, from December 1, 1971 to April 13, 1972 and from December 1, 1995 to April 23, 1996, with the descent rate estimated above. A natural chlorine loading of 0.8ppbv and a total chlorine loading of 1.34ppbv for 1972 and 3.35ppbv for 1996 were obtained from the Equivalent Effective Chlorine Loading (EEESC) estimated in "Scientific Assessment of Ozone Depletion:1998" (Madronich and Velders, 1998) which represent the stratospheric chlorine loading below 25km in middle and high latitudes.

The model estimated ozone mixing ratio was 1.6ppmv on 23 April, 1996 while observations showed 1.4ppmv. For 1972, the calculated ozone mixing ratio was 3.1ppmv and the observed ratio was 3.0ppmv on 13 April. Thus, the agreement between the model and observation is good.

The calculated amounts of ozone depletion in the decaying Arctic polar vortex when it covered Sapporo and Moshiri are summarized

in Table 1. It is concluded that the increase in chlorine loading is primarily responsible for the increase in ozone loss in the Arctic polar vortex from 1972 to 1996. Ozone loss would have changed from 2.0 to 0.7ppmv if the chlorine loading had been 1.34ppbv in the 1996 vortex. The results also suggest that ozone depletion should be considerably enhanced by strong and cold Arctic polar vortices under high chlorine loading. Such a scenario is quite likely since there are positive trends in vortex intensity, stability, duration and size as seen in Figure 1, and the chlorine loading is predicted to decrease slowly.

Acknowledgements

This work was carried out as a part of the project in the framework of the Global Environment Research Fund financed by the Environment Agency of Japan. The study on the Arctic polar vortices was done with M. Ninomiya, GEF and G. Bodeker, NIWA. The ozone sonde observations were carried out in co-operation with STEL of Nagoya University. The trajectory box model was developed in NIES by A. Lukyanov, CAO.

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Table 1. Calculated ozone loss (ppmv) from December 1, 1971 to April 13, 1972 and from December 1, 1995 to April 23, 1996

EEESC (ppbv)	0.8 (natural abundance)	1.34 (1972)	3.35 (1996)
1972 Vortex	0.35	0.5	1.3
1996 vortex	0.35	0.7	2.0

Bold values are the actual cases.

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The Transition from SSU to AMSU Data in CPC Stratospheric Analyses

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Introduction

The daily global stratospheric analyses of the Climate Prediction Center, NCEP (formerly NMC) have used temperature data from NOAA operational polar orbiting satellites. Stratospheric Sounding Unit (SSU) data since 1979 have been the major source of upper stratospheric temperature information. This will change because the last available SSU instrument was launched on NOAA-14 in 1996. The NOAA-14 SSU instrument continues to function. Meanwhile, as replacement for SSU instruments, the first Advanced Microwave Sounding Unit (AMSU) instrument on the NOAA-15 polar orbiting environmental satellite has been providing information since June 1998.

For at least the next 10 years, AMSU instruments are scheduled to operate on all NOAA polar satellites. AMSU instruments provide radiometric information from 6 stratospheric channels, compared to the 3 SSU channels, so we may expect a positive impact on stratospheric temperature retrievals. The purpose of this note is to highlight effects of this change for users of CPC stratospheric analyses and for users of NOAA polar orbiting satellite temperature information.

Weighting functions

Figure 1 presents the stratospheric weighting functions of the AMSU and SSU instruments. We have simulated, by multiple regression, radiances for the 3 SSU channels

from the 6 AMSU channel radiances, using a climatological collection of 1200 atmospheric temperature profiles. The SSU radiances are simulated with an RMS errors of only 0.3K. Thus, for purposes of climatological continuity, we should be able to provide, from the SSU to AMSU data, a consistent, continuous record for determinations of long-term change.

Nash and Brownscombe (1983) demonstrated that diurnal and semi-diurnal temperature variations are a source of differences in the temperatures derived from successive satellites. Temperature differences arise because of differences in the local time of observation between morning and afternoon satellites and also because afternoon NOAA satellites have precessed in their orbits. This will be rectified with the launch (expected in the year 2000) of NOAA-L, which will have a non-precessing orbit. Time of day factors affect comparisons involving CPC stratospheric analyses because the CPC analyses (Finger *et al.*, 1993) have used both ascending and descending NOAA data. Furthermore, CPC-AMSU comparisons are affected because the equatorial crossing times of NOAA-14 and NOAA-15 are about 6 hours apart.

CPC comparisons with haloe

As an independent source of temperature information we use data from the Halogen Occultation Experiment (HALOE) from the NASA Upper Atmosphere Research Satellite (Russell *et al.*, 1993;

Hervig *et al.*, 1996) for the period 1998 through early 1999.

Figure 2 shows the mean and RMS statistics for HALOE minus CPC temperature comparisons, as a function of pressure. Here we compare zonal average HALOE data versus the zonal average CPC data (based on SSU observations) for each day of HALOE availability, with differences averaged by month. Thus each latitude has a varying number of comparisons. Mean differences are less than 1K from 10 to 4hPa, increasing to as much as -4K at 2hPa. The RMS differences are about 1K at lower levels, increasing to 4K at the upper levels. We attribute the difference between the sunrise versus sunset results of HALOE minus CPC to atmospheric diurnal and semi-diurnal tidal effects.

In Figure 3 the above statistical differences at 2hPa are presented as a function of latitude. Overall, HALOE is colder than the CPC results by about 3K, as also seen in Figure 2. We note that there is a tendency for differences to be larger at the equator and at higher latitudes than at mid-latitudes.

AMSU comparisons with haloe

For comparisons of AMSU with HALOE data, time differences are very small. The NOAA-15 satellite has equatorial crossing times of about 7:30 am and 7:30 pm. Thus, the AMSU data are very close in time to the sunrise and sunset HALOE observations. Therefore, the impact of atmospheric

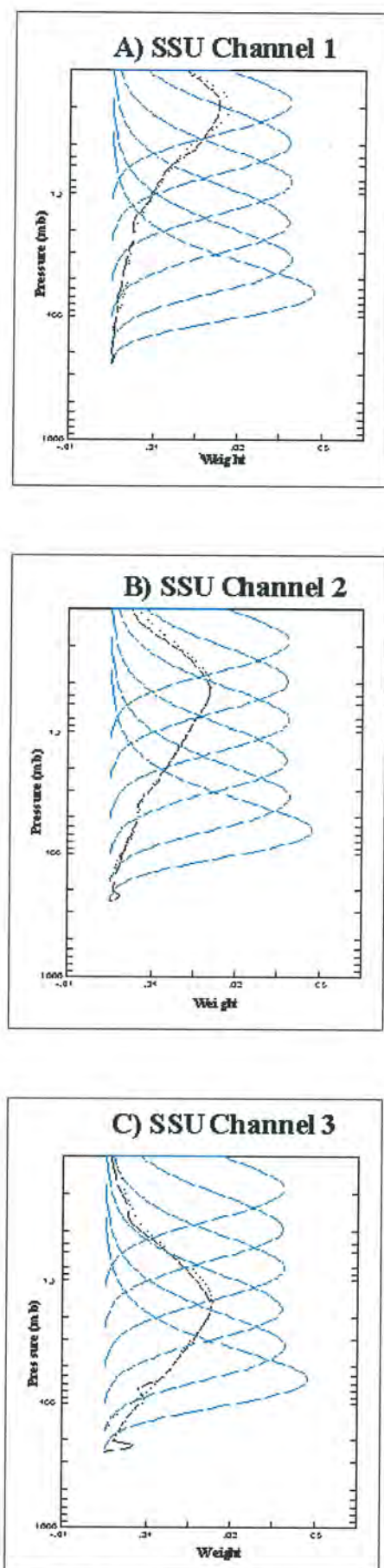


Figure 1: SSU channel weighting functions (solid black lines) and reconstructed SSU channel weighting functions (dashed black lines) using AMSU weighting functions $\times$ (blue lines).

diurnal and semi-diurnal variation is minimised. Figure 4 shows the HALOE-AMSU bias and RMS statistics for the period July 1998-January 1999. In contrast to the HALOE minus CPC statistics, which are based on zonal averages, the HALOE minus AMSU statistics are based on individual match-ups within 500 km. We see that HALOE-AMSU average differences are less than 1K, and RMS differences are less than 3K up to 2hPa, increasing to about 5K at 1hPa. The average differences are much smaller than the HALOE-CPC comparisons of Figure 2. Note that the zonal averaging process smoothes some of the variability of Figure 2. So, for comparison with Figure 2 the HALOE-AMSU RMS values in Figure 4 should be divided by about 4.

Figure 5 presents the 2hPa HALOE-AMSU differences as a function of latitude. The average bias for the 2hPa level is 0.1K, with a standard deviation of 2.8K. This standard deviation, however, includes the point-to-point variability that is averaged out in the zonal means of Figure 3. Taking this into account reduces the standard deviation to about 1K.

Finally, Figure 6 shows time series at four stratospheric levels of zonal mean 85-90°N temperatures from CPC and from AMSU. Largest differences are shown at 2hPa, with AMSU temperatures generally lower than CPC by as much as 10K in wintertime. Previous comparisons with other UARS instruments have indicated that the CPC winter polar temperatures are biased warm. This provides some reason to lend additional credence to the AMSU temperatures.

Final remarks

We have shown that we expect significant improvement in the CPC stratospheric temperature analyses when we transition to the NESDIS temperature retrievals derived from the Advanced Microwave Sounding Unit. We expect that switch to AMSU data to occur in the CPC stratospheric analyses in early 2001. However, when the change occurs, we must determine the optimum way to transition from CPC use of the SSU data to the CPC use of the AMSU data, to ensure a consistent data set for climatological use.

Atmospheric diurnal and semi-diurnal temperature variations contribute significantly to the observed differences between satellite observations and analyses that use those satellite data. For this reason we plan to introduce two daily analyses in addition to the current 1200 UT CPC stratospheric temperature analysis which uses 12 hours of ascending and descending satellite data). The new analyses will be based on separate morning or afternoon (24 hours of ascending or descending orbital) observations. Separate morning and afternoon analyses will clarify the time-of-day factor for users of CPC stratospheric analyses, and allow better temperature comparisons with sunrise and sunset occultation experiments and with data from NOAA SBUV/2 ozone monitoring instruments.

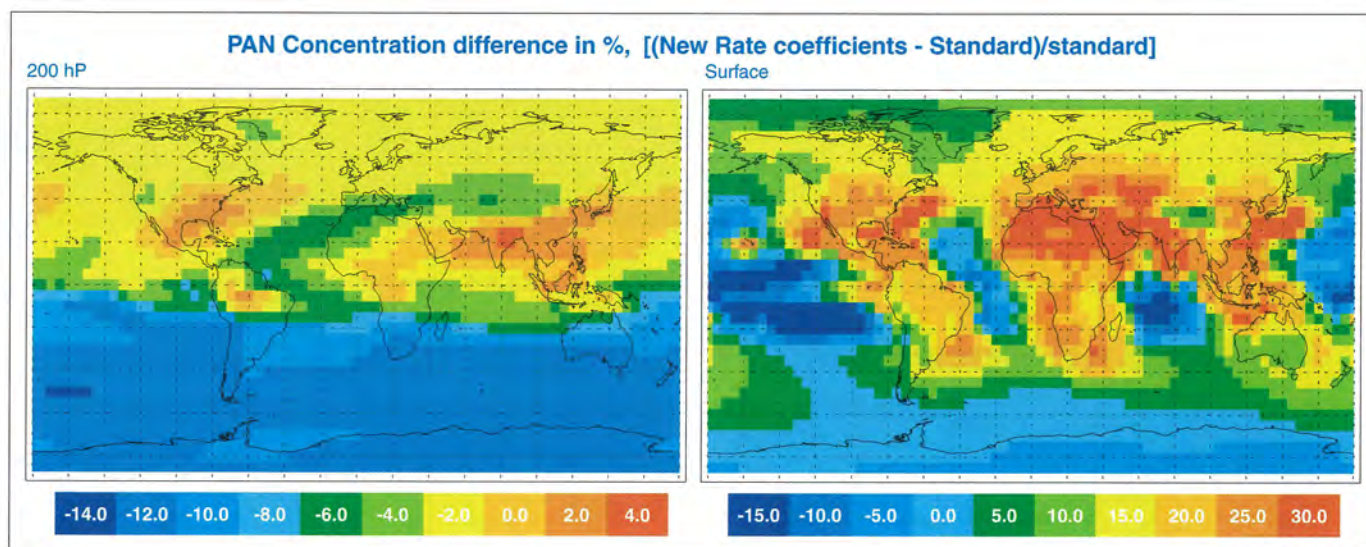
See CPC stratospheric web site: <http://www.cpc.noaa.gov>.

AMSU information <http://orbit-net.nesdis.noaa.gov/crad/st/amsuclimate/amsu.html>.

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Small Organic Peroxy Radicals: Big Players in Ozone Production



▲ Figure 3

The results of the IMAGES model showing the percentage change in calculated abundance of peroxy acetyl nitrate (PAN) when the rate constants recommended here are used in place of the currently used values. The changes at two altitudes, surface and 200 hPa, are shown. PAN is crucial for the transport of NO_x from one region to another and couples the ozone production in one region to another via transport.



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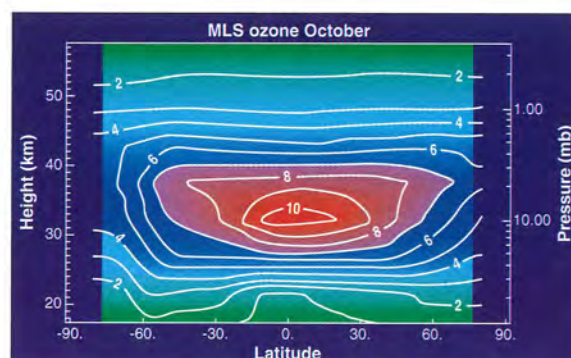
► Figure2

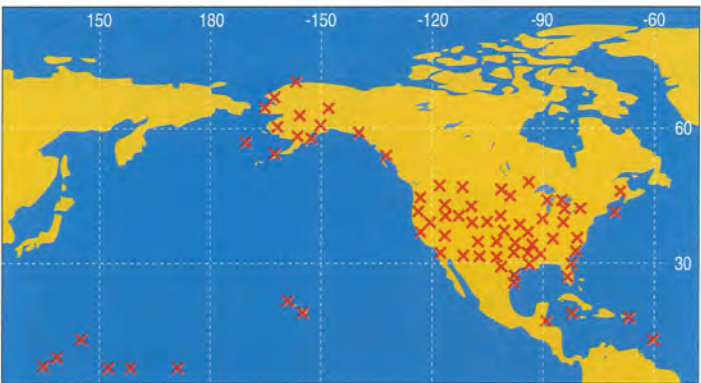
Example data for the SPARC Reference Climatology Project: Zonal mean ozone mixing ratio derived from UARS MLS data. Contour interval is 1ppmv. (Data source: W.J. Randel; Graph: P.M. Udelhofen SPARC-DC).

The SPARC Data Center

◀ Figure 1

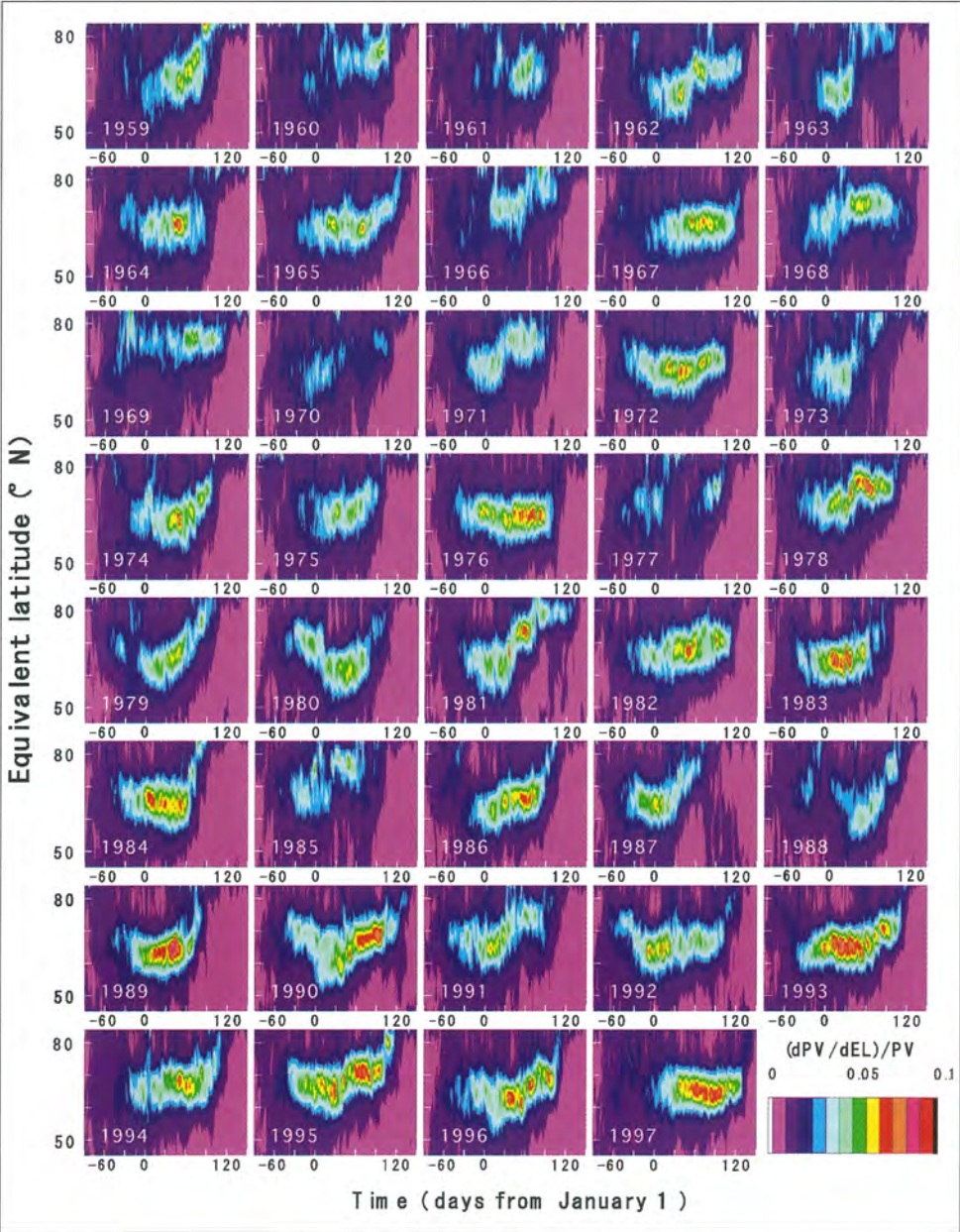
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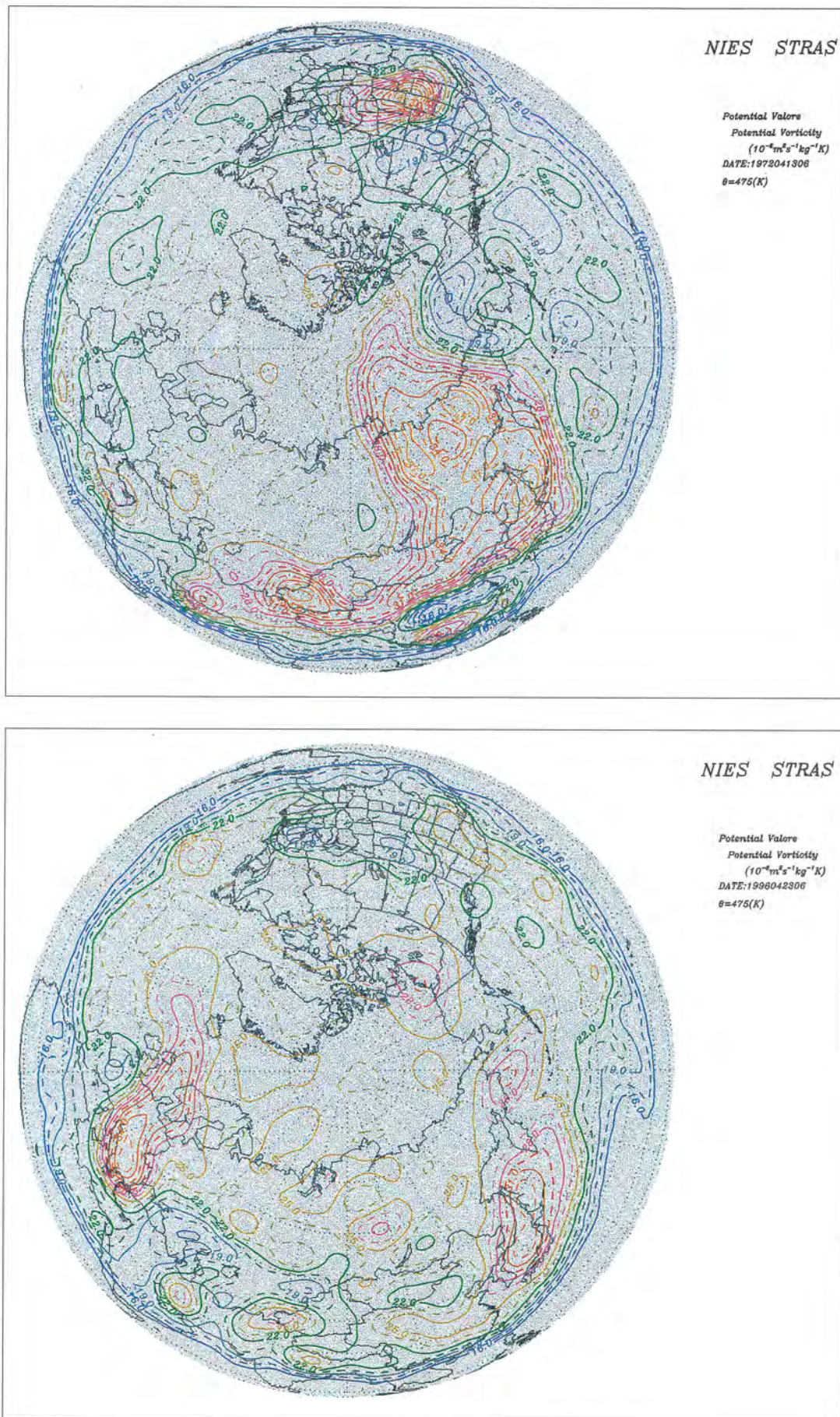


► **Figure 3**
Location of US high-resolution radio sounding stations.

Long and short term variability in the dynamical attributes of the Arctic polar vortex and its effect on ozone



► **Figure 1**
The normalised gradient of potential vorticity (NGPV) at 475K as a function of equivalent latitude and time for the 39 winters from 1959 to 1997 from 45°N to 85°N calculated using the NCAR/NCEP re-analysis data.

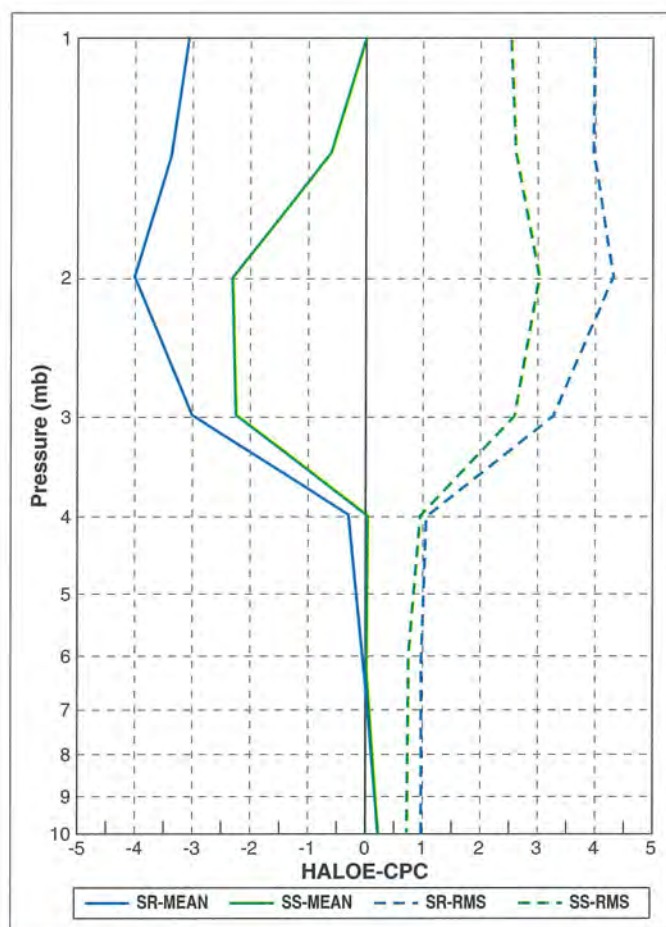


▲ Figure 2

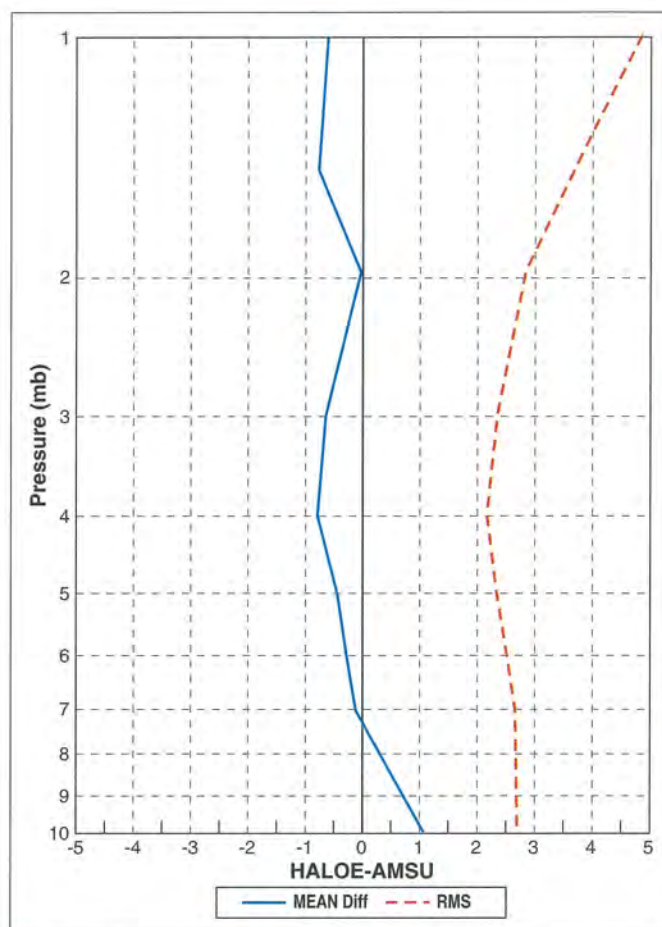
The PV maps ($q=475\text{K}$) on April 13, 1972 (top) and on April 23, 1996 (bottom) calculated using the NCEP/NCAR reanalysis data. In 1972, Sapporo was covered by a part of the polar vortex, and in 1996 Moshiri was also covered by a part of the polar vortex just broken.

The Transition from SSU to AMSU Data in CPC Stratospheric Analyses

IV

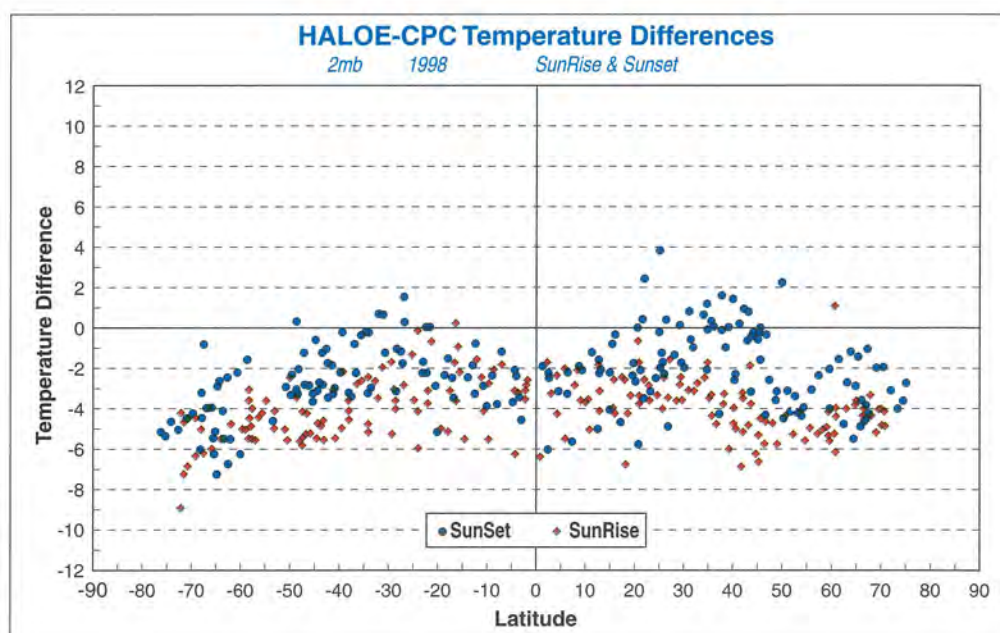


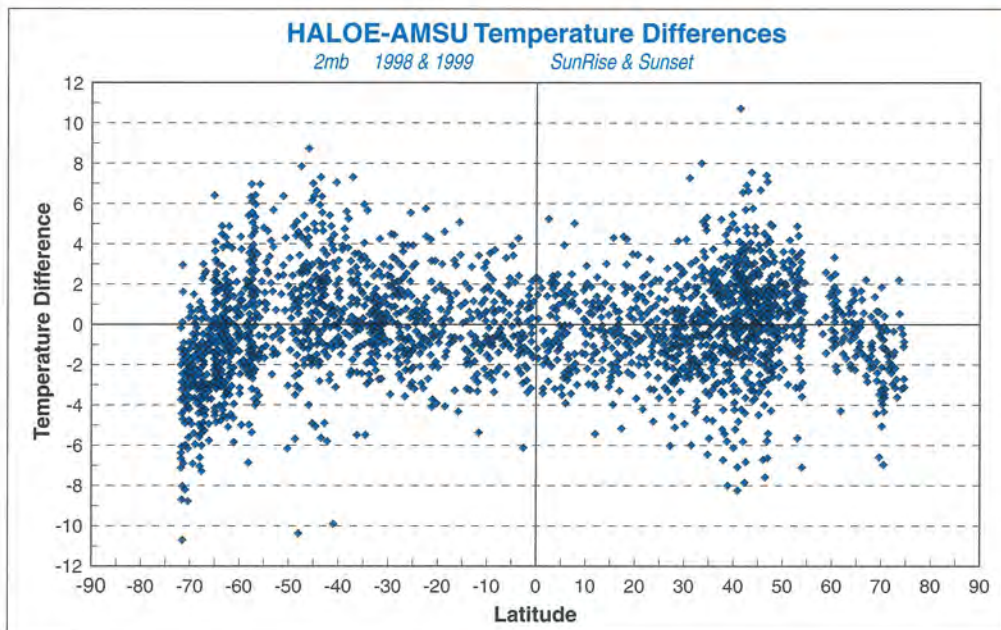
▲ **Figure 2**
Zonal mean HALOE minus CPC mean and RMS temperature differences, for the calendar year 1998. The differences are shown for HALOE sunrise (SR) and sunset (SS). The latitude range corresponds to that shown in figure 3.



▲ **Figure 4**
HALOE minus AMSU temperature differences, mean and RMS, for July 1998 to January 1999. The latitude range corresponds to that shown in figure 5.

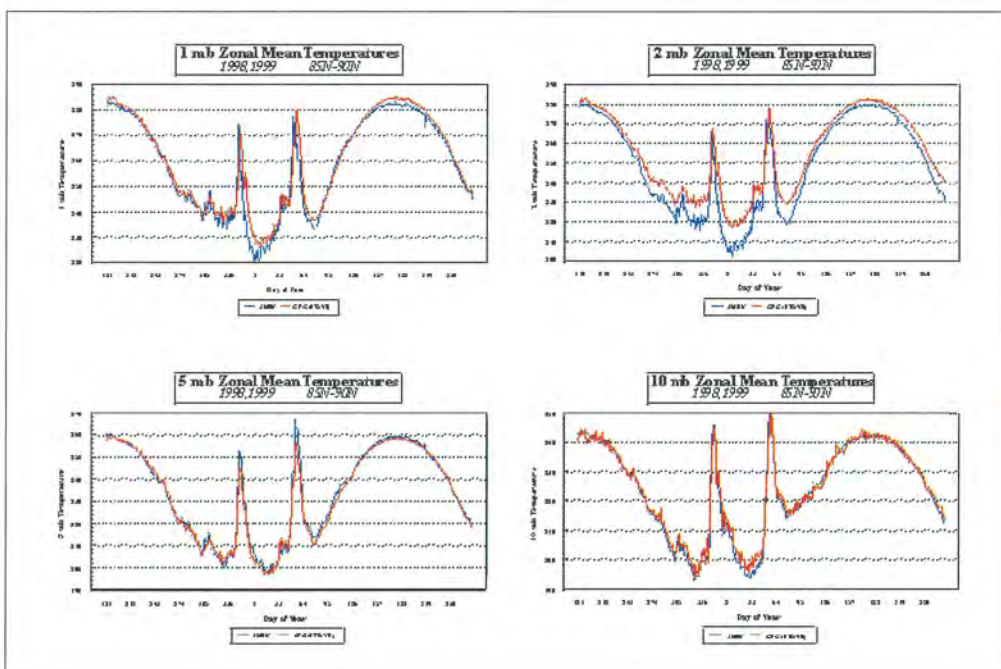
► **Figure 3**
Daily HALOE minus CPC zonal mean temperature differences at 2hPa, for the year 1998. The differences are shown for HALOE sunrise and sunset.





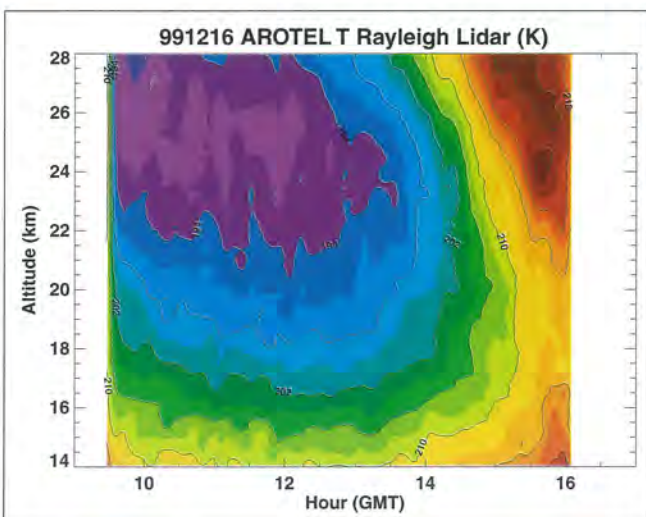
◀ Figure 5

HALOE minus AMSU temperature differences at 2hPa; data from July 1998 to January 1999. The larger scatter than in Figure 2 is due to use of individual observations, rather than zonal means.



◀ Figure 6

AMSU and CPC zonal mean 85-90°N temperatures for July 1998 to January 1999, at four stratospheric levels (1, 2, 5, and 10hPa). Note largest differences at 2hPa during winter. Colder AMSU temperatures have better agreement with HALOE data.



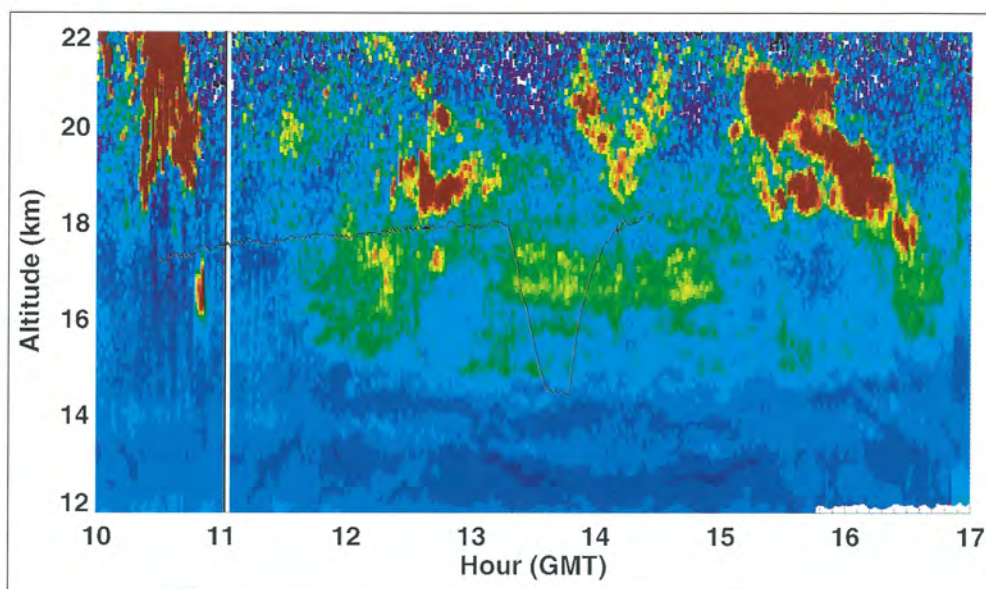
Report on the SAGE III Ozone Loss and Validation Experiment, SOLVE

◀ Figure 2

Temperatures observed by the joint NASA/GSFC and NASA/LRC Airborne Raman Ozone and Temperature Lidar (AROTEL) aboard the NASA DC-8 on the DC-8 flight of December 16, 1999 (PIs: Drs. John Burris, Thomas McGee, and Chris Hostetler).

► **Figure 3**

Aerosol backscatter (1064nm) as measured by the NASA Langley UV Differential Absorption Lidar (DIAL) system on January 20, 2000 (PI: Dr. Edward V. Browell). The farthest north position of the DC-8 (89.4N) was reached at 14.12, and the thin black line running across the page shows the ER-2 flight track above the DC-8. The two aircraft flight tracks were coincident to within 50km and 15 minutes. Backscatter ratio values greater than 0.8 are indicated by the deep red colour.

► **Figure 4**

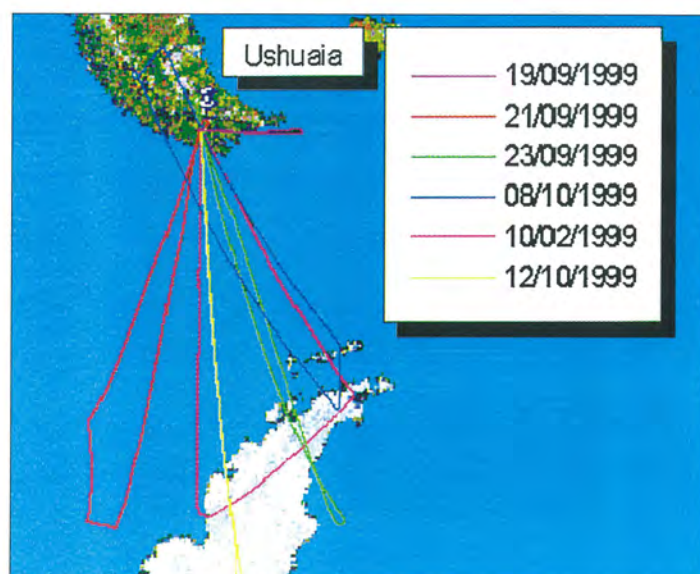
Polar Stratospheric Cloud photographed by Drs. Randy Kawa and Paul Newman on January 27, 2000 from the ground just outside the Arena Arctica hangar in Kiruna, Sweden.



Airborne Polar Experiment Geophysica Aircraft in Antarctica (APE-GAIA)

► **Figure 1**

Map of the test flight and the five scientific flights performed by the M-55 Geophysica from Ushuaia (Argentina) during the APE-GAIA mission.



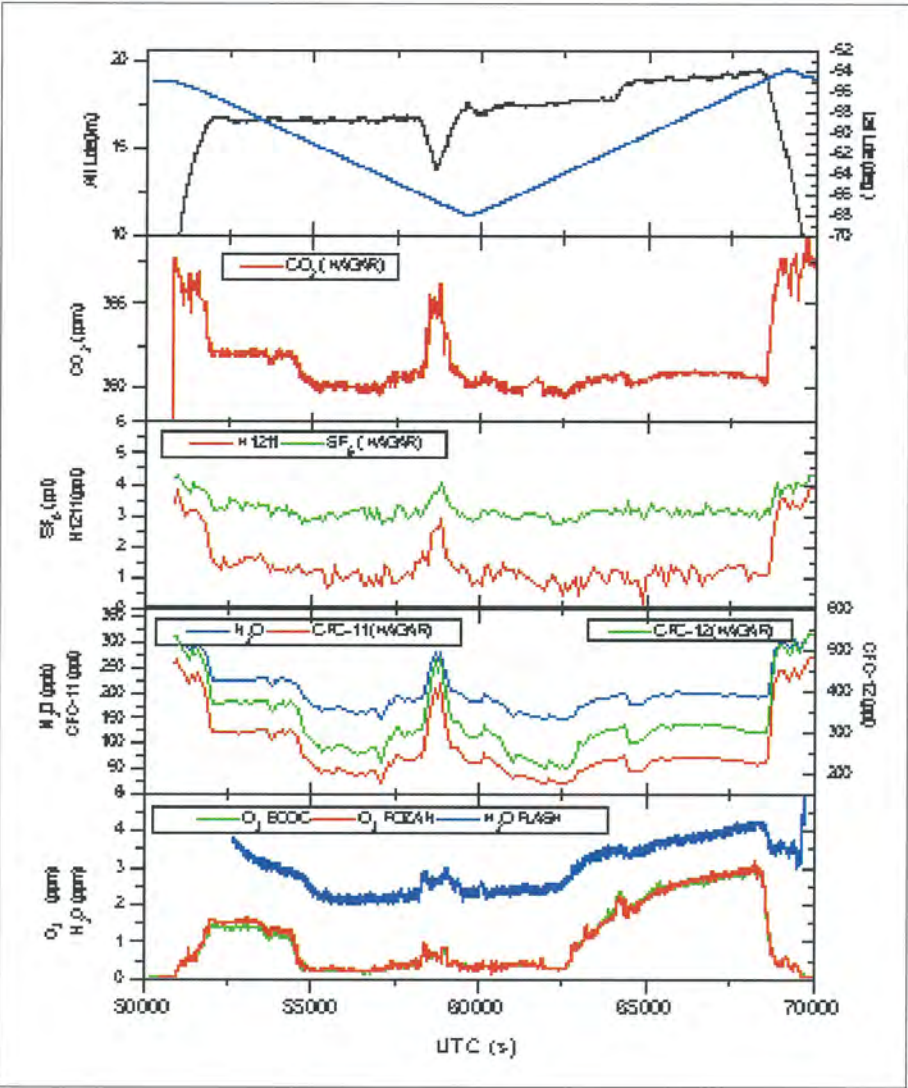


Figure 2
Altitude, latitude and in-situ measured minor constituents acquired during the flight of 23 September 1999. The measurements are preliminary.

VII

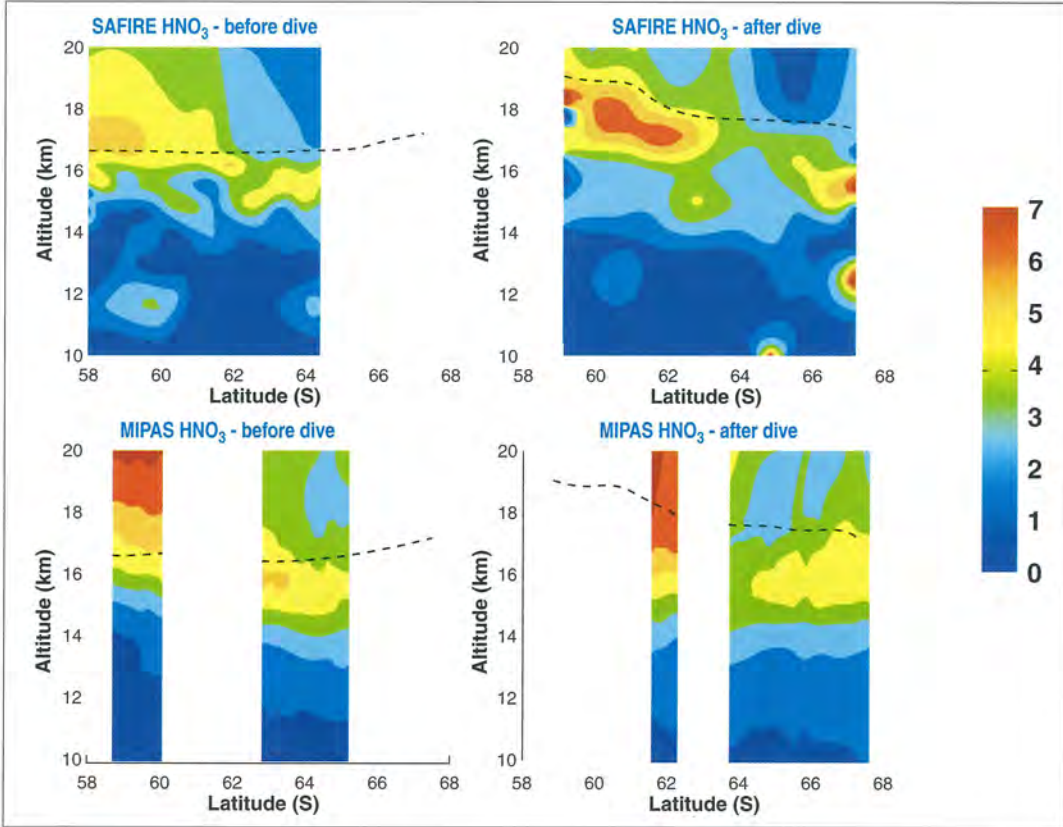
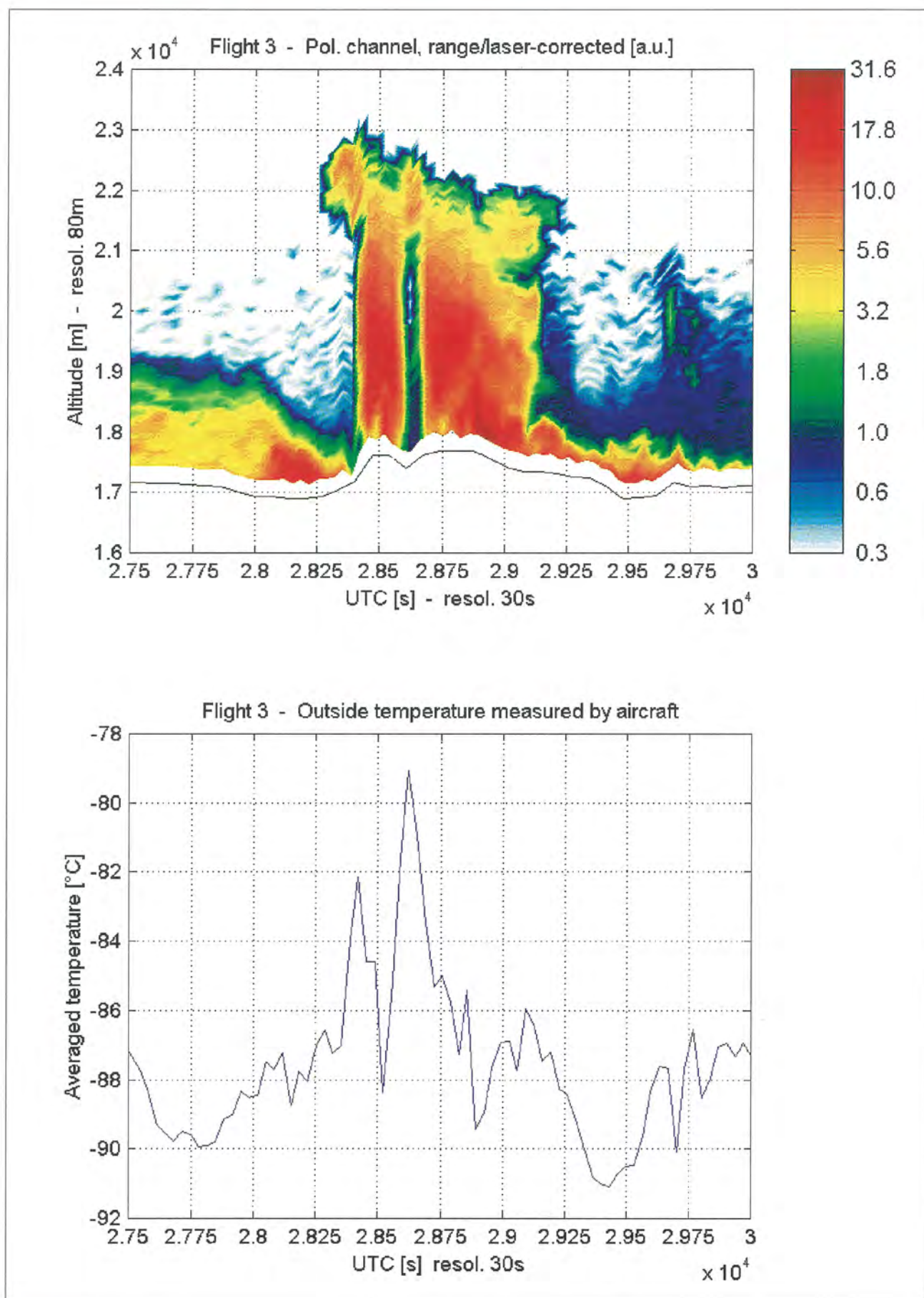


Figure 3
The volume mixing ratio [ppb] of nitric acid as a function of altitude and latitude for two remote sensing instruments and the two legs of the same flight of 23 September 1999. The continuous line shows the flight altitude. The measurements are preliminary.



▲ **Figure 4**

Observations of the MAL instrument during the flight of 2 October 1999 in the area in which lee wave formation was forecast. The upper panel presents the altitude profile of the back-scattering signal, the lower panel presents the temperature of the atmosphere at the flight level.

Report on the SAGE III Ozone Loss and Validation Experiment, SOLVE

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During the winter of 1999-2000, the NASA-sponsored SAGE III Ozone Loss and Validation Experiment (SOLVE) was jointly conducted with the European Commission-sponsored Third European Stratospheric Experiment on Ozone (THESEO 2000).

This SOLVE/THESEO-2000 mission was designed to both investigate the processes that control polar and mid-latitude winter and spring ozone levels, and to validate measurements from the Stratospheric Aerosol and Gas Experiment (SAGE) instrument. Unfortunately, SAGE III was not launched on schedule, but SOLVE/THESEO-2000 was able to take advantage of our other satellite assets to understand polar ozone losses from this last winter.

Since the discovery of the Antarctic ozone hole in 1985 by Farman *et al.* [1985], concern over the possible appearance of similar Arctic losses has led to a series of American and European field campaigns in the Arctic region. In fact, during the 1995-96 and 1996-97 winters, large losses were observed over the Arctic as a direct result of the cold and persistent stratospheric winter conditions. While measurement campaigns during the last decade have greatly increased our understanding of polar processes and the reasons behind these low ozone winters, a number of key questions remain unanswered and certain observed aspects of the polar stratospheric winter remain unresolved. The science goals of SOLVE/THESEO-2000 involved: 1) acquiring a complete body of information that would allow us to understand the detailed mechanisms that lead to these large ozone losses, 2) understanding the possible bearing of polar ozone on mid-latitude ozone decline, and 3) to sufficiently validate satellite instrument ozone observations used for long term trends in the polar regions.

The SOLVE/THESEO-2000 campaign combined aircraft, ground, balloon, satellite, and theoretical investiga-

tions for a comprehensive measurement campaign over the course of the 1999-2000 Arctic winter. Aircraft platforms included the NASA ER-2 high-altitude aircraft, the NASA DC-8 long-range aircraft, the DLR Falcon, the French Mystère, the French ARAT, and the Swiss Learjet. Amongst the numerous balloon launches were two launches of the NASA OMS in situ payload, two launches of the NASA OMS remote payload, two launches of the PSC analysis payload, a launch of the TRIPLE payload, launches of the CNRS SAOZ package, 2 launches of the CNRS long duration balloons, launches of the HALOZ package, and numerous ozone sonde launches as part of the MATCH campaign. Additional ground based measurements were provided from instruments at Kiruna, Alomar, and Ny-Alesund.

SOLVE was staged from Kiruna, Sweden over three phases: 1) early winter (15 Nov. to 15 Dec.), 2) mid-winter (12 Jan. to 5 Feb.), and 3) late winter (24 Feb. to 14 Mar.). The first

phase was timed to see the early winter conditions both prior to and during the appearance of polar stratospheric clouds (PSCs). The second phase was timed to the peak period of PSC activity and chlorine activation. The third and final phase was timed to the period of large ozone loss. The aircraft were stationed at the Arena Arctica facility at the Kiruna airport, while the balloon launches were made from the balloon launch Facility at Esrange, about 30km from Kiruna.

The 1999-2000 winter was quite cold, and the deployment timing worked perfectly. Figure 1 shows the total area covered by temperatures below 195K at 50hPa in the Arctic as derived from the NCEP/NCAR reanalysis data from 1958 to the present. The 1st row of diamonds at the top of the figure show the dates of DC-8 flights, and the 2nd row shows the dates of the ER-2 flights. The initial deployment of the DC-8 occurred during this period of first appearance of temperatures cold enough for PSC formation. The 2nd

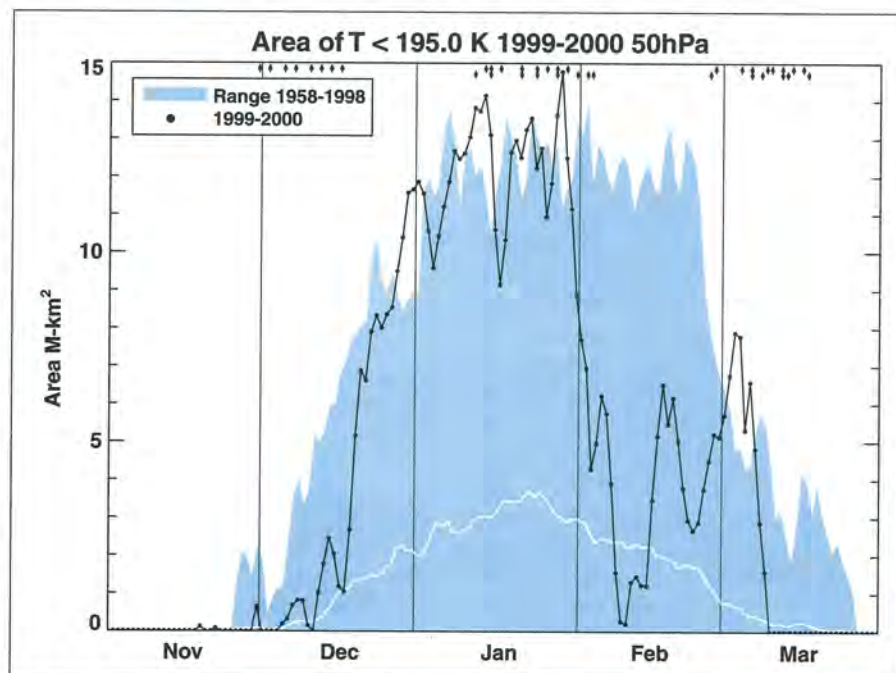


Figure 1: Areal coverage of temperatures below 195K in the Arctic during the 1999-2000 winter (connected dots). The grey shading shows the range of this areal coverage for all of the winters from 1958-59 to 1998-99, while the white line shows the average areal coverage from this 42-year climatology.

deployment included both the NASA ER-2, NASA DC-8, DLR Falcon, and French ARAT aircraft. Cold temperatures covered extensive regions over this period. In fact, the coverage nearly reached 15 million km² on January 28, 2000, setting a record for the Arctic winter in this 42-year climatology. The third period covered the disappearance of cold temperatures as the vortex began its break up. During this period, the vortex was again sampled by the NASA ER-2, NASA DC-8, and the Swiss Lear Jet. The areal coverage of cold temperatures was almost always greater than average for the winter with the exception of a few isolated days in December, a week long period in February, and a slightly below average period from mid-March.

The cold temperatures observed during the December deployment are illustrated with data from the NASA/GSFC AROTEL Rayleigh lidar aboard the NASA DC-8 (Figure 2). This flight track was the return trip from Kiruna, Sweden to NASA Dryden on December 16, 1999. The flight passed over Northern Greenland, down across Central Canada, and directly to NASA Dryden in Southern California. As can be seen from the plot, temperatures were quite cold in the mid-altitude region between 22 and 28km (30hPa is at about 24km).

The cold temperatures of the Arctic lower stratosphere during the 1999-2000 winter led to the formation of extensive layers of polar stratospheric clouds (PSCs). While PSCs were observed during the December period, extensive PSCs were encountered during the second deployment by the various platforms of SOLVE/THESEO-2000. The importance of PSCs to the chemistry of the stratosphere is that they convert the reservoir species HCL and ClONO₂ into reactive forms of chlorine. The extensive coverage of PSCs fully processed the air inside the vortex by mid-December. Temperatures tended to remain cold throughout the winter and into mid-March. Figure 3 displays 1064nm backscatter data from the NASA Langley UV Differential Absorption Lidar (DIAL) system on January 20, 2000. In this figure, the PSCs are viewed as

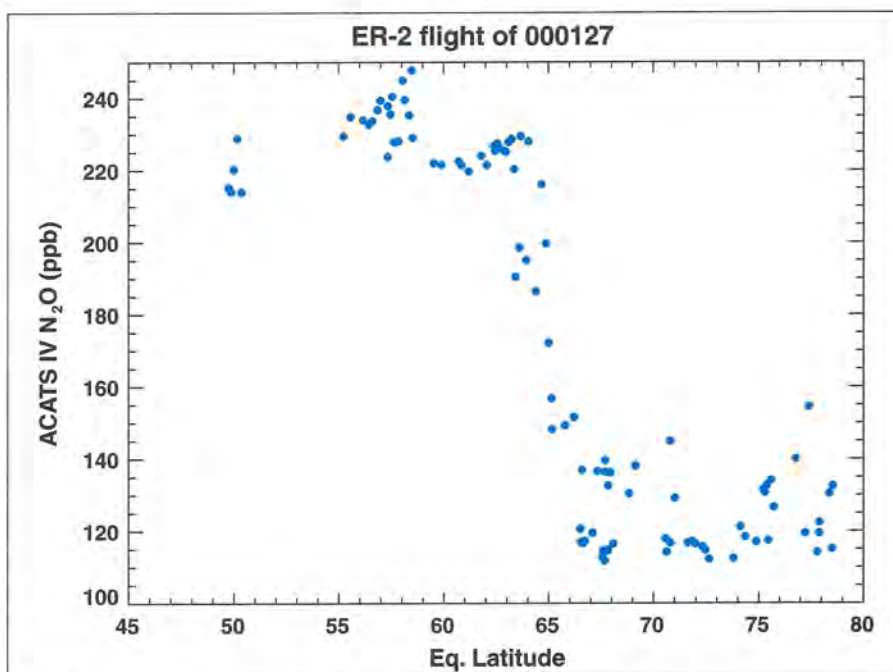


Figure 5. Measurements of N₂O from the ACATS-IV gas chromatograph (PI: James Elkins) versus equivalent latitude for the ER-2 flight of January 27, 2000.

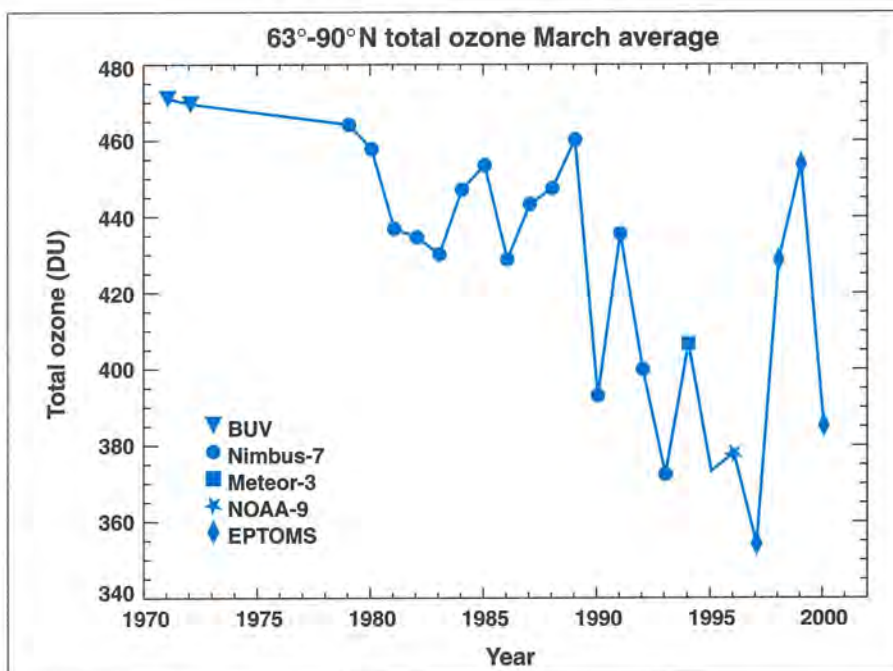


Figure 6. March average of Arctic total ozone from satellite observations. The various satellites are indicated on the figure legend. This figure is updated from Newman et al. [1997].

the higher backscatter ratios as green, yellow, and red colours. This flight was the first joint flight of the ER-2 and DC-8 during the SOLVE mission. The flight track ran from Kiruna, Sweden up to Spitzbergen, and then up to the North Pole, with a return along the same track. The maximum latitude of 89°N is reached near 14:12 GMT. The flight track of the

ER-2 above the DC-8 is superimposed as the black line. The lidar clearly shows the extensive PSC that the ER-2 sampled *in-situ* over the course of this flight. These PSCs were both extensive and thick during this phase of SOLVE. Figure 4 shows one of these beautiful PSCs as photographed from the ground in Kiruna on January 27, 2000.

The levels of ClO and Cl₂O₂ were also quite high during the campaign. The Harvard ClONO₂ instrument makes in situ measurements of ClONO₂, ClO, Cl₂O₂, and NO₂. Figure 5 displays a trace of N₂O measured by the NOAA/CMDL Airborne Chromatograph for Atmospheric Trace Species (ACATS IV, PI: Dr. James Elkins) as a function of latitude during a flight into Russia (just near St. Petersburg, then down to about 50°N) on January 27, 2000. Inside the vortex are found very low N₂O values, while outside the vortex, the N₂O values are quite high. Hence, N₂O provides a high resolution marker for the vortex position. Associated with the low inner vortex N₂O are very high levels of ClOx as a result of conversion of HCl and ClONO₂ into reactive forms on the surfaces of PSCs. Outside the vortex, levels of ClOx were quite low, while levels of HCl and ClONO₂ were quite high. In addition to the scientific importance of this flight, this flight was a historic first flight of the ER-2 (the civilian

version of the U-2) into Russian airspace since of Francis Gary Powers in the U-2 in May 1960. This flight would not have been possible without the close collaboration of our Russian colleagues in obtaining overflight clearance.

Because of the high amounts of ClOx in the Arctic, and because of the persistence of PSCs into March, there was large chemical ozone losses in the lower stratosphere. Using *in-situ* ER-2 ozone observations constrained by measurements of long lived tracers, we estimate that the local loss at about 20km exceeded 60%. Local losses of this magnitude are in reasonable agreement with the Total Ozone Mapping Spectrometer (TOMS) observations from the Earth Probe Satellite. Figure 6 displays the March average of ozone in the polar region. March 2000 was an extremely low ozone winter, similar to the values observed during March 1997.

Activities generally came to a conclusion in mid-March with the departures

of the ER-2 and DC-8. However, additional launches of ozonesondes, two SAOZ flights, and some flights of the French Mystère provided further information on the ozone losses that continued into late March, and the breakup of the polar vortex.

The SOLVE/THESEO-2000 missions have concluded their observational phases, but the analysis of the information will continue into the next few years. The first joint science team meeting will be held in Palermo, Italy on September 25-29, 2000. A special session will then be held at the American Geophysical Union's meeting in San Francisco on December 15-19, 2000.

Additional details on SOLVE and THESEO-2000 can be respectively found via the World Wide Web at

<http://cloud1.arc.nasa.gov/solve/>
and

<http://www.ozone-sec.ch.cam.ac.uk/>



Airborne Polar Experiment Geophysica Aircraft in Antarctica (APE-GAIA)

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Introduction

APE-GAIA (Airborne Polar Experiment -Geophysica Aircraft In Antarctica, Carli et al. 1999) has obtained interesting new information on the stratospheric chemistry and transport during the transition of the southern polar vortex from the ozone depletion to the recovery phase. The mission with the high-altitude aircraft M-55 Geophysica took place from 15 September to 15 October 1999 when five scientific flights were performed from Ushuaia (55°S, 68°W). A comprehensive

payload of remote-sensing and *in-situ* instruments monitored an essentially unexplored altitude range of the vortex. Flying southward over the Antarctic Peninsula at latitudes up to 70°S and at altitudes from 14 to 20km, while probing the vertical distribution with remote sensing techniques and occasional 'dives', allowed to investigate the main targets of the mission (Carli and Blom, 1997):

- the chemical composition of the Antarctic lowermost stratosphere during the transition phase,

- the mixing of mid-latitude and polar air masses, and
- the re-activation of chlorine over the Antarctic peninsula.

The planning of the flights was first based on the relevant scientific objectives (M. P. Chipperfield and G. Redaelli, 1999). Individual flight plans were then finalised using detailed forecasts. These forecasts included standard output from global numerical weather prediction models, as well as high resolution trajectory/contour advection models (to identify air filaments), lee wave

models (to identify mountain wave PSCs) and global chemistry transport models. The use was also made of near real-time TOMS column O_3 data.

The M55-Geophysica aircraft has become available for scientific purposes in the frame of a co-operation between the Italian National Programme for Antarctic Research (PNRA) and the Russian Myasishchev Design Bureau (MDB), AviaEcoCenter (AEC), and Central Aerological Observatory (CAO) of Moscow. The first mission (APE-POLECAT) was held out of Rovaniemi, Finland, in winter 1996/97 (Stefanutti *et al.*, 1998). More recently, the Geophysica was used in the APE-THESEO campaign (MacKenzie *et al.*, 2000) to study transport and clouds at the tropical tropopause. With respect to these missions, APE-GAIA deployed a new payload that combined for the first time several instruments for the study of atmospheric chemistry. The earlier US-American ER-2 deployments AAOE (Tuck *et al.*, 1987) and ASHOE (Tuck *et al.*, 1997) demonstrated the importance of high-altitude aircraft for research in and around the southern polar vortex. The large payload capacity of the Geophysica, as well as its operational flexibility and manoeuvrability have offered, however, a new opportunity for observations above Antarctica. The capability for housing relatively large and heavy sensors (up to a maximum weight of 1500kg and to a volume larger than 11m³) has allowed the deployment of a large payload of remote-sensing and in-situ instruments aboard a single stratospheric aircraft. This, along with the fact that the Geophysica is able to fly at various altitudes, within cold air masses (temperature around -90°C), with deep dives, and to operate despite difficult ground conditions (strong cross wind), significantly advanced the scientific output of APE-GAIA. In this paper a short description of the campaign and some preliminary results are presented.

Instrumentation

The scientific payload of the M55 aircraft during the Antarctic campaign is summarised in Table 1. It includes remote sensing and *in-situ*

equipment for gaseous trace compounds, instruments aimed at characterising aerosols and cloud particles, and devices for radiation measurements. The remote sensing chemistry instruments are the core of this payload as they provide simultaneous measurements of a large suite of chemical species. The two Fourier Transform Spectrometers MIPAS-STR and SAFIRE-A make limb and upward soundings to obtain column amounts above the aircraft and profiles below. Together they cover the main components of the Cly family (ClO , HCl and $ClONO_2$), MIPAS-STR almost covers the entire NO_y family, whereas SAFIRE-A obtains information on Bry . GASCOD-A uses the DOAS technique and measures total amounts of the trace constituents at zenith and nadir in the case of high sun, and vertical profiles in the case of sunrise and sunset. Important active species observed by this instrument are $OCIO$ and BrO . The three instruments also observe several long-lived source gases. Their redundant measurements allow validation of the individual measurements. *In situ* instruments provide measurements of ozone, water vapour and long-lived tracers at flight altitude with high spatial resolution. From these measurements vertical distributions can be observed during dives of the aircraft. The horizontal variability of the atmospheric composition and the dynamics of the mixing can be derived during flights at constant altitude ensuring an important synergy with the more extended vertical maps that the remote observations provide on the basis of the assumption of horizontal homogeneity. Aerosols and PSC particles are detected and characterised by lidars and scatterometers pointing at different directions and operating with different ranges and different longitudinal resolutions. Correlation of PSC detection with the measurements of atmospheric composition simultaneously made by the chemistry payload provides an opportunity for the direct analysis of the effects of PSC formation (denitrification, dehydration, heterogeneous chemistry). Finally, two instruments provide measurements of radiation fluxes for a more precise quantification of photochemical parameters.

Scientific flights

Figure 1 shows a map of the five flights performed from Ushuaia. The flight route and timing of each flight were determined on the basis of stratospheric meteorological forecast complemented by high resolution reverse domain filling trajectories (RDFT) maps (Sutton *et al.*, 1994) and MM5 mesoscale fields (Ferretti *et al.* 2000) and were verified with an early alert of the flight opportunities made possible by ground weather forecast.

The modelling activities provided, in forecast mode, accurate information about the location of the boundaries of the polar vortex, occurrence of lee waves and formation of filaments eroded from the vortex edge. Three flights aimed at entering as deep as possible inside the polar vortex and at performing dives inside, one of them reaching down to almost the tropopause. This type of flight was repeated more than once in order to study the chemistry of the polar air and the mixing between polar vortex and middle latitude air at different times as a function of the season and with slightly different vertical coverage. One flight was performed inside a major event of lee wave formation over the Antarctic Peninsula, and one flight route crossed twice a filament eroded from the vortex edge. The flight planning took into account also the requirements of the different instruments, such as night time versus day time preference, constant versus variable altitude, and solar blinding effects. A compromise was always found and the heterogeneous character of the techniques allowed the simultaneous measurement of many different parameters with a major enhancement of the acquired information, even if some instrument with specific requirements was not always able to perform measurements during the whole flight.

The vortex edge

Figure 2 shows an example of observations made by the *in situ* instruments on 23 September. As shown by the top plot, the altitude was about 17km in the forward (southbound) leg. While maintaining the southbound direction, a dive was performed down to 14km. After

regaining altitude, the reverse (northbound) flight was performed, with two steps at 18km and at 19km. The first part of the flight was at night-time and the second in the day-time with sunrise occurring during the dive. Most species were observed by a single instrument, but in the case of ozone (bottom plot) two independent measurements are superimposed and show very good agreement. Ozone, water vapour and most tracers have very similar variations both in the general shape of the curves as well as in some small features. A rapid drop of concentrations synchronously observed for most species can be considered to identify the crossing of the vortex edge. From the figure one infers that during this particular flight the vortex edge was situated at 60°S on the forward leg (17km altitude), but at 62°S during the return leg (19km altitude). The changing latitudinal location of the vortex edge and gradients near it were also observed during other flights.

Together with the in-situ measurements shown in Figure 2, during the same flight vertical profiles of several minor constituents were observed by the remote sensing instruments as a function of latitude. A clear anti-correlation was observed by SAFIRE-A between ozone and ClO, the latter being detected only in the reverse leg in the daytime. A comprehensive analysis is still in progress. As an example of the detail of the information and of the remaining data analysis activities required for the remote sensing instruments, Figure 3 shows a comparison between the measurements of HNO₃ made with SAFIRE-A and MIPAS-STR. Colour-contour plots are used for the comparison of the two measurements performed on different grids in the altitude-latitude domain. A simplified and possibly incomplete spectroscopic data base is used in the case of SAFIRE-A, and temperature profiles provided by models instead of those directly measured are used in the case of MIPAS-STR. Furthermore, some gaps were present in the data acquisition of the two instruments during this particular flight and different regularisation and determination of the distribution above the flight altitude were operated in the two retrievals.

Despite the fact that some differences are observed both between the two instruments and between the forward and reverse legs of each instrument, the overall distributions are quite consistent. At latitudes less than 63°S, HNO₃ is mainly observed at altitudes above 16km, there is a reduction of HNO₃ concentration around 64°S and the distribution peaked at 16km at latitudes higher than 65°. The lower altitude of the HNO₃ maximum with increasing latitude is in agreement with the expected effect of the sedimentation process. The differences observed between the forward and the reverse legs could also be due to the atmospheric variability since the aircraft followed the same track in the two legs and the remote instruments looked on the two sides at air masses that are up to 500km apart. A detailed analysis of the results obtained by the two instruments will validate the observed differences and provide a better understanding of the atmospheric variability.

Lee waves and filaments

With an alert of about 36 hours, the model predicted lee wave formation over the Antarctic Peninsula for 2 October 1999. A flight route was accordingly aimed at probing the temperature oscillations and observing the induced effects in the atmosphere. Measurements made during that flight are shown in Figure 4, which displays a comparison of the back-scattering signal measured above the aircraft by MAL-1 and the local temperature as a function of time. The back-scattering signal is corrected for the aircraft altitude and placed at the correct relative position in the atmosphere. The very low ambient temperature made it difficult for the aircraft to maintain a constant altitude. The temperature profile along the flight track shows clearly some oscillations, at different frequencies, with the major amplitudes well matched in time and space with the forecast temperature fields. The PSC occurrences are also well correlated with local temperature minima.

From high resolution RDFT fields, an air filament was forecast for 8 October 1999, between Argentina and the Antarctic Peninsula.

The signature of the filament was already clearly present in the RDFT forecast products 96 hours in advance. In order to verify and study the occurrence of this structure, a flight was planned, with a four legs track on two isentropic surfaces: 390K and 450K. As forecast, while flying quasi isentropically at 450 K, the aircraft observed intense and localised variations of the tracers, supposedly related to the sampling of the vortex filament structures. Also, the RDFT products (not shown here) picture a three-dimensional filament with strong horizontal gradients, large vertical depth and high resolution internal structure that seems to correspond well to the preliminary analysis of the time series of the experimental data. Further analyses involving the complete set of tracer data available from onboard instrumentation are needed to have a more complete understanding of this case.

Auxiliary measurements

In parallel to the Geophysica flights, a series of correlative ground-based and balloon measurements were made from different sites located in Tierra del Fuego and on the Antarctic Peninsula. Ground-based ozone, NO₂, SO₂ and UV radiation measurements were carried out at Ushuaia. On the Antarctic Peninsula, real-time measurements of total ozone and NO₂ were obtained from the base of the British Antarctic Survey in Rothera (67° 34' S; 68° 07' W), along with radiosonde launches and ozone soundings. Total column ozone values were obtained from the Argentinean bases of Marambio (64°23' S, 56°72' W) and Belgrano II (77° 52' S; 34° 37' W), the activities in the former base being performed in collaboration with the Finnish Meteorological Institute. At these two bases, ozone and NO₂ measurements were also obtained by the INTA (Spain) and CNR-IFA (Italy). A preliminary comparison of the ozone measurements made at Marambio and from the aircraft during a dive made nearby during the flight of 8 October show very similar shape, but some quantitative difference in the amplitude of the ozone variability. The different resolution and coverage of aircraft and ground based observation makes the comparison difficult, but enlarges significantly the comprehensiveness of the acquired picture.

Measurements during the transfer flight

The transfer flights of the Geophysica from Europe to South America and reverse made it possible to explore the upper troposphere and lower stratosphere over a wide range of latitudes, and to study the interactions between the polar vortex and the mid-latitude stratosphere. These measurements were very demanding from the logistical point of view. The limited range of the M55 required the transfer flights to be completed in four legs. However, access to the instruments was only possible at the airport of Recife (Brazil), with no access during the other two short stopovers. For this reason, the instruments had to operate and store data for a period of 14 hours of unattended operation. During the transfer flights auxiliary measurements were provided from Buenos Aires, Mar del Plata, Sal Island and Izana (Canary Islands).

Conclusions

APE-GAIA has been a major international collaboration. About 70 scientists, engineers and technicians worked in Ushuaia during the campaign, and almost as many at other locations were involved, contributing with models, data analysis, auxiliary measurements and logistic organisation. A total of nine countries (Italy, Russia, Germany, United Kingdom, Switzerland, Finland, Spain, Argentina, United States) participated in the project. The M-55 stratospheric aircraft operated successfully from the base of Ushuaia in Argentina with a comprehensive set of new instruments. Thorough forecast modelling allowed accurate planning of the observations aimed at the exploration of the edge of the polar vortex, and the probing of lee waves and air filaments. The timely operation of both the aircraft and the instruments ensured the operative success of the campaign. The conflicting measurement requirements of the different observing methods have only marginally reduced the productivity of the single instruments while ensuring a unique wealth of complementary information. The features of the vortex edge were probed at different altitudes with the simultaneous

measurement of a large number of tracer species. Patterns of minor constituent distributions were observed as a function of altitude and latitude by the remote sensing instruments. An example has been shown in the case of HNO_3 in which strong indications of sedimentation effects are observed. The formation of dense clouds of ice particles was observed at 20km altitude in the cold regions generated by orographic waves above the Antarctic mountains. A filament of polar vortex air was forecast and then sampled by the Geophysica. The flights from Moscow to Ushuaia and back were used to perform further scientific measurements over a wide latitude range. The bulk of the data will require an elaborate analysis process (comparisons between the instruments, testing of the consistency of the models with the observations, etc). It is believed that the campaign has provided important new information on the dynamics of the vortex and on the processes controlling ozone in the atmosphere that will improve our estimates of Antarctic ozone recovery time after the phasing out of CFCs.

Acknowledgments

We gratefully acknowledge the support of PNRA (Programma Nazionale di Ricerche in Antartide), which started the project in 1995 in collaboration with the Russian Myasishchev Design Bureau, and funded most of the aircraft preparation and campaign activities. The project was possible also thanks to the contributions of several national and European research agencies such as ASI (Agenzia Spaziale Italiana), CNR (Consiglio Nazionale delle Ricerche, Italy), PSTda (Parco Scientifico e Tecnologico di Abruzzo, Italy), BMBF (Bundesministerium für Bildung, Wissenschaft, Forschung und Technologie, Germany), NERC (U.K. Natural Environment Research Council), Royal Society (U.K.), OFES and FNRS (Switzerland), ESF (European Science Foundation), EU (European Union), which concurred in the development of new dedicated instrumentation, in the execution of intermediate campaigns and in the support of the campaign participation. The mission would not have been possible without the assistance

and help of numerous hosting organisations and individuals, to whom we wish to express our gratitude.

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Brewer-Dobson Workshop,

13-15 December 1999, Oxford, UK

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First row: Alan Iw, Steven Pawson, Amanda Kerr-Munslow, Vicky West, Sarah Sparrow, Maarten van Aalst.

Second row: Ross Bannister, Joe Farman, Adrian Tuck, Kirill Semeniuk, Darryn Waugh, Alan Brewer, Peter Siegmund, Jim Holton, Emily Shuckburgh, David Rind, Michael McIntyre, Philip Mote, John Austin, Hans Graf, Alan Plumb, Lesley Gray, Kristie Boering, John Pyle, Alan O'Neill, Warwick Norton.

Third row: Peter Haynes, Dave Stainforth, Michael Palmer, Bill Grose, Katrin Nissen, Antje Dethof, Martyn Juckes, Bill Randel, Lon Hood, Giovanni Pitari, Hubert Teyssedre.

Fourth row: Tim Hall, Ulrich Schmidt, Bjorn Hassler, Karen Rosenlof, Eliza Manzini, Adrian Simmons, Byron Boville, Toshihiko Hirooka, Kevin Hamilton, John Thuburn.

To mark 50 years since Brewer's seminal paper on the transport of water vapour (Brewer, 1949), and 70 years since Dobson's paper on the transport of ozone (Dobson *et al.*, 1929), a 3-day workshop was held to review all aspects of the Brewer-Dobson circulation. The Sub-department of Atmospheric, Oceanic and Planetary Physics, University of Oxford, was an appropriate venue since Alan Brewer worked there from 1948 to 1962 and Gordon Dobson worked there from 1920 to 1956 (and continued his research from his house on Shotover Hill on the outskirts of Oxford until his death in 1976).

There were 33 talks given at the workshop which covered topics including

the driving mechanisms of the Brewer-Dobson circulation (with particular emphasis on the tropical region), new diagnostics to understand stratospheric transport and mixing, the representation of the Brewer-Dobson circulation in analysis systems and GCMs, diagnosis of the Brewer-Dobson circulation from long-lived tracers (e.g. age of air, water vapour and ozone), and possible future changes in the Brewer-Dobson circulation from changes in greenhouse gases.

The workshop started off with talks by Jim Holton and Alan Brewer of how our understanding of Brewer-Dobson circulation has developed over the last 50 years. Transcriptions of these particularly interesting talks are reproduced here.

50 years of the Brewer-Dobson circulation, a personal view

One can divide the history of the study of the Brewer-Dobson circulation into 5 decades, more or less. First in the '50s people who thought like physicists were attacking the problem and moved the subject forward through physical reasoning. In the '60s, thanks to the MIT school where I was a graduate student at the time, we really went backwards because of the domination of the Eulerian mean way of thinking. In the '70s, with Andrews and McIntyre and others, theory moved us forward. In the '80s, benefiting from this theory, modelling moved

us forward. In the '90s, it was observations with the Upper Atmosphere Research Satellite, and theory done here (in the UK) and elsewhere. The last couple of decades will be covered by other people, so I will concentrate on the first three decades.

So what is the Brewer-Dobson circulation? It is summarised in the classic figure from Brewer (1949). This beautiful diagram has arrows showing circulation upward through the tropical tropopause poleward and then downward, with a concentration showing a stronger circulation in the winter hemisphere than in the summer hemisphere. The numbers mentioned were quite remarkable, Brewer (1949) said 'The observed distributions of water vapour can be explained by the existence of a circulation in which air enters the stratosphere at the equator, where it is dried by condensation, travels in the stratosphere to temperate and polar regions, and sinks into the troposphere. The sinking however, will warm the air unless it is cooled by radiation and the idea of a stratosphere in radiative equilibrium must be abandoned. The cooling rate must lie between about 0.1 and 1.1°C per day but a value near 0.5°C per day seems most probable. At the equator the ascending air must be subject to heating by radiation.' It is interesting to see that he didn't say the air is forced to ascend by heating, but rather in order to ascend it had to be heated.

What was Dobson's role? Brewer (1949) quotes from Dobson *et al.* (1929): 'The only way in which we can reconcile the observed high ozone concentration in the Arctic in spring and the low concentration in the tropics, with the hypothesis that the ozone is formed by the action of the sunlight, would be to suppose a general slow poleward drift in the highest atmosphere with a slow descent of air near the poles. Such a current would carry the ozone formed in low latitudes to the poles and concentrate it there. If this were the case the ozone at the poles would be distributed through a moderate depth of atmosphere while that in low latitudes would all be high up.' So had Dobson figured it out in 1929? Definitely not; Dobson (1929) goes on to say that it couldn't work this way because there was no evidence in 1929 that the altitude of

ozone maximum in high latitudes was lower than it was at low latitudes which would be required for that circulation to hold.

Next in Dobson, Brewer and Cwilog (1946) they comment 'It has often been suggested that the difference in temperature in the stratosphere between low and high latitudes is due to general world circulation between the equator and the pole which causes the air in the stratosphere to rise slowly near the equator and subside slowly near the pole.' They (i.e. Dobson) then expressed doubts as to whether this is really the case. However Dobson (1956) on the basis of better ozone observations, Chapman theory, and Brewer's (1949) work had accepted the global circulation in the stratosphere. But, his schematic figure is not as good as that of Brewer (1949). He still thought that vertical mixing by turbulence was a predominant factor.

In 1959 as an undergraduate at Harvard I took my first meteorology course, taught by Richard Goody. Goody told me that we had the necessary information to calculate the heat budget of the stratosphere. He thought this would be a great PhD project for somebody, and indicated that he would like me to stay at Harvard and work with him. Fortunately I didn't take up that topic because soon after that, Murgatroyd and Singleton (1961) published an analysis of the heat budget of the stratosphere. As a result of calculating the heating and cooling rates they calculated what we now call the diabatic circulation. They did point out there were serious problems in the momentum budget, but didn't address this any more than had Brewer or Dobson earlier.

When I started graduate school at MIT in the early '60s, people were aware of the work of Dobson and Brewer, and Murgatroyd and Singleton, but the message that the professors at MIT gave was that this work was all wrong and could be ignored. We lost a lot of time through the '60s because of two different approaches. First there were deductions from radioactive tracers. In the late '50s there was extensive atmospheric nuclear testing by the US in the tropical Pacific. Very early on, Dyer and Yeo (1960) in Australia argued that the pattern of

radioactive fallout measured on the ground at Melbourne was consistent with the Brewer-Dobson circulation, with the maximum observed a few months after the equatorial explosion. But Feely and Spar (1960) in the US, basing their information on observations of tungsten 185 in the stratosphere from U2 aircraft, saw rapid meridional transport along the isentropes with very little evidence of an upward and poleward advective circulation. They argued that Brewer and Dobson were wrong, and that really eddy motions were mixing things along isentropes. Reggie Newell at MIT was one of the primary backers of the view that it was meridional mixing by eddies that controlled the distribution of tracers and the heat budget. In Newell (1963) he said 'Large scale quasi-horizontal eddies can transport ozone poleward in sufficient quantities to account for the spring build up of ozone. Such large-scale mixing as opposed to mean meridional motions also allows explanation of radioactive tungsten in the stratosphere.' Sawyer (1965) argued that to explain the heat budget you didn't need a mean meridional circulation at all, it could all be done by eddies.

The other line of reasoning in the '60s which took us back a bit was the Victor Starr school of general circulation research; the Eulerian mean approach for understanding the circulation. Vincent (1968) had the most complete analysis of the Eulerian mean meridional circulation in the stratosphere. He found (which had been found somewhat earlier) that if you do averaging on isobaric surfaces around latitude circles that, rather than seeing the single per hemisphere cell Brewer-Dobson circulation, there are two cells, with a reverse cell in the high latitudes with upward motion in the poleward region and downward motion in mid-latitudes.

Discovery of this led to discussion, when I was a graduate student at MIT, of a magical situation in which the eddy motions were able to work against that reverse Eulerian mean meridional circulation in just the right manner to produce poleward and downward ozone transport, and the poleward water transport.

As far as I can tell, in the early part of the '70s, there was minimal activity in the study of transport in the

stratosphere. The next real progress was in the work of Andrews and McIntyre at Cambridge in the introduction of the transformed Eulerian mean (Andrews and McIntyre, 1976) and then the generalised Lagrangian mean (Andrews and McIntyre, 1978). But the transformed Eulerian mean paper wasn't addressing tracer transport but rather the momentum and heat budget of the stratosphere, only later people began using this as a way to approximate the transport circulation.

But before that, right at the end of the '60s, Jerry Mahlman (1969) in a general circulation model study had pointed out that the way you do the averaging really makes a big difference. He found in his GCM that if you did the Eulerian mean averaging, as the MIT school would have you do it, that indeed his GCM showed the two cell circulation with upward motion over the pole and sinking in mid-latitudes. But if he took a scheme which averaged along the streamlines of the jet stream, he then found he had a single cell very much like Brewer had originally shown. That paper didn't have much influence for quite a while, but after Andrews and McIntyre had done the work on the transformed Eulerian mean and the generalised Lagrangian mean, Wallace (1978) and then Matsuno (1980) gave a schematic interpretation to help people understand why the way you average really makes a difference.

Dunkerton (1978), when he was my student at the University of Washington, after reading Andrews and McIntyre, and thinking back to Murgatroyd and Singleton, quickly put a picture of the middle atmospheric circulation together in a paper which used a simplified version of the Andrews and McIntyre Lagrangian mean, together with the heating distribution of Murgatroyd and Singleton. His diagram showed mean transport streamlines with the Brewer-Dobson circulation in the stratosphere and a single pole-to-pole solstice circulation in the mesosphere.

I'll skip forward through the '80s and end with a couple of comments on the '90s. With the work of Haynes *et al.* (1991), we now have a much better understanding of what is forcing the Brewer-Dobson circulation, which is the upward propagation of planetary and gravity

waves from the troposphere. Wave breaking, particularly planetary wave breaking in the winter stratosphere, produces this westward force which McIntyre gave the name 'wave driven pump'. Because the earth is rapidly rotating, you get a quasi-gyroscopic effect in which a westward force produces a poleward drift. The poleward drift will draw air up in the equatorial region, which will then become colder than radiative equilibrium and so becomes radiatively heated, and push air down at high latitudes accompanied by radiative cooling. Here is the Brewer-Dobson circulation. The momentum budget problem, which was so difficult to understand for Brewer, Dobson, Murgatroyd and Singleton, is now solved by this wave breaking idea.

This same circulation will transport tracers. A vertically stratified tracer will tend to have its mixing ratio surfaces tilted by the effect of the upward and downward motion, but at the same time, the wave breaking is producing mixing in mid-latitudes. The effect of that mixing is to tend to homogenise the tracer distribution in this region. So that a typical mixing ratio surface will end up looking something like this, lifted high in the tropics, depressed downward in the polar vortex, and with a more or less level surface in mid-latitudes. That is just the sort of thing we see from the wonderful data from the UARS satellite, for example methane as viewed by the HALOE instrument.

The Brewer-Dobson circulation has recently become the standard way in which one tries to explain and understand the global-scale exchange between the troposphere and the stratosphere. This is taking us back in 1999 to 1949 where Brewer was explaining stratosphere/troposphere exchange of water vapour, which is one of the primary problems we are still working on today.

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The stratospheric circulation: a personnel history

My involvement with the stratosphere began in the height of summer 1942, in the very depths of World War II. The Germans controlled the Atlantic coast, from the North of Norway to the south of France. They were besieging Stalingrad, in sight of Moscow, crossing the Don and looking forward to dealing with the great oil fields of Asia Minor. In Africa they had reached El Alamein and Rommel was satisfied that with one last push they would capture Alexandria, the Nile Valley and the Suez canal. The Commonwealth would be split in two and Germany would join with Japan.

Me, I was just a shift forecaster at an RAF station not far from Oxford. In an afternoon off, a messenger came to say I had to return to the office because I was going to be moved. I was informed that I had to be at HQ in London first thing the next day. I got there and was briefed. The problem I was asked to deal with was condensation trails. It is quite easy to make a physical theory in which you balance the effects of water and heat from the aircraft. In a height-temperature plot, on one side of a line that we called Mintra (sloping from high altitude and low temperature to low altitude and high temperature) an aircraft was able to make trails because its passage increased the humidity, on the other side it was not. Everybody was very satisfied with this theory and this is what the pilots were told. However, in practice, pilots were rather worryingly reporting trails when they were flying at moderate altitude in regions where we expected no trails; and they were reporting no trails when flying at high altitude in regions where we were expecting big trails. Understanding this discrepancy was very important. In a raid on Nuremberg we had lost 93 aircraft, a totally unsustainable loss. It was therefore essential that we found out what was happening.

I was invited to investigate this. I was sent to Boscombe Down to a special unit, which was to be just me, attached to High Altitude Flight. I was briefed by the Director of the Met Office himself, who emphasised the problems and also mentioned



Alan Brewer, Kirill Semeniuk, and Michael McIntyre discussing the Brewer Spectrophotometer

the difficulties of making air temperature measurements from aircraft. I was told that Dobson had already started on hygrometry and would help and supervise. I was to have 2 Bostons and 1 sergeant instrument maker. (When I arrived at High Altitude Flight, I met a recent Cambridge graduate, a red-headed character who proved immensely helpful. His name was Richard Goody, and he was concerned with the avionics section. We worked side-by-side for the rest of the war.)

I realised that the air temperature measurement problem was a very acute one, and I soon learned that no-one had any idea how to solve it. (They were very busy, let me say). I knew that there was a real problem because we were getting quite ridiculous temperature measurements across the forecast bench. So the first thing to do was to solve this problem, which I was able to do very quickly and with great asset to my own prestige. A forecaster's prestige was not really very good - in modern terms somewhere between a lawyer and a politician - and to come in and solve a problem that was worrying the experts was a good start and it stood me in good stead for a long time. It also enabled me to capture a very competent instrument maker, whom Richard Goody had tipped me off about.

Dobson was rather annoyed at the delay. Dobson looked after me very closely, travelling the 50 miles down from Oxford to Boscombe Down. In the first place Dobson supplied a hygrometer which he had shown to

work in the lab. under a wide range of temperatures. However, in the aircraft it suffered from the fact that the light changes as the aircraft passes through cloud and turns etc. So, for aircraft use it was clear that we needed a proper illumination system. We therefore further developed the frost point hygrometer. The object is to watch for deposition of dew or frost on a surface that is ventilated by outside air. It is cooled by pumping a

coolant from below. The temperature is measured by a resistance thermometer. The viewing surface is at a focus of an elliptical glass lens, with a lamp at the other focus for illumination. You watch for deposition on the surface through a good magnifying glass. We soon had this working and the first time we made condensation trails under conditions when we ought not to have made them, it was clear that we were in that no-man's land where the air was saturated with respect to ice, but was not saturated with respect to super-cooled water; so the air could be clear, but the passage of the aircraft caused the trail. We had that solved fairly quickly. There was not much trouble with the fact because the physical possibility was well known.

The Boston aircraft had a performance almost exactly equal to that of the modern Hercules, with a ceiling of about 30,000 ft and so we could not get into the stratosphere where the expected trails did not occur. The air force provided a Flying Fortress said to be one of six that were given as a personal present from Roosevelt to Churchill. We took everything we could move out of it and we got it to 37,000 ft and this was the first time we got into the stratosphere. To my surprise, as the temperature turned up, the frost point turned down, and at the highest levels I could get no deposit. We were using solid carbon dioxide as a coolant and we obviously had to change to liquid oxygen, which we did, but it was clear that the reason that there were no trails in the stratosphere was that the air

was exceedingly dry. I would not have believed before I had started that the air could be so dry, but I saw it with my own eyes. I had plenty of trouble with people convincing them that we could really measure that dryness.

The war progressed and we got the Mosquito aircraft. The instrument was redesigned and rebuilt to work in the pressure cabin. The Mosquito was a lovely airplane. The cockpit was size of a small car and I sat side-by-side with the pilot, with the frost point hygrometer by my right hand and a short pipe going outside. I made very great effort indeed to ensure that the exceedingly dry measurements were really right. With the Mosquito could get higher, and with the new instrument the lowest frost point I ever measured was 190K absolute. The effects at the tropopause were very striking. Personally, I very soon formed the opinion that the very dry air had come from the equatorial tropopause. All we knew about temperatures at different altitudes in the tropics came from Brunt (1934), which had a figure based on measurements in the Dutch East Indies, now Indonesia. This showed the equatorial tropopause to be about 190K absolute consistent with my lowest frost point.

Dobson gave the 1946 Bakerian Lecture (Dobson, 1946). It was Dobson's lecture; Cwilog's name and my name were put on it simply because he used the humidity measurements. If you examine it you will find that he is actually merely reviewing the traditional theory that had been started by Emden (1913) and had continued through Simpson (1928) and in it he recalculates the radiation balance of the stratosphere with the new humidity measurements. Simpson had assumed that there was 0.3mm of precipitable water in the stratosphere, we had divided that by something like 50. Dobson was attempting to make a new calculation with the new concept of the dryness.

Eric Eady was an old friend. We had been on a Met Office training course together, we had worked on the same forecast bench. He came and visited me occasionally and he would stay at my home. He came to my office and we would talk. He would tell me about long waves in the westerlies and baroclinic waves and I would tell him how the

stratospheric air had come from the equatorial tropopause. He was not convinced. He had very great difficulties with the momentum problem and in fact there is a caveat in my 1949 paper (Brewer, 1949) to the effect that there is a problem with momentum. Dobson was not very impressed with my suggestions as an explanation of the ozone observations. I came to Oxford and at about that time Dobson began to work with R.H. Kay developing a sampler to measure ozone concentration that could be flown on an aircraft, it was based around being flown on a Met Flight Mosquito. It followed the method in which iodine is produced by the ozone and is measured electrochemically. We got no useful results in Southern England. Then one day Dobby came into the lab and said he had had a letter from Tönsberg in Tromsö. Tönsberg was the very active director of the observatory in Tromsö and had worked with Dobson for many years on ozone measurements. The early high latitude measurements that are so often quoted are due to him. I suspect that Tönsberg had been measuring the low level of the ozone and no-one was believing him. Tönsberg said that if we sent someone with the sampler he could get flights from Tromsö to measure ozone through the tropopause. Dobby asked me what we should do and I said we should take up the offer. Dobby asked who we should send and I said a student, and if we can't find a student then I would go. Well, we didn't have a student and I went. I was able to get 4 good flights there in July 1955 on a Vampire jet that flew to 43,000 ft. The results showed a very sharp transition in ozone concentration at the tropopause (Brewer, 1957). Dobby had been totally committed to a stagnant stratosphere, but after these results he was convinced and he published his well known paper. The results convinced me that we needed an ozonesonde. Soon afterwards James Milford joined the department as a graduate student and took on the job of making one.

Now, there was in the late fifties the atom bombs and hydrogen bombs. The hydrogen bombs were penetrating stratosphere and going to a great height. We were told that it didn't matter because the debris would go up there and float around for years and years, quietly decaying. But, of

course it came out PDQ. What was more, they fired the hydrogen bombs in the Pacific, but the debris came out in a broad swathe from California to Boston, where it worried people who mattered. As a result, in New York in 1960 the UN Scientific committee held a discussion on the problem of the transfer of debris into the troposphere. I received a letter from the Director of the Met Office, who was Sutton at the time, saying that he had an invitation for me to go to the discussion, but that there were no provisions in any budget for my expenses. So, I proposed not to go, but I was on sabbatical leave at MIT at the time, and MIT encouraged me to go and offered to pay my expenses. So I went. It was a meeting of about 50 people, and it covered a lot more than just the stratosphere, focusing on all aspects of the fallout problem. I gave my opinions and MIT, having paid my expenses, reasonably invited me to write up what I said. I had forgotten about this until a copy of the MIT report (Brewer, 1960) turned up in Dobson's papers and was brought to my attention by John Barnett. One of the things that I pointed out was that there is a great readjustment of the level of ozone. We had made some ozone ascents in 1958 using Milford's ozonesonde. Some were made in Antarctica, I went to Malta (36°N), and I sent John Houghton (!) to Tromsö (70°N). The differences at different latitudes were very clear. In my 1949 paper I noted that there has to be this circulation, but I hadn't the foggiest how fast it was. It might be a hundred metres a day, it might only be a few. By 1960, it was obviously fairly large because the fallout was coming very fast. I guessed, based on the outflow of radioactive debris, that the annual downflow was a tenth to a fifth of the hemispheric store. This was the greatest I dared say without being laughed out of court.

While I was at MIT on sabbatical in 1960 I had a bet with Reggie Newell. At the time, I said that I bet 25 cents that in spring we would get new outflow events even though there had been no bomb in the last six months. He bet 25 cents that there wouldn't be. We wrote it on a piece of paper, got it properly typed and signed it. I found the piece of paper about 20 years later and sent it to Reggie, claiming my 25 cents with interest. He sent me a silver dollar.

In 1960, Anthony Wilson joined the department and I invited him to make measurements of the UV that was producing the ozone. The hope was that by measuring the UV radiation at wavelength 200nm and its divergence we could, by using the Chapman equations, get an idea of the rate of ozone production and perhaps the rate at which it was being turned over. The results that we published in 1965 were that the solar radiation at wavelength 200nm is three times less than that reported by Detwiler *et al.* and that the oxygen absorption cross-section is about 25% lower than reported in Ditchborne and Young. We went on to examine what the consequences of this were. I thought that if we took an account of the whole global store of ozone and watch it swilling about from the equator to the winter hemisphere, and assume that it is always going downwards (so that it is made but never comes back), we ought to get a rough idea of the total turnover in ozone. We did the calculation and found that the downward transport was much larger than I had expected with about half the total store of ozone coming down each year. There was no doubt that the turnover was very large, and it was confirmed by the debris data that was coming in. The calculation also showed that the outflow is seasonal and that in the northern hemisphere is about three times that in the southern hemisphere. So then we set out to do the calculations to relate the radiation to the outflow of ozone. One problem is, where is the ozone being made? This was discussed in our 1968 paper (Brewer and Wilson, 1968). The difficulty was to get people to grasp that the ozone that matters is not being made at the top of the atmosphere, it is being made relatively low down. At the top of the atmosphere the ozone concentration, in mixing ratio terms, is highest and therefore people thought that this is where the ozone is made, but in fact this region is in photochemical equilibrium. This means that the Lagrangian equation gives you no ozone production, there may be lots of chemistry going on, but there is nothing being made to transport downwards. The main production region is at 10mb. There is considerable negative feedback in the ozone production process. If at high levels, some of the ozone is destroyed by nitrogen dioxide or

chlorine, then the radiation that was absorbed by that ozone is able to penetrate to lower levels where it is able to create more ozone. This gives considerable stability to the ozone levels in the world.

Much the same story was told from radioactive fallout. The radioactive fallout is a body of very useful information and I hope that people are aware of it and will make use of it where possible. In a figure from Cambray *et al.*, quoted by Peirson, (1970), it can be seen from the radioactive measurements that the northern hemisphere fallout is much more than the southern hemisphere fallout, in a ratio of about 3:1, as we deduced from the ozone measurements. In the period around 1966, the debris was pretty uniformly mixed through the stratosphere and the fallout is indeed diminishing by about 50-60 per cent per year. I hope that people realise that this information is available.

What were people thinking around 1970? In 1969 the Royal Meteorological Society (RMS) held a major meeting on discussions of the global circulation. This is an extract from the paper on The structure and dynamics of the stratosphere (Murgatroyd, 1970); 'The earliest explanation of the general form of the vertical temperature profile by such workers as Gold (1909), Emden (1913) and Gowen (1947) was that the troposphere is broadly in convective equilibrium and the stratosphere is in radiative equilibrium. These conclusions have been broadly confirmed by recent authors (e.g. Goody 1949, Leovy 1964, Manabe *et al.*, 61,64,67)...' I would like to say about the 1949 paper that I knew very well that it was not in accordance with most people's views, and that it would be difficult to get published. Indeed, part of the paper was designed to be acceptable to the referees and the editor. It would have been very easy for the referees to rubbish it, and to say that I had no experience in this field, was out of touch and that the idea was ridiculous because it was widely recognised that the stratosphere was in radiative equilibrium. I thank Reggie Sutcliffe, who was the editor at the time, for it being published. Twenty years later, with a good deal of hindsight, the RMS paper was written. At this time the humidity measurements were beginning to be accep-

ted, and I've talked about the fallout information, so the conclusions of the paper are particularly surprising.

We are now up to 1970, and I had learned that there was a lot of valuable information in the ozone measurements, but the Dobson spectrometer was becoming a problem. The supply had dried up and it was difficult instrument to maintain and required considerable skill to operate. So, I decided to prepare the ground for a new instrument which was to make use of digital methods of measurement. We had started that work when another stratospheric problem reared its ugly head. Concorde was being built and looked like being a success, and it looked, moreover, that it would become a big European symbol. It was receiving a lot of publicity. The Russians entered the race, making their copy which people called Concorde. The proposal was to build 250 Concorde and 250 Concorde. The Americans were not to be left out. Boeing had plans for a bigger and better supersonic transport and they were going to make 500. An unthinkable fraction of the world's total fuel resources would be burnt in the stratosphere. I wasn't worried about the stratosphere, but I think there is a lot to be said for walking and cycling!

The suggestion which started to receive a lot of publicity was that NO_x would be produced by these transports and is incompatible with ozone, that it would catalytically destroy ozone and in the stagnant stratosphere this NO_x would accumulate indefinitely, the ozone would go and we would all die. From what I could see of pollution operations and measurements, NO_x and ozone seemed to be positively commensal and the argument that they were mutually destructive I did not think very sensible. Also, was the pristine stratosphere really free of NO_x? I wondered whether we could settle something about this by remote sensing method which we were working on for the new Dobson spectrometer. We dug out the spectrum of nitrogen dioxide, and at 450nm (in the blue) there was a very striking feature which meant that we could measure the NO₂ between us and the sun. There were three wavelengths quite close together, which gave us a strong curvature of absorption and using methods similar to the Dobson AD method of

calculation we could get the NO_2 with very little interference from other substances. So we made a suitable instrument and took it onto the roof of the physics building in Toronto, where I was at the time. It was quite obvious as soon as we started that the NO_2 in the Toronto atmosphere meant that it was quite impossible to work there. The instrument was quite small. It was considerably smaller than the modern 'Brewer' as they call it. The actual spectrograph went in a light-tight enclosure about the size of a top hat, and looked a bit like one too. I decided to take it to the family cottage, our datcha. There, 100 miles at least from either Toronto or Montreal and almost that from Ottawa and Rochester and lots of countryside and Lake Ontario around, we would be free from pollution. There was a very good horizon as the astronomers call it. During the winter of 1972/3 we made a new instrument taking advantage of all the experience we had so far and it was a very considerable success. Part of the requirement for the Dobson successor was transportability and at weekends and holidays I would dump this thing in the car and drive out to the cottage. So we only got measurements on fine days when I was at the cottage at weekends and in the vacations.

The instruments worked liked a dream. It was a fabulous success, it proved very stable and gave consistent results throughout. It was small, so when we felt we wanted measurements from an aircraft, we could use it as a carry-on device, and we could take it from the cabin of the aircraft looking at the sun through the porthole and we could carry it into the pilot's cockpit and take the zenith sky measurement where there was possibility of seeing the sky. We took the measurements in spring and summer 1973 and we decided we needed some high level measurements because the tropospheric pollution is in the way and is very variable. We got some results of the concentration of nitrogen dioxide over southern Ontario. They surprised us very much and I think most people would disagree with them. The striking thing is the very large diurnal variation which we observed. The sunrise and sunset values were remarkably stable throughout the period I was making measurements. We were surprised they were so steady when all the other daytime

measurements were so variable, and I understand that in fact they show remarkable little variation over the whole globe. They do vary with latitude and season, but not a great deal. Sunrise is always less than sunset, and the noon is very, very variable. I published the results in '73 in a paper with Kerr and McElroy (Brewer *et al.* 1973). The sunrise and sunset figures are quite easily measured by a variety of methods, and are accepted. The noon values are totally rubbished.

The Concorde story proved different from what was expected. A Concordski crashed at the Paris Air Show and the Russians abandoned civilian supersonic entirely. After a lot of effort, Boeing decided that their aircraft wasn't even going to get off the drawing board and they abandoned it. So an important prestige field was left to Concorde. This resulted in a very unholy collaboration between Boeing who did not want Concorde to work, and the Greens who did not want anything to work. I am satisfied that we bounced the eco-warriors and got our Nature paper published before they even knew it was there, and it caused great annoyance - still does.

The students that I was working with, Kerr and McElroy, are still interested in the problem and they recently sent me some NO_2 measurements taken in the northern suburbs of Toronto. They are from two days close together, and yet they show enormously different mid-day peaks. In interpreting these values you need to take into account that the tropospheric values are highly variable, but I am sure the stratospheric values are variable too. However, there is immensely little data on this. Adrian Tuck (Tuck, 1996), in his very valuable Symons memorial lecture makes no mention of NO_x or of NO_2 , which is rather strange given the amount of fuss that was made about it.

In our 1973 paper there is an example of the observations as they were taken from the instrument. When I look back at it, I see that on 23 July 1973 I started work with the sun 6° below the horizon and was still working when it was 6° below the horizon in the evening. Whether it was a weekend or holiday I don't know. The difference between the morning and evening is visible and when the sun is getting high, at 76° zenith angle, the difference is very

clear. The problem is how much is tropospheric and how much is stratospheric.

In the paper we mention the problem of where does the NO_2 go at night. We suggested that it turns into NO_2 . If you put NO_2 and ozone in a bottle in the lab it turns a beautiful green with NO_3 , which has a very nice absorption spectrum. Mervyn Davis came to work with us and made use of this absorption spectrum to measure NO_3 . We found none. Modern theories include the conversion to N_2O_5 , and N_2O_5 necessarily includes NO_3 on the way. If there is no NO_3 , or very little, then I don't think that there is much chance of there ever being much N_2O_5 .

I thought a very great deal about this work recently. I came to the conclusion that my measurements were correct. I am totally satisfied in my own mind that in the stratosphere we get a strong but very variable diurnal variation of the NO_2 content. We may show its nature by plotting the NO_2 against solar elevation (positive and negative). We get a curve rather like the well known hysteresis curve of magnetism. In electronics we sometimes get a similar figure when we produce a Lissajous' figure on an oscilloscope. I think that NO_2 follows this sort of variation but differently every day, and that NO_x offers the potential of being a very useful tracer.

If we are to get any one to accept this we need a coherent explanation of the chemistry. Contemporary accepted chemistry of NO_x , so far as I understand it, seems to me to be very clever; but the effects I observed were very strong and we need a good simple explanation. What does it take to get such a curve? It needs (1) positive feedback (2) storage and (3) non linearity provided by limits or stops.

I think there is a simple theory. I suggest that the NO_x in the stratosphere has no significant components other than NO and NO_2 , that the total ($\text{NO} + \text{NO}_2$) is very variable, and might sometimes be as much as 20 DU. Now it was part of the doctrine of the eco-warriors that there was little NO_x in the native, pristine, stratosphere. I think it is more probable that there was (and is) a lot. Stratospheric air comes from the cold equatorial tropopause where thunderstorms are important; and we know that thunderstorms produce

a lot if NO. NO will also be produced by lightening between thunderstorm tops and the ionosphere.

NO₂, which we measured, is decomposed (photolysed) by radiation from the sun ($\lambda < 300\text{nm}$). These penetrate the atmosphere and always tend to convert NO₂ into NO. Why then do we observe the NO₂ when the sun is high? It need not be complicated chemistry.

The NO₂ occurs in the day through a reaction with atomic oxygen which is being produced by solar UV. It could be through a three body reaction or through a reaction where the excess energy is got rid of by a yellow-green chemiluminescence. This reaction has been shown in the laboratory to occur over a wide range of temperatures and pressures by Gaydon (1946). The suggestion is not entirely stupid. The atomic oxygen is provided by photodecomposition of ozone and also oxygen, it depends totally on the illumination which of course changes from none to full sunlight, and also on the ozone concentrations. Ozone always decompose when it absorbs light. Visible light at 600nm will give a small amount of product so visible light will never get rid of the NO₂ completely. At 300-350nm there is strong absorption and rapid production of O when the sun is high, but as the sun goes down you get very much reduced production. There is also production at 200nm from oxygen. This is the atomic oxygen coming from ozone which produces ozone. If the hypothesis that NO₂ results from

the reaction with atomic oxygen and nitric oxide is right, and if nitric oxide is also very variable, then you have a very complicated picture and it is not surprising that the NO₂ is very variable. So I put it forward as a suggestion that I think that NO_x, which is not discussed by Adrian Tuck in his Symon's memorial lecture, ought to be discussed, and that you need to consider NO + NO₂, nothing more clever. I think there is a lot of useful information there.

Well now it seemed to me that there was a lot of harm being done because NO_x was not getting the attention it should and that it was being treated too much as something we knew everything about. So I put this together in a short letter to the editor of the Quarterly Journal. I was not optimistic that it would get accepted; just as I thought I was lucky to get the '49 paper published. But the referees said that I was out of touch. I think that it is very serious that these ideas are too easily thrown out. I can't do anything about this matter now, but clearly much more research on NO_x is needed.

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25-29 April 2000: EGS General Assembly,

Nice, France

(<http://www.copernicus.org/EGS/egsga/nice00/nice00.htm>).

Session ST2: Open session on the middle atmosphere

Convener: Martin Dameris, DLR Oberpfaffenhofen, Wessling, Germany (martin.dameris@dlr.de).

This year, the open session had two main topics, the variability of the stratosphere, and the assimilation of chemical data. For example, N. Butchart presented results from

the middle atmosphere version of the UKMO Unified Model of two 60 year model runs (1992-2051), assuming a plausible scenario for the future change in the concentrations of well-mixed greenhouse gases. In particular it was shown that changes in the frequency of occurrences of stratospheric warmings was unpredictable because of internally generated decadal variability.

Some new results were presented with respect to the in-situ simulation of the QBO in GCMs. The QBO has been successfully simulated in the UKMO Unified Model employing the Hines gravity wave drag scheme (D. Stainforth). K. Weber studied the influence of the QBO on the polar vortex during northern winter in the Berlin CMAM GCM, by imposing westerly and

easterly winds in the equatorial lower stratosphere. The model results show significant differences at high latitudes in most dynamical parameters. D. Klasen showed that the 4-dimensional variational data assimilation represents a well-suited method to incorporate the mostly synoptic data of all available measurements into a chemistry-transport model. She showed some interesting new results of the CTM COMMA of the application of CRISTA measurements. The assimilation of temperature data from the UARS-MLS instrument into the UKMO assimilation system was also presented. V. Asenek showed results of two model experiments: one incorporated UKMO operational observations in addition to MLS data while the other did not include the MLS data. To maximise the benefits offered by the high resolution of the MLS data, it was necessary to retune some of the assimilation parameters.

As every year, this open session will be held again next year during the XXVI Assembly of EGS.

● Martin Dameris

ST6.02 Stratosphere-troposphere exchange (STE)

Conveners: V. Wirth, Meteorology Institute, Munich, Germany (volkmar.wirth@Meteorologie.Physik.Uni-Muenchen.dbp.de) and Peter H. Haynes, DAMTP, Cambridge, UK (p.h.haynes@damtp.cam.ac.uk)

This session attracted some 30 oral contributions plus 6 posters. Several contributions presented first results from a major EU-funded field campaign APE-THESIO focussing on the tropical tropopause region and other campaigns focussing on the subtropical tropopause and on the upper troposphere/lower stratosphere (UT/LS) region at higher latitudes. We now have better knowledge of the variation of chemical constituents in the region immediately above the extra-tropical tropopause, where the air tends to have a character intermediate between tropospheric and stratospheric.

In quantification of STE the focus seems to have shifted away from computing fluxes across control surfaces such as certain constant-PV surfaces; instead, it seems increasingly

to be based on a Lagrangian framework, for example determining histories of air parcels that arrive to a particular region.

Low latitudes: An overview paper by L. Stefanutti *et al.* summarised the measurements (*in situ* and lidar) taken at the tropical tropopause during APE-THESIO in 1999 (seven flights from two aircraft). The IR lidar identified thin subvisible cirrus in the tropopause region which could not be associated with anvil outflow from local convection. V. Santacesaria *et al.* tried to infer the cloud formation mechanism at the tropical tropopause. Apparently, the (relatively low) height and the (relatively high) temperature of the convective clouds during APE-THESIO are not consistent with a local freeze-drying mechanism in the light of the observed dryness of the tropopause region. However, thin cirrus clouds observed above the top of the convective clouds could be related to buoyancy waves suggesting that such waves play a role in drying the tropical UT. R. Scott and J.-P. Cammas related observations of high ozone in the tropical UT to a wave breaking event at the sub-tropical jet by applying the technique of contour advection with surgery to a particular case.

G. Ancellet *et al.* analysed ozone sonde and ozone lidar profiles taken in the subtropics; although tropopause folds are often observed and can be considered as the generic mechanism for the generation of ozone filaments in the subtropics similar as in mid-latitudes, there may be cases in which vertical motions induced by nearby synoptic systems (like tropical cyclones) considerably modify the ozone profiles in the UT. B. Hassler and M. McIntyre showed that in a simple model, which includes the tropical QBO, the Brewer-Dobson meridional circulation may have multiple equilibria; they found strong sensitivities and pointed out the need to investigate the issue with a more realistic model. B. Legras *et al.* reported ozone observations taken in July 1998, featuring a persistent, almost planetary scale tropopause fold associated with the SH subtropical jet. Analysis of upper level convergence and divergence fields suggests that this giant fold was associated with the descent branch of the Hadley circulation during the Asian monsoon.

The degree to which this is a climatological feature has yet to be determined.

Extratropics: A. Tuck reviewed both historic and modern aircraft *in situ* measurements of wind, temperature, and tracers in the tropical tropopause region. The data reveal scale invariance over a large range of scales implying power law scaling and probability density functions with long tails. The underlying mechanisms leading to this behaviour are not yet fully understood. A. Zahn analysed ozone and CO measurements taken in the UT in the context of the CARIBIC project. As a major result he noted that the ozone/CO relationship could be used as an objective definition of the tropopause, but that the strong seasonality of the relationship needed to be taken into account. H. Gouget analysed data from the TROPOZ II campaign to study STE in the SH; by combining observations with backtrajectory calculations he identified different classes of events, one of which was interpreted as mixing between stratospheric and tropospheric air. C. Kiemle showed that airborne DIAL lidar measurements of UT water vapour taken during the MAP campaign in autumn 1999 can provide realistic high resolution sections of water vapour in the tropopause region. The small-scale structure detected on those sections is broadly consistent with mesoscale modelling results. T. Wetter analysed high-precision tracer observations from the tropopause region finding a substantial amount of variability on tracer-tracer correlation plots; this was interpreted as a result of mixing associated with deep convection.

A. Dethof discussed contour advection analysis of isentropic water vapour transport from tropical UT to extratropical ST, particularly in association with the Asian summer monsoon. The transport appeared to be larger in 1998 (which was an El Niño year) than in 1997. P. Hoor showed chemical observations from the STREAM and POLSTAR campaigns. Correlation between different species indicated a distinct transition layer above the high-latitude tropopause. B. Schaefer showed LS water vapour measurements from the CRISTA satellite instrument and simultaneous aircraft. J. Kowol-Santen showed results from numerical simulations in a high vertical

resolution mesoscale model of folds and intrusions near the subtropical jet. V. Wirth described a study, based on idealised dynamical models, of differences in tropopause structure between cyclones and anticyclones, with a view to explaining earlier studies of such differences. M. Juckes discussed different mathematical formulations for mean transport and implications for transport across the tropopause; this transport defined by a constant potential vorticity surface would naturally be from troposphere to stratosphere in high latitudes, consistent with earlier observational studies.

H. Wernli argued that deep exchange events, where air moved from LS to UT or vice versa, were particularly significant, e.g. for affecting chemical distributions; in the NH, such events were most common in the storm-track region in the western parts of the Atlantic and Pacific oceans. P. Konopka described a new modelling approach based on parcels on an irregular grid, with mixing between nearest neighbours allowed on a criterion based on local deformation rate of horizontal wind. M. Bourqui analysed Lagrangian behaviour in mesoscale-model simulations of baroclinic disturbances and used this as a basis for quantification of exchange. S. McCaffrey described the NARE campaign chemical observations in the UT and model simulations to investigate the bringing together of stratospheric and boundary-layer air masses as suggested by the observations. M. Greenslade discussed quasi-geostrophic baroclinic channel simulations in which distinct transport regimes were seen at upper and lower levels and argued that the transition between these regimes was a useful paradigm for the extratropical tropopause. O. Morgenstern investigated tracer transport in the wintertime subtropical tropopause region using effective diffusivity. The same quantity, namely effective diffusivity, was considered by P. Haynes and E. Shuckburgh in order to suggest an alternative definition of the tropopause. They argued that a minimum in effective diffusivity, when existing, is a more fundamental and relevant definition of the tropopause than any specific PV surface (which is the definition often used).

Global: A. Stohl presented a Lagrangian climatology of NH

transport in which he identified various classes that were significant for STE. A. Bratseth discussed transport circulations and diffusivities for the troposphere and LS calculated from particle integrations based on ECMWF reanalysis data. C. Reithmeier described a new on-line transport scheme for the ECHAM4 GCM and gave results for a number of passive tracer experiments. G.-J. Roelofs used a 3D fully coupled chemistry climate model to investigate the different sources of ozone in the troposphere; the seasonality of the ozone transport from the stratosphere turns out to play a significant role. B. Bregman gave a summary of the TOPOZ-2 results in which a simplified ozone was used as a basis for intercomparison of different chemical transport models. B. Saendig described variations in the transport and mixing of tracers in a simplified GCM as the planetary wave forcing was varied.

Posters: Posters were presented both on measurements and on model-based transport studies in the tropopause region. Somewhat aside from the main focus of the STE session, but nonetheless very interesting, was a poster by K. Turnbull *et al.* on the development of a frost-point hygrometer with considerably increased sensitivity and much reduced response time; laboratory experiments and a balloon flight demonstrated its potential for accurate high resolution measurements in the tropopause region.

● V. Wirth and P. H. Haynes

ST6.03 The impact of aviation upon the atmosphere

The sub-session covered 12 oral presentations (including 1 solicited paper) and 4 posters. Only 2 announced papers were not present. The audience was over 100, which led to lively discussions.

Kelder presented an overview on the topic, with emphasis on the results of the recent IPCC Special Report 'Aviation and the Global Atmosphere'. Three papers covered in situ observations: Wohlf from (massive chemi-ions in the wake of aircraft), Wohlf from (sulphuric acid in the wake) and Hagen (particles from rocket emissions). Palikonda

and Meyer, both studied satellite observations of contrails. Nine papers on modelling aircraft effects were presented: on plume effects (Sorokin), the impact of bromine chemistry (Hendricks), parameterising NO_x from lightning (Brunner, Meijer), 3D modelling of the perturbation of atmospheric chemistry due to aviation (Berntsen, Jourdain, Hauglustaine), the impact of climate-chemistry interaction on the response to supersonic aircraft emissions (Pitari), and on the impact of aircraft powered by liquid hydrogen (Sausen). Finally, Lee discussed problems arising from controlling only aviation carbon dioxide.

● Robert Sausen
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Session ST7: Atmospheric chemistry-climate interaction

This session was organised for the first time during an EGS-meeting. It was very well attended. At the beginning, D. Shindell gave an overview concerning global water, ozone recovery, and increasing stratospheric water. He showed that upper stratospheric temperatures indicate large decreases extending back several decades. Only by including an increase in stratospheric water vapour in climate models can these trends be reproduced. The required water vapour trend is too large to be accounted for solely by increased production within the stratosphere, suggesting that climate change may be altering input from the troposphere. A second interesting topic was the comparison of interactively coupled chemistry-climate model results. Both, a middle atmosphere version of the ECHAM/CHEM model as well as a high vertical resolution version with an upper boundary near 10 hPa indicate a general cooling of the stratosphere caused by the increased greenhouse gas concentrations, except in the northern hemisphere in late winter/early spring. This could be an indication for a general change of wave activity in this region due to a warmer troposphere (greenhouse effect). D. Hartmann's analysis of observations suggest a

trend in annular modes of variability in both the surface and stratospheric climate over the past 30 years. These trends may result from a combination of changes in the chemistry of the stratosphere and increasing greenhouse gases. A further analysis of coupled models should give a clearer picture in the near future. The future development of the ozone layer (2015) was estimated using an interactively coupled chemistry climate GCM (ECHAM4.L39(DLR)/CHEM). First results indicate that in the southern hemisphere the recovery of the ozone layer is clearly delayed due to chemistry-climate interaction yielding an additional decrease of stratospheric ozone. In the northern hemisphere the recovery of ozone above 20 km is also delayed whereas a significant increase of ozone is found from the surface up to the lowermost stratosphere caused by enhanced NO_x emissions, resulting in an increase of the total ozone column.

This session will be held again in two years, during the EGS 2002.

● **Martin Dameris**

Session OA24: Tropospheric aerosols: formation and heterogeneous chemistry

Conveners: Markus Ammann, PSI, Villigen, Switzerland (markus.ammann@psi.ch); Christian George, LACE, Lyon, France (Christian.George@univ-lyon1.fr); Thorsten Hoffmann, ISAS, Germany (hoffmann@isas-dortmund.de).

This session attracted about 65 presentations focussed on heterogeneous or multiphase processes on solid or in liquid aerosols that are potentially important in determining the composition of the troposphere or affecting climate.

Emphasis has been given to four different topics which are: organic aerosols (especially formation from biogenic hydrocarbon oxidation), ageing processes of aerosols in the atmosphere and impact on their reactivity, heterogeneous chemistry on carbonaceous aerosols and finally cloud chemistry.

The session started with an overview lecture on the formation of

secondary organic aerosols (SOA) given by R. Kamens from the University of North Carolina (USA). Special attention was given to model simulations showing that reasonable predictions of secondary aerosol formation are possible from both dark ozone and light/NO_x reactions, exemplified for the α -pinene system. However, it was also underlined that there is little information on the quantum yields for product aldehydes which "drive" the reactivity of the NO_x-light reactions. A number of other presentations also dealt with the modelling of SOA formation and the reactivity of secondary aerosols, which highlighted the importance of secondary aerosols from biogenic origin but also included studies on air quality related topics.

Aerosol modelling and characterisation requires careful analysis of the history of the particles. This point was addressed by U. Baltensperger (Paul Scherrer Institute, Switzerland) who in another invited talk focused on the modification of aerosol particle characteristics due to ageing processes in the atmosphere. This is an important issue because heterogeneous reactions have the potential to influence the gas phase chemistry of the atmosphere but also to modify the characteristics of the particles themselves. This is the case, for example, for soot particles for which adsorption or surface chemical processes are expected to drastically change the hygroscopicity by increasing the amount of water soluble material which may result in the incorporation of the particles in cloud droplets. This overview was followed by several contributions on the characterisation and reactivity of aerosol particles from different parts of the troposphere and addressing organic acids, mineral dust and sea-salt. One important topic in this session was the reactivity of soot particles. H. Saathoff (Forschungszentrum Karlsruhe, Germany) in his invited lecture, presented the AIDA soot aerosol campaign in October 1999: dynamics and optical properties of pure and mixed aerosols. This campaign with 8 German, Swiss and Austrian institutes participating was organised in October 1999 at the 84 m³ large aerosol chamber facility AIDA. The goal of the campaign was a comprehensive physical and

chemical characterisation of spark generated soot (a laboratory soot source widely used in aerosol research) in comparison with real Diesel soot from a commercial engine, of ammonium sulphate aerosol, of secondary organic aerosol, and of their mixtures, as a function of ageing time. A number of other talks then presented results using novel single particle analysis instruments in field and laboratory studies, and also addressed the surface reactivity of organic compounds adsorbed on soot particles. It seems that the latter are the driving force of the particle reactivity.

Finally, the session also addressed the potential impact of cloud chemistry. This complex topic was introduced by H. Herrmann (Institute For Tropospheric research, Germany) with results from field, laboratory and modelling studies of tropospheric aerosol and cloud chemistry. This contribution and the following presentations addressed the relations between gas-phase and condensed phase chemistry (especially via OH-forming and NO₃ reactions), the effects of cloud processing of aerosol particles and the microphysical effects of organic components. Especially the latter topic highlighted the need for a better understanding of multiphase organic chemistry.

Regarding the quality and quantity of contributions and the significant number of participants, it was decided to hold a similar session at the forthcoming EGS general assembly in Nice in 2001.

● **Christian George**

Future SPARC and SPARC-related Meetings

- 2-6 October 2000: First S-Ramp Conference**, Sapporo, Japan (<http://www.kurasc.kyoto-u.ac.jp/s-ramp>).
- S17. Middle Atmosphere Including Response to Forcing from Above and Below** (M. Geller and T. Tsuda) (dynamics, chemistry, tropospheric wave forcing, response to Sun and energetic particle effects, observations, modelling, laboratory results, chemistry/radiation/dynamics coupling).
- S18. Solar Variability Effects Upon the Lower Atmosphere and Climate** (L. Hood and J. Austin) (External energy flux variability, its effects on middle and lower atmosphere, models to simulate this effects and identify mechanisms, 27-day and 11-year period effects, interaction with QBO).
- 6-10 November 2000: 2nd SPARC General Assembly "SPARC 2000"**, Mar del Plata, Argentina. Chair of the SOC: Alan O'Neill. Chair of the LOC: Pablo Canziani. (<http://www.sparc2000.at.fcen.uba.ar/>) and (<http://www.aero.jussieu.fr/~sparc/>)
1. Stratospheric Processes and Their Role in Climate
 2. Stratospheric Indicators of Climate Change
 3. Modelling and Diagnosis of Stratospheric Effects on Climate
 4. UV Observations and Modelling
- 15-19 December 2000: American Geophysical Union (AGU) 2000 Fall Meeting**, San Francisco, California, USA (meetinginfo@agu.org)
- 14-19 January: 81st American Meteorological Society (AMS) Annual Meeting; A Millenium Symposium on Atmospheric Chemistry: Past, Present and Future of Atmospheric Chemistry; Climate Variability, the Oceans and Social Impacts; 12th Symposium on Global Change Studies; 11th Symposium on Meteorological Observations and Instrumentation; Etc.**; Albuquerque, New Mexico, USA. E-mail: amsmtgs@ametsoc.org
- 19-22 March 2001: Workshop on Nitrogen Oxides in the Lower Stratosphere and Upper Troposphere**, Heidelberg, Germany (<http://www.ozone-sec.ch.cam.ac.uk>).
- 26-30 March 2001: EGS XXVI General Assembly**, Nice, France: (<http://www.mpa.gwdg.de/EGS/egsga/nice01/nice01.htm>).
- 25-29 June 2001: International Workshop on Trends in the Atmosphere (IAGA/ICMA/SPARC)**, Prague, Czech Republic: Convener: Jan Lastovicka (jla@ufa.cas.cz)
- 10-18 July 2001: IAMAS 2001**, Innsbruck, Austria (<http://iamas.org>).
- Symposium "Middle Atmosphere Dynamics", 7 days, Lead Convener: K. Hamilton, Co-conveners: T. Hirooka, B. Lawrence, F.-J. Luebken, L. Hood (turbulence, GW, planetary waves, tides, forcing of mean circulation, long-term variability, mesospheric layered phenomena, MA aspects of climate change)
 - Symposium "MA Chemistry, Transport and Radiation", 6 days, Lead Convener: A.R. Ravishankara, Co-conveners: N. Harris, V. Fomichev, D.W. Waugh (observations, models, transport relevant to understanding chemistry, aero sol microphysics, MA climate-chemistry interactions, energy budget of the MLT-layer)
 - Workshop "Solar Cycle Forcing of the Stratosphere and Troposphere", 1 day. Convener: L. Hood (possible mechanisms: solar UV variability, energetic particle precipitation, etc.).
- 18-30 August 2001: IAGA-IASPEI 2001 Joint Scientific Assembly**, Hanoi, Vietnam: (<http://www.IAGAandIASPEI.org.vn/pta.htm>).
- 20-24 August 2001: Climate Conference 2001 (IGBP/WCRP/IHDP)**, Utrecht, The Netherlands (cc2001@phys.uu.nl)

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Published by Météo-France - Direction
commerciale et de la communication
ISSN 1245-4680 - Dépôt légal 1^{er} trimestre 2000