Lawrence Livermore Laboratory

SOURCES AND SINKS OF STRATOSPHERIC WATER VAPOR

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MASTER

November 1979

This paper was prepared as an invited paper for presentation at, and inclusion in the Proceedings of the NATO Advanced Study Institute on Stratospheric Ozone, Aldeia das Acoteias, Albufeira, Algarve, Portugal, October 1-13, 1979.

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Abstract

Herein is presented a tutorial review of our understanding of stratospheric H_2O and the processes controlling it. Paradoxes posed by currently available observational data are cited and suggestions made as to how they might be resolved.

Such resolution appears to require:

- 1) That the bulk of our current data provides unrepresentative and misleading vertical and latitudinal H_2O gradients immediately downstream from the tropical tropopause.
 - 2) That there exist within the troposphere a mechanism different from or in addition to the tropical tropopause cold trap for drying air to the mixing ratios found in the lower stratosphere.
- Satisfaction of these requirements will reconcile much heretofore puzzling observational data and will obviate the necessity for a stratospheric sink for H_2O .

INTRODUCTION: When the Meteorological Research Flights (MRF-see Cluley and Oliver [1978]) for measuring stratospheric humidity with a frost point hydrometer began in the 1940's, they found H_2O mixing ratios of 2 to 3 ppm corresponding to relative humidities of only a few percent. As this extreme aridity was confirmed by additional flights, Brewer and Dobson [Dobson et al., 1946; Brewer, 1949] hypothesized that all air entering the stratosphere did so via the tropical tropopause where it was freeze dried sufficiently to produce the low mixing ratios consistently observed in the lower stratosphere. In the polar regions where tropopause temperatures were warm enough to permit diffusive upward transport of air of much greater moisture content, such transport was prevented by the subsidence of the return Hadley flow.

Others tried by a variety of techniques to measure the humidity of the stratosphere. The results were widely varying and gave rise to a controversy between those who defended a "dry" stratosphere and those who claimed that the mixing ratio increased significantly with height. This controversy was summarized by Gutnick [1961] in a paper entitled "How dry is the sky?" Sissenwine et al., [1972] wrote a sequel entitled "How dry is the sky?"

* This report contains more detail than could be presented orally and some of the new ideas have been developed beyond the stage they had reached at the time of presentation.

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A decade later and the SST.* As you will note below, this controversy is still not laid to rest. Due to the tremendous drop in H_2O mixing ratio between the surface and the stratosphere, balloon lofted platforms and instruments provided many opportunities for contamination. This became very obvious when data recorded on descent were compared with that recorded during the ascent portion of a flight; mixing ratios were generally much lower on descent and the difference increased with altitude [Mastenbrook, 1965]. The contamination problem was also discussed by Zander [1966, 1973].

In the early 1960's Mastenbrook, [1968, 1971, 1974a, b] began a series of balloon soundings with a frost point hygrometer, using data only from the descent portions of the flights. These included two soundings from Thule, a year of soundings from Trinidad and a continuing series from Washington, D.C. These gave a median mixing ratio of ~ 2.6 ppmv, confirming the low humidities found by the MRF and showing remarkably little variation with latitude, altitude and flight to flight. Most other observational programs consisted of short data records and frequent modifications in equipment and/or technique which didn't allow accumulation of sufficient homogeneous data to permit estimation of repeatability and reliability. Both the MRF and the Mastenbrook flights, on the other hand, have been sufficiently extensive and consistent as to more or less gain acceptance as a standard against which other soundings are judged. For these reasons and because their observations were consistent with the Brewer-Dobson hypothesis for the consistent aridity of the stratosphere, observations of stratospheric H_2O departing significantly from the mean Mastenbrook profile have in general been disregarded as at least unsupported.

If we accept the Brewer-Dobson hypothesis, then all air entering the stratosphere should be freeze dried to the saturation mixing ratio of the tropical tropopause. This has generally been assumed to have an average temperature of $-80^\circ C$ and an average pressure of 100 mb - corresponding to a saturation mixing ratio of ~ 3.4 ppmv. However, when I attempted to develop an H_2O budget for the stratosphere for CIAP [Elsaesser, 1974], I was immediately struck by the question; how can the lower stratosphere maintain a mixing ratio of ~ 2.6 ppmv if all the air entering through the tropical tropopause is freeze dried to only 3.4 ppmv? This appeared to require a stratospheric sink for H_2O .

At that time, other than the return flow of the Hadley circulation (which presumably consisted of air with a mixing ratio of ~ 2.6 ppmv), the only H_2O sinks which had been proposed were planetary escape of hydrogen [Brinkman, 1969] and the H_2O incorporated into the H_2SO_4 particles of the Junge layer [Grans and Flococo, 1967]. These were too small by 4 and 2 orders of magnitude respectively. It occurred to me that the low temperatures developing around 25 to 30 km in the winter polar vortices might be cold enough to form ice crystals or cirrus cloud which might precipitate into the troposphere before reevaporation. A paper by Newell called my attention to the fact that stratospheric cirrostratus had been observed during at least 3 separate winters in Antarctica [Liljequist, 1956]. At about this time I learned that Stanford [1973] had independently proposed that the Antarctic winter vortex might act as a freeze-dry sink for the stratosphere.

Figures 1, 2, and 3 show meridional profiles of winter temperature for the northern and southern hemispheres and the annual cycles of temperature at the 100- and 50-mb levels. The temperature distributions shown in these Figs are consistent with saturation mixing ratios ≤ 10 ppmv from 22 to 30 km over the Arctic pole and ≤ 3 ppmv from 16 to 24 km over the Antarctic pole. These are only marginally of the required coldness for an effective sink even over Antarctica. The two latitudinal profiles of stratospheric H_2O available in 1974 (curves of McKinnon and Morewood [1970] and Kuhn and Stearns [1973] of Fig. 8) indicated a maximum near the equator and a minimum at the southernmost observation point.

I pointed out that these data were consistent with an Antarctic winter freeze-dry sink sufficient to remove the approximately 3×10^8 tonnes H_2O/yr apparently required to balance the stratospheric H_2O budget. No one, including myself, has been particularly happy with this proposal. But neither has any one yet come up with a more satisfactory resolution of this apparent paradox.

But this is not the end of our paradox. By current understanding air entering the stratosphere contains ~ 1.6 ppmv of CH_4 which, if completely oxidized would add 2 ppmv to

the H_2O mixing ratio of the stratosphere and some 1.8×10^8 tonnes H_2O/yr to the stratospheric budget. (This number is the amount of H_2O which could be produced by the flux of CH_4 across the tropopause computed by a recent version of the LLL 1-D model.) This makes a stratospheric sink for H_2O even more imperative. In addition it raises the questions: 1) When H_2O soundings show no increase in stratospheric mixing ratio with height, what is happening to the H_2O from CH_4 oxidation? 2) When H_2O soundings show an increase in stratospheric mixing ratio with height of more than 2 ppm, where is the extra H_2O coming from? Present data and understanding would appear to suggest that observations reporting stratospheric and mesospheric mixing ratios outside the range of about 1-2 to 6-8 ppm cannot currently be explained and thus suggest the observation should be questioned.

THE HADLEY CIRCULATION: Now that I have given a brief overview of the problem, let's take a look at the meridional circulation of the atmosphere. Originally the meridional Hadley circulation was described in terms of its annual mean; i.e., two symmetric cells on either side of the equator with converging equatorward surface currents (the northeast and southeast trade winds) merging at the equator into one upward current which then splits into two diverging poleward currents aloft. When enough observational data had accumulated it became clear that this circulation pattern varied seasonally by following the sun and undergoing antisymmetric changes in intensity. As the cell of the winter hemisphere advances across the equator into the summer hemisphere it grows in size and intensity while its twin in the summer hemisphere shrinks and weakens as it moves farther away from the equator.

Figs. 4 and 5 show the Jun-Aug and Dec-Feb extreme conditions of asymmetry in which the cell of the winter hemisphere sits over the equator dominating the low latitude circulation and the cell of the summer hemisphere is displaced up to 30° latitude from the equator and is so shrunken and weakened to be barely discernible. As indicated in Figs. 4 and 5, the Hadley circulation extends into the stratosphere and appears to provide all the flux of air and contained tracers from the troposphere into the stratosphere which makes up the Brewer-Dobson circulation. Note also that the Hadley circulation tends to transport air from the troposphere of the summer hemisphere to the stratosphere of the opposite or winter hemisphere.

There are additional features of the Hadley circulation which are important for understanding tropospheric to stratospheric transport. In the tropics, tropospheric profiles of equivalent potential temperature are concave with broad minima at mid tropospheric levels. This indicates both convective instability in the lower tropical troposphere with static stability above and the fact that only air parcels from near the surface have sufficient energy to rise convectively all the way to the tropical tropopause. This and other evidence has convinced most meteorologists that within the troposphere at least the upward Hadley flux is confined to the "hot towers" of cumulus convection. Some, including myself, have at times expressed the belief that the upward Hadley flux into the stratosphere is also confined to cumulonimbus clouds penetrating the tropical tropopause.

There is a hemispheric asymmetry in the Hadley circulation and the tropical tropopause which is not very apparent in Figs. 4 and 5. Most investigators (e.g., Reed and Vilek, [1969] and Dutsch [1973]) have reported that the northern hemisphere Hadley cell is 2 or more times stronger than the southern cell. The data of Newell et al., [1972] (used to prepare Figs. 4 and 5) is the only set known to me which does not show this asymmetry and, in my opinion, is unrepresentative in this respect. Part of the support for this view is the annual variation of the tropical tropopause which is highest and coldest during Dec-Feb when the northern hemisphere Hadley cell is at its maximum strength. The annual variation of the northern hemisphere tropopause is shown in Fig. 6. Both satellite (Kennedy and Nordberg, 1976) and radiosonde (Reed and Vilek, 1969) data show that the tropical tropopauses of both hemispheres vary in phase and are highest and coldest (~ 40 mb, $-84^\circ C$) in Dec-Feb and lowest and warmest (~ 100 mb, $-80^\circ C$) in Jun-Aug. It was this in-phase variation which first suggested to me that the Hadley flux to the stratosphere was probably not confined to tropopause penetrations by cumulonimbi. It seems logical that the height of the tropical tropopause is related to the strength of the Hadley flux through it. If this flux is confined to cumulonimbus penetrations then it would appear,

to follow that the ITCZ should be strongest and produce the most precipitation when it is in the southern hemisphere, i.e. during Dec-Feb. The available data rather strongly imply that the ITCZ is strongest and produces the most visible cloud and precipitation in Jun-Aug when it is in the northern hemisphere. This is not conclusive, of course, since the convection could be confined to fewer but deeper cells in the southern hemisphere, but why it should behave differently in this respect is not at all clear.

The strength of the Hadley flux through the tropopause has been estimated from atmospheric models, from vertical velocities computed from radiosonde data, from vertical velocities required to balance radiational heating of the tropical tropopause, from the mass of the stratosphere divided by its mean residence time and from the 6 month change in mass of the stratosphere indicated by the tropopause positions in Fig. 6. The latter value applies only to the northern hemisphere, of course. The derived estimates range from 1.5 to 9.2×10^{14} tonnes/yr [Eilsaesser, 1974; Robinson, 1978]. The preferred estimate, based on a stratospheric mass of 6.7×10^{14} tonnes/yr divided by a mass weighted mean stratospheric residence time of 2 years, is 3.3×10^{14} tonnes/yr [Eilsaesser, 1974]. The hemispheric asymmetries in the Hadley cells, tropical tropopauses, stratospheric ozone [London, 1975] and tropospheric tritium [Schell et al., 1974] all suggest that this flux is not evenly divided and that significantly more than half of it returns to the troposphere of the northern hemisphere.

THE H₂O BUDGET OF THE STRATOSPHERE: The above flux rate corresponds to a mean vertical velocity spanning 22.5° of latitude on either side of the equator of 0.03 cm/sec. If this flux of air enters the stratosphere with a mixing ratio of 3.4 ppmv and leaves with a mixing ratio of 2.6 ppmv, we have 11.3×10^8 tonnes H₂O/yr entering the stratosphere and 8.6×10^8 tonnes/yr returning to the troposphere leaving 2.7×10^8 tonnes/yr unaccounted for. And we have not yet accounted for the H₂O presumably generated by oxidation of CH₄ within the stratosphere. According to a recent calculation by the LLJ 1-D model this produces another 1.8×10^8 tonnes H₂O/yr making a total of 4.5×10^8 tonnes/yr which apparently vanishes without a trace.

THE OBSERVATIONS: Now let's take a look at the observations of H₂O in the stratosphere. Figs. 7, 8 and 9 show summaries of observations of stratospheric H₂O mixing ratio. The observations taken prior to 1976 have been reviewed by Mastenbrook [1974b], Harries [1976] and Penndorf [1978], all of whom arrived at conclusions quite similar to the following:

- a. The data support a northern mid-latitude constant mixing ratio from the tropopause to about 30 km with a mean value (Circa 1968) of 2.6 ± 0.6 ppmv with some evidence of an increase at higher levels. While the data fairly consistently indicate a slight decrease above the tropopause to a minimum around 19-20 km and an increase above this level, it is difficult to argue that the instrumental accuracy is sufficient to resolve such structure.
- b. While nearly every relevant data set suggests latitudinal gradients with a maximum near the equator there remains the fact that most soundings, even in the tropics, reveal a decrease in mixing ratio in the first km or so above the tropopause. Due to the greater height of the tropopause in the tropics and the ceiling of most aircraft, observations near the equator may simply reflect this vertical gradient or may actually represent some tropospheric air.
- c. All relevant northern hemisphere data sets indicate or support an annual cycle below 20 km, amounting to 10 to 15% in the lower stratosphere and decreasing with altitude. In mid-latitudes the maximum occurs around November at the base of the stratosphere and lags with altitude. Thus the cycle and phase are consistent with a maximum influx of H₂O in late summer at the time of the lowest and warmest tropical tropopause. However, Hyson [1978] failed to find an annual cycle over Longreach, Australia from 1973 to 1976.
- d. The MRF data reevaluated by Cluley and Oliver [1978] and the Mastenbrook series over Washington, D.C. (see Penndorf [1978]) support an upward trend from 1954 to 1973 of $\sim 0.8\%$ ppmv/decade and a steeper decrease thereafter about twice as fast. See Fig. 9.

There is additional evidence of long period cycles in stratospheric H₂O. Fig. 10 shows the year to year distribution of northern hemisphere sightings of nacreous clouds as compiled by Stanford and Davis [1974]. Nacreous clouds occur in high latitudes in winter at altitudes of 20 to 30 km. They are presumed to be due to condensed ice crystals

resulting from local cooling to the ambient frost-point. The 26-year gap in the sightings from 1895 to 1923, confirmed by Stormer [1929], appears to suggest long period cycles in stratospheric mixing ratios exceeding anything that might have occurred since observations were begun in the 1940's.

A summary of recent soundings of stratospheric and mesospheric H_2O appears in Figs. 11 and 12. These represent new instruments and techniques and some feel that the fairly consistent suggestion of an increase in mixing ratio with height above 20 km may be real. If the trend suggested by Fig. 9 is correct, the recent drop in stratospheric mixing ratio should make any contribution from oxidation of CH_4 more apparent since 1975 than at anytime since the beginning of the Mastenbrook series. On the other hand it should be noted that the temporal and spatial variability suggested by Figs. 11 and 12 is well outside anything we would expect from the Mastenbrook series or our current understanding of the stratosphere. Several of the soundings show increases with height greater than the maximum 2 ppm expected from complete oxidation of CH_4 . Accordingly it appears prudent to echo Gutnick's [1961] comment of nearly 20 years ago: "lack of a tenable explanation of the various conflicting reports on stratospheric humidity casts doubt on the accuracy of part or all of the data." It should also be noted that the Jan. 1979 sounding reported at this institute by D. Kley, aside from fine-scale variations, only increased from ~2.3 ppm at 12 km to ~2.6 ppm at 32 km.

DISCUSSION: How do we reconcile the apparent inconsistencies in the H_2O budget of the stratosphere? To fit the available data we need to increase the sinks or decrease the sources. The sinks so far proposed include: the "cold finger" of overshooting thunderstorms of Johnston and Solomon [1979]; Antarctic winter freeze-out of Stanford [1973] and Ellsaesser [1974]; the Junge layer of H_2SO_4 particles; H_2O bound in BrO_3 and HCl and planetary escape of hydrogen. The more certain of these are too small by 2 or more orders of magnitude and both overshooting thunderstorms and Antarctic winter freeze-out appear highly uncertain. But can we reduce the sources? I believe we can. Other than the increases with altitude above 20 km which frequently exceed CH_4 oxidation, the principal sources of our difficulty have been the apparent poleward and upward directed H_2O gradients immediately downstream from the major source - the tropical tropopause. But remember that all the observations suggesting these gradients were taken over continents. In fact, most of them were taken near the downwind boundaries of continents. If we make the quite reasonable assumption that H_2O carried to the stratosphere by penetrations of the tropical tropopause by convective clouds will be concentrated in such locations, then the gradients we are seeing would be due to essentially point sources, i.e. they would not be representative of zonal mean gradients as they have been interpreted to be.

If these gradients do disappear in valid zonal mean data, the requirement for a stratospheric sink is reduced to that required to account for the H_2O from oxidation of CH_4 , i.e. approximately 1.8×10^8 tonnes/yr. Here again a resolution may be in sight. With a yearly troposphere-to-stratosphere Hadley flux of 3.3×10^{14} tonnes of air/yr, an excess in mixing ratio of only 0.55 ppm in the return flow over that in the entering flow would be sufficient to balance the budget. As pointed out by Cluley and Oliver [1978] and Penndorf [1978], and as is clearly evident in Fig. 9, the MRP measurements over the British Isles are consistently ~0.5 ppm higher than those of Mastenbrook over Washington, D.C. Penndorf [1978] noted that this difference appears to exceed the estimated systematic errors of 8-10% and may be real. That the difference should be so near the required magnitude may indeed be fortuitous! However, the equatorward gradient is supported by the northern hemisphere data of McKinnon and Morewood [1970] shown in Fig. 9. This solution to our problem would also vindicate Kley's [1979] proposal that above the "hydropause" (level of minimum mixing ratio near 60 mb) there is an equatorward directed gradient and that above this level the downwelling return Hadley flow in high latitudes carries more H_2O than the low latitude upwelling branch of the Hadley flow.

At present the above proposals can only be regarded as ad hoc solutions designed to fit specially selected data. For example the last one would require that Harries' [1973] data in Fig. 9 be in error (or unrepresentative) by a factor of 2 and that all of the profiles in Fig. 8 (except McKinnon and Morewood's [1970]) be in error (or unrepresentative) and in

particular that the latitudinal gradient implied by Mastenbrook's soundings from Trinidad, Washington, D.C. and Tule also be in error (or unrepresentative). While such a circumstance is clearly possible for each individual data set, to expect all of them to be off in the same sense is asking a lot. But the above proposals do point a way out of our current paradoxes and establish the types of observational data required for their confirmation or refutation. Thus they should advance our understanding of the stratosphere if only by the elimination of some of the currently available possibilities.

However, I believe we have an additional problem. As Robinson [1978] recently pointed out, if we visualize the Hadley circulation as a large scale mean motion, then the tropical updraft should give rise to a widespread layer of thick cloud as the tropospheric air is being freeze-dried in passing upward to and through the tropical tropopause. Such clouds are rarely if ever observed. In fact, Robinson [1978] noted that most soundings (those on so-called 'dry' days) show, even in the tropics, that "humidity at points as much as 100 mb below the tropopause is less than saturation humidity at [the overlying] tropopause temperature. On the 'wet' days humidity corresponds to saturation at [the] tropopause temperature, about 6×10^{-6} and is reasonably constant up to pressures as much as 60 mb below the tropopause pressure." Robinson [1978] concluded that Kuhn's data from the NASA 1977 ITCZ Experiment [Poppoff et al., 1979] appear to imply stratospheric injection by a structure made up of relatively humid (~ 6 ppmv) and dry (~ 3 ppmv) air columns in close juxtaposition extending over the pressure range 200 to 60 mb. The soundings on the 'wet' days appear to conform to the requirement for freeze-drying to the tropopause temperature in the deep cloud of cumulus towers and are surrounded by descending air with mixing ratio typical of the stratosphere, i.e. ~ 3 ppmv. He observed: "Most of the tropical observations, if considered from the point of view of humidity alone, suggest that stratospheric air is entering the troposphere in the tropics, but other tracers, particularly ozone, show no evidence of this."

From Robinson's [1978] observations it would be tempting to conclude that all air enters the stratosphere through cumulus penetrations of the tropical tropopause and that these cumuli are surrounded by subsiding air producing a dry layer sometimes extending from the tropopause down to 200 mb. However, if this were true and the mixing ratios conformed to those so far reported in the vicinity of tropopause penetrations we would still need a stratospheric sink to get rid of the excess water. An even stronger objection is the apparent rarity of tropopause penetrations. Holton, Director of the NASA 1977 ITCZ Experiment, strongly supported the belief that the entire upward flux of the Hadley cell is concentrated in cumulonimbus towers in the troposphere. But with regard to the stratosphere he wrote [Poppoff et al., 1979]: "However, observations indicate that relatively few tropical cumulonimbus actually penetrate the tropopause. Very few clouds actually overshoot their levels of neutral buoyancy sufficiently to penetrate the high tropical tropopause. In addition it is by no means clear that those few clouds that do penetrate the tropopause actually mix tropospheric air into the stratosphere. Rather, it is likely that the overshooting cells as they collapse due to negative buoyancy actually entrain stratospheric air which is then mixed into the troposphere."

From these observations it would appear that the aridity of the stratosphere is not controlled by freeze-drying at the tropical tropopause after all. The specific observations suggesting this conclusion are as follows:

- 1) As pointed out by Robinson [1978], freeze-drying requires a deep layer of cloud, either the widespread deep cirrus of a mean up draft or the hot towers of cumulus convection surrounded by subsidence. Observational data support neither in sufficient quantity to maintain the Hadley flow to the stratosphere.
- 2) Available observations indicate that when convective clouds do penetrate the tropical tropopause they produce mixing ratios > 6 ppmv rather than < 3 ppmv. This point is also made by Danielson's comment in the NASA 1977 ITCZ Experiment: "Kuhn's stratospheric water vapor measurements indicate large, day-to-day temperature variations which are positively correlated with easterly waves. Thus, the moist stratosphere appears to be directly related to the highest coldest clouds. These results are inconsistent with the hypothesis that the dry stratosphere is produced by vapor in equilibrium with cirrus from cumulonimbus clouds" [Poppoff et al., 1979].
- 3) As emphasized by Robinson [1978], mixing ratios typical of the stratosphere (< 3 ppmv) are observed to extend as much as 100 mb beneath the tropical tropopause in clearly tropospheric air. If correct, these seem to demonstrate that tropospheric air is not only

dried before it cools to the temperature of the tropical tropopause but that it is also dried more than it would have been by simple freeze-drying to the ambient tropopause temperature.

This analysis appears to have led to an even bigger dilemma than the one with which I started. As of now I have only two suggestions as to how this dilemma might be resolved.

- 1) There remains the possibility that all or most of the Hadley flux to the stratosphere is restricted to geographical areas and seasons in which the tropical tropopause is colder than -80°C . This could include the period Dec-Feb when the ITCZ is in the southern hemisphere and the tropical tropopause is highest and coldest (~ 80 mb and -84°C). It might also include the period Jun-Aug when the Indian monsoon is pushing against the Himalayas and this area experiences its highest and coldest tropopause, although the temperature of -75°C indicated by Sastry and Narasimham [1966] does not appear cold enough. These are the periods and areas in which the Hadley cells achieve their maximum intensity. Also Doppelick's [1979] recent downward revision of the total radiative heating rates at the tropical tropopause makes an impulsive, as opposed to a steady, Hadley flux to the stratosphere somewhat more palatable. Even if this were sufficient to explain the sustained aridity of the stratosphere it would leave unexplained the apparent widespread occurrence of mixing ratios ≤ 3 ppm in the upper tropical troposphere.
- 2) The other possibility pushes even farther into the unknown. We know that cirrus clouds, particularly the so-called mare's tails, provide a mechanism for condensing H_2O from upper levels of the troposphere and, through gravitational sedimentation and re-evaporation, transporting it to a lower level. I pose the following as a question rather than as a theory: Is it possible that in the temperature range of -50 to -80°C at pressures below 200 mb there exist seeding agents, possibly including cirrus crystals themselves, which are capable of collecting water vapor from what we now consider to be unsaturated air until the H_2O mixing ratio is reduced to the range of 2-3 ppm? This is reaching but it is the type of phenomena which appears to be required to reconcile our present observational data. If such were found to be true it would open up a whole new pathway for transport of air from the troposphere to the stratosphere i.e. through the jet stream gap between the polar and tropical tropopauses. While many have proposed such a route, most have rejected it because it appeared to provide no means for maintaining the aridity of the stratosphere. However, if the aridity is produced in the troposphere this objection goes away and a great deal of observational data suddenly becomes understandable. For example several authors have noticed the frequent occurrence poleward of 30° of a minimum in the vertical profile of O_3 at a height of about 15 km which strongly suggests that tropospheric air regularly enters the stratosphere through the tropopause gap [Dobson, 1973]. Mastenbrook [1974b] included one of his Washington, D.C. soundings to "illustrate the frequent occurrence of pronounced ozone structure and the absence of pronounced water vapor structure in the altitude layer between the polar and equatorial tropopauses (in this case April 25, 1968 between 300 mb and 85 mb)." "In the multiple tropopause region," he reported, "an ozone peak just above the polar tropopause is overlaid by a layer low in ozone which is more characteristic of tropospheric air. The water vapor mixing ratio for the ozone poor layer does not differ noticeably from the overlying stratospheric air above the tropical tropopause." Mastenbrook [1974b] also cited Helliwell's [1960] description of flights which traveled north below the tropical tropopause and entered the stratosphere above the polar tropopause; these found that the frost point of air in the multiple tropopause region was not noticeably different from that of stratospheric air to the north.

At present I am not much happier with these suggestions than with my earlier suggestion [Ellsaesser, 1974] that Antarctic winter freeze-out removes 3 to 4.5×10^8 tonnes of $\text{H}_2\text{O}/\text{yr}$ from the stratosphere. However, no more palatable way has yet occurred to me for reconciling the apparent paradoxes posed by our current observational data on stratospheric H_2O .

ACKNOWLEDGEMENTS: This work was performed under the auspices of the U.S. Department of Energy by the Lawrence Livermore Laboratory under contract No. W-7405-Eng-48 with support by the High Altitude Pollution Program of the Department of Transportation, Federal Aviation Administration. Deep appreciation is expressed to Marcel Nicolet for the tendering of an expense-paid invitation to attend this NATO Advanced Study Institute on Stratospheric Ozone which made the above support possible.

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Figure 1. Northern hemisphere meridional section of 1948-1951 mean January temperature in °C along 80°W from Kochanski [1955].

Figure 2. Southern hemisphere meridional section of Mean winter temperature (June-July) in °C along 180°E composited from radiosondes and satellites by Leibitzke and van Loon [1972].

Figure 3. Zonal average 100- and 50-mb temperatures derived from Nimbus-4 IRIS measurements for April 1970 through January 1971 by Prabhakara and Rodgers [1976]. Contour interval 4 K.

Figure 4. Streamlines (10^9 Kg/sec) of mean meridional circulation for Jun-Aug composited from data of Newell et al. [1972] and Louis [1974]. From Danielson [1975].

Figure 5. Streamlines (10^9 Kg/sec) of mean meridional circulation for Dec-Feb composited from data of Newell et al. [1972] and Louis [1974]. From Danielson [1975].

Figure 6. 1948-1951 mean heights of the northern hemisphere tropopause along 80° W for months of Jan., Apr., Jul. and Oct. from Rochanski [1955].

Figure 7. Summary of observations of vertical profiles of stratospheric H₂O compiled by Harries [1976].

Figure 8. Summary of observations of the latitudinal gradient in stratospheric H_2O . Data sources identified by references.

Figure 9. Long term trend in stratospheric mixing ratio as indicated by data of MRF [Cluley and Oliver, 1978], Mastenbrook [Penndorf, 1978] and Harries [1973].

Figure 10. Map of northern hemisphere sighting of nacreous clouds compiled by Stanford and Davis [1974].

Figure 11. Summary of recent stratospheric H₂O soundings from the DOT HAPP Bulletin for June 1979.

Figure 12. Summary of recent soundings of stratospheric and mesospheric H₂O not included in Figure 11.