The Atmosphere of Venus: Conference Review[1]

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(Manuscript received 5 December 1974, in revised version 6 February 1975)

This summary is organized neither by sessions of the conference nor by atmospheric region. Rather, it uses the headings: composition, clouds, structure, motions, evolution. Because of time limitation, it is personal and somewhat sketchy; important topics had to be omitted.

1. Composition

Belton[3] reminded us that there could be a major gas present besides CO_2; the evidence that the CO_2 abundance is greater than 95% is still shaky. Indeed, the proportion of other gas could be as great as 10–20%. If the gas were lighter than CO_2, the temperatures from occultation data would be reduced, and would in fact agree better with the direct Venera temperatures. Regions with apparently superadiabatic lapse rates would be brought closer to adiabatic, with less need to postulate errors in the data. The whole stratosphere, for which we have only occultation temperatures, would be cooler, and condensation would be easier.

The most likely candidates are nitrogen and neon. From the Mariner 10 UV measurements, the abundance of helium is clearly small. Nitrogen was considered by McElroy (1969) a few years ago in a series of ionospheric models, but has not been popular recently. If he and Donahue are correct about the very low abundance of O atoms, a gas like N_2 or Ne could be useful replacement as a source for the upper ionosphere.

Another important gas is water vapor. There has been a tendency, especially among aeronomers, to assume a very low mixing ratio, even below the clouds [e.g., McElroy *et al.* (1973) propose 50 ppm]. The swing of opinion at this meeting was back to a considerably larger amount, several parts per thousand. For the region observable spectroscopically, Barker presented the interesting results of a patrol, showing clearly how variable the abundance is. He quoted variations of a factor 2–5 across the disc on a single occasion, and showed a dramatic effect of phase angle resembling that for CO_2. Taylor gave a preliminary analysis of the IR data from Mariner 10; the longwave channel is sensitive to H_2O near the 250 K level. While this is not much deeper than is seen by conventional spectroscopy, it is much less affected by the cloud particles.

For published data on O_2, we must still refer to the old work of Belton and Hunten (1968, 1969). A much stronger limit, by Traub and Carleton, has been mentioned by McElroy *et al.* (1973), who also give a still lower prediction ($\sim 10^{-7}$ mixing ratio) from their aeronomical model.

Mariner 10 gave us excellent data on H atoms in the thermosphere and exosphere, and confirmed the controversial results from Mariner 5 (Broadfoot *et al.*, 1974; Barth *et al.*, 1967). The amount of hydrogen is strikingly small, and strongly constrains the range of possible models. First, the total amount of hydrogen in the stratosphere must be limited to a few parts per million; this includes HCl, H_2O and H_2. Second, even with this limitation, the homopause (or "turbopause") must be very high, to prevent diffusive separation from building up the high-altitude hydrogen. The eddy coefficient must be at least 10^8 cm^2 s^{-1}, or an equivalent large-scale circulation must exist. These conclusions have independently been reached by Kumar and Hunten (1974), and, at this meeting by Donahue and Liu and by McElroy and Sze.

If transport is so rapid, the amounts of CO and O in the thermosphere are also limited, probably even more so than on Mars. Along with the low temperature, also indicated by the Mariner 10 data, these ideas have forced a new look at the formation of the ionosphere. Several suggestions have been made recently, but none seems to have gained wide acceptance (Bauer and Hartle, 1974; Kumar and Hunten, 1974). Other ideas were discussed at the meeting by Donahue and by McElroy. There seems to be agreement that, in addition to CO_2, a lighter ionizable constituent must also be present, and that the influence of the solar wind may be important even below the "ionopause."

A few years ago, the means by which photochemical CO is oxidized was a major issue, and the Fifth Arizona

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[1] Presented at the Conference on the Atmosphere of Venus, Goddard Institute for Space Studies, 15–17 October 1974.

[2] Kitt Peak National Observatory is operated by the Association of Universities for Research in Astronomy, Inc., under contract with the National Science Foundation.

[3] Author citations without dates refer to papers presented at the Conference. Further information can be obtained from Goddard Institute for Space Studies, or from this issue of the JOURNAL OF THE ATMOSPHERIC SCIENCES.

Conference on Planetary Atmospheres in 1971 was largely devoted to it. The conference proceedings issue of this journal also published the first paper on HCl aeronomy (Prinn, 1971). McElroy *et al.* (1973) have now produced a convincing picture, in which photolysis of HCl leads to copious production of odd hydrogen from H_2. The CO is then oxidized by reaction with OH. We still do not know whether Prinn's mechanism, involving an unstable ClOO radical, is of comparable importance.

This work has direct application to the terrestrial stratosphere, and indeed the most important threat to the ozone layer now seems to involve chlorine, produced from chlorofluoromethanes. The work of Wofsy and McElroy (1974) on this question had its origin in their studies of Venus.

2. Clouds

I find the identification of the visible clouds as concentrated H_2SO_4 by Sill (1972) and Young (1973) to be totally convincing, despite the warnings of a "bandwagon effect." But there are still a number of issues to discuss.

It is at first plausible to identify the bottom of the H_2SO_4 layer with the boundary at ~35 km inferred from the Venera 8 radiometer data (Lacis and Hansen, 1974). But according to Wofsy's discussion, liquid H_2SO_4 is not stable at 480 K unless the partial pressures of SO_3 and H_2O are large. It seems likely, therefore, that the bottom is closer to 45 or 50 km. This interpretation may not lie outside the error bars of the data; alternatively, there may be clouds of a different composition below the sulfuric acid.

Young's review stressed the remarkable involatility of the cloud at the visible surface, caused by the nature of a concentrated solution. This cloud therefore behaves totally unlike a terrestrial cloud. In particular, it is not expected to mark out waves in the atmosphere, which are seen on Earth by virture of their temperature variation. It is very difficult to reconcile H_2SO_4 clouds with any idea that the Mariner pictures are showing wave motions, even though many features of the pictures are reminiscent of wave phenomena. If it really is waves we are seeing, we must either find a supplementary cloud material or some way, other than mere condensation, to make the waves visible. Hapke and Prinn have both suggested sulfur, but that too is involatile.

Another striking thing about the particles is their very narrow size distribution. Hansen and his collaborators, here at the Goddard Institute, continue to emphasize that this interpretation is required by the observations. It seems likely that an active mechanism is necessary to produce and maintain the narrow distribution; but none of the suggestions made here seem to me fully convincing. A feature as unusual as this seems likely to have something important to tell us, if we could only tell what. One factor, mentioned by Gierasch, is that large particles fall faster than small ones. Perhaps atmospheric motions (an aeronomer would say eddy diffusion) can support the small particles but not the large ones, which simply fall out the bottom of the cloud. They would then evaporate, decompose into H_2O and SO_3, and be recycled back into smaller particles. Eddy support of particles has recently been discussed for the stratosphere by Prinn (1974), and for our own atmosphere by Hunten (1975).

The COS mechanism for recycling material to the clouds (Prinn, 1973) is attractive, but does have some difficulties, as discussed by Wofsy. It may be that COS is only involved in a "makeup" of a cycle that mainly operates with SO_3. The rate at which the COS processes had to operate would then be much smaller. We are ignorant of the rate at which COS would be formed from SO_3 near the surface; all we know is the thermodynamic end-point. At the top, there are questions whether the oxidation of the intermediate SO_2 to SO_3 is fast enough to prevent the SO_2 from being visible spectroscopically.

Three days of discussion have brought us no closer to an explanation of the ultraviolet markings. Our bafflement is well expressed by phrases used by Young: "weather on Venus," and "it is almost as if somebody is painting the clouds with pale yellow paint." (We do, however, know that both light and dark areas have similar spectra, merely stronger for the dark.) Is the material dissolved in the H_2SO_4 droplets, or in independent particles? Where does solid sulfur fit in? Where is the dark material located relative to the main cloud: above it, near its top, or deep within it and barely visible? Hapke showed a tentative measurement of polarization differences, but it seemed as if such a result would still not lead to a unique choice.

At any rate, it seems likely that the motion of the markings does reflect actual mass motion of the atmosphere.

3. Structure

The high surface temperature is widely thought to be supported by the greenhouse mechanism, but as Belton pointed out, there is little quantitative evidence for this supposition. He did point out one little-known study, by Roeckner (1972), and we heard a progress report by Pollack and R. Young. When this work is completed, we will have a much better idea of the situation. Venera 8 has shown that some solar energy actually does reach the surface, although according to Lacis the exact amount is extremely uncertain.

At the same time, the Goody-Robinson mechanism is looking less probable. Stone expressed doubt that such a Hadley cell, driven from the top, could ever transport heat downward. Then Kálnay de Rivas showed results of several of her numerical models. Originally this work had confirmed the Goody-Robinson idea, but only with a Boussinesq approximation. An improved model showed a cell, but only in the top 10 km of the cloudy region. By allowing some solar radiation to penetrate, she could get the cell to penetrate deeper and deeper. Clearly, this sequence is beginning to approach a greenhouse model, but with the air motions treated explicitly. It is not always remembered that a greenhouse model, which is one-dimensional, implicitly requires circulations to maintain spherical symmetry.

It has always been surprising how well the mean thermal structure of an atmosphere can be given by one-dimensional radiative-equilibrium models. I suspect (though the statement is heretical) that this is more than a coincidence. The situation could be achieved by atmospheric motions that are so dominantly horizontal that they redistribute heat in horizontal shells before they cause much vertical transport. The motion of material tracers in the Earth's stratosphere is of just this kind. Other stratospheres may be similar; my statement may not apply at all to tropospheres (Stone, 1972). Gierasch *et al.* (1970) have given a comparison of radiative and dynamical time constants, and warned us to be suspicious when the latter is the shorter. I would remain suspicious, but optimistic for the reasons given above.

I would like to recall a point made by Pollack, that a knowledge of our own stratosphere is a useful guide to other atmospheres. Particularly, one should sometimes look at actual measurements rather than smoothed averages. An observed profile of anything is usually ragged; the peaks and valleys are real, but the details are of limited significance. Such structure is normally seen in temperature profiles from planetary occultations, even for an object as distant as Neptune. And the haze layers seen at the limb by Mariner 10 are another example. Perhaps a third is the structure in the electron-density profile.

One's tendency is always to explain such behavior in a one-dimensional picture, e.g., waves may be invoked. On the Earth, the real explanation often requires extra dimensions. A spectacular example is seen in ozone profiles below 20 km, where variations of a factor of 2 can occur in less than a kilometer (Dütsch, 1971). Here the explanation is clearly "fingering," or interleaving of layers of air of different geographical origin. Correlated structure is seen in the temperature, though it is much less spectacular.

Temperatures in the stratosphere are obtained from the occultation experiment (Howard *et al.*, 1974), but become uncertain toward the top of this region. It should be kept in mind, however, that there is more information in the electron-density profile. The peak occurs at a well-established density, and any model of the stratosphere and mesosphere must be consistent with that density.

At the top of the atmosphere, there is now universal agreement that the exospheric temperature is "low"—about 400 K from Mariner 10, and according to Anderson, as low as 275 K from Mariner 5. As I have already mentioned, ionospheric models therefore require an appreciable amount of something lighter than CO_2. Thermal escape of hydrogen becomes very slow, and the implications for the hydrogen budget are also mentioned above.

4. Motions

Atmospheric dynamics was discussed in two different senses: the global and stratospheric circulation, and the thing we aeronomers call eddy transport. The deep circulation, and its implications for the thermal structure, have been mentioned above where it is indicated that there seems to be an agreed position. If this survives we should have a coherent picture in a year or two.

For stratospheric winds, we have a wealth of data from Mariner 10 and from Earth-based photography. Most of the time Traub sees Doppler shifts that generally confirm the motions. This ties in with the suggestion that the cloud particles cannot reasonably show wave motions and are probably showing actual air velocities. Zonal winds are therefore usually near 100 m s^{-1}, and meridional motions are too small to be reliably detected so far.

The curious thing, however, is that the large-scale pattern gives a strong impression of being wound up by differential rotation. What could be causing those huge streaks extending across a whole hemisphere?

One meaty bit of data was shown by Dollfus. The typical pattern seen from Earth repeats three times around the equator, just like the montage made from Mariner pictures. The Mariner situation *is* typical, and so is wavenumber 3.

The other aspect of dynamics is vertical eddy transport. The eddy coefficient in the thermosphere, where the level of diffusive separation occurs, is around 10^8 cm^2 s^{-1}. If the vertical mixing is due to large-scale phenomena, they must work at the same rate. The homopause is above the ionospheric peak, as on Mars, but very different from Earth. In the stratosphere, lower values (approaching 10^6 cm^2 s^{-1}) seem to be indicated, from oxygen transport and from the scale height of the haze (Wofsy, this meeting). Such values exceed by nearly three orders of magnitude what we are used to just above the Earth's tropopause. Within the clouds, we have evidence for actual turbulent layers. Woo's analysis shows that the effect can be seen in both the amplitude and the phase of the two-frequency radio experiment.

5. Evolution

We had four interesting papers on the origin and history of Venus' atmosphere. I find them impossible to summarize, except to say that I was struck by the variety of opinions, some of them orthogonal. The remarkable thing is that we are beginning to know enough about Venus that it is fruitful to think about its history. The subject is strikingly interdisciplinary; it would be unthinkable to discuss it in the absence of meteoriticists, geologists, and geochemists, and these people range with freedom (mentally, at least) over the whole solar system.

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