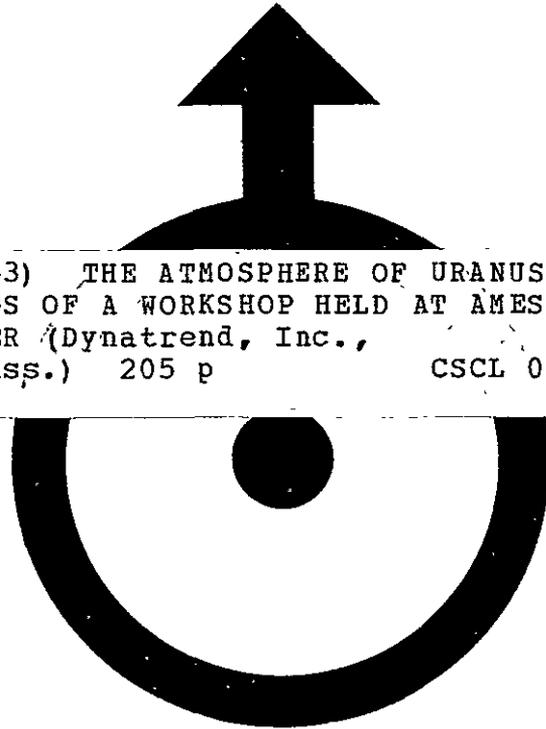


The Atmosphere of Uranus



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THE ATMOSPHERE OF URANUS

The Proceedings of a Workshop held at Ames Research Center
September 30, 1974

Edited by

Donald M. Hunten
KITT PEAK NATIONAL OBSERVATORY

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

Ames Research Center
Moffett Field, CA 94035

URANUS ATMOSPHERE WORKSHOP

Donald M. Hunten, Kitt Peak National Observatory, Chairman

Michael Belton
Kitt Peak National Observatory

Samuel Gulkis
Jet Propulsion Laboratory

Torrence V. Johnson
Jet Propulsion Laboratory

John S. Lewis
Massachusetts Institute of Technology

Tobias C. Owen
State University of New York

James B. Pollack
Ames Research Center

Ray T. Reynolds
Ames Research Center

Peter H. Stone
Massachusetts Institute of Technology

Frederick W. Taylor
Jet Propulsion Laboratory

Laurence M. Trafton
University of Texas

Lloyd Wallace
Kitt Peak National Observatory

A. Bruce Whitehead
Jet Propulsion Laboratory

Nick Vojvodich
Ames Research Center

OTHER CONTRIBUTORS

AMES RESEARCH CENTER

Louise R. Boyce
Lawrence Colin
John V. Foster
William L. Jackson

DYNATREND INCORPORATED

John A. Polutchko

PRINCETON UNIVERSITY OBSERVATORY

R. E. Danielson

PREFACE

An early mission to Uranus was recommended by the Science Advisory Group (1973), because of the existence of a Jupiter swing-by opportunity in 1979. They further recommended a Saturn-Uranus package, involving three launches in 1979 and 1980, focusing on direct measurement of these atmospheres by entry probes. The desirability of a Uranus probe was also emphasized by the more recent Mariner-Jupiter-Uranus Science Advisory Committee (Van Allen, 1975). Probe technology is moving ahead rapidly now that the Pioneer-Venus mission is going into its hardware phase. It is therefore appropriate to take a closer look at the details of outer-planet probing.

Saturn and Uranus were chosen ahead of Jupiter, because entry into Jupiter's atmosphere, at some 50 km/sec, poses formidable problems. The existence of the Jupiter-Uranus opportunity suggests another possibility: a probe designed specifically for Uranus. NASA Headquarters therefore convened a special committee to examine the scientific payloads of outer-planet probes, with emphasis on Uranus. At the first meeting, September 30-October 2, 1974, several guests were invited to a Uranus Atmosphere Workshop, whose proceedings form the present document. The paper by R. E. Danielson, who could not attend, was added later. There is no attempt at a complete discussion of Uranus science, because two excellent compendia are already in press (Newburn and Gulkis, 1975; MJU-Science Advisory Committee, 1975), in addition to the papers from the Science Advisory Group (1973) and an earlier version of the Newburn and Gulkis review (1973). Most of the contributions here are concerned with the atmosphere, or those aspects of the interior that bear on atmospheric questions. Special attention is paid to the entry environment for a probe, specifically the mean molecular weight and temperature at the entry altitude. The last paper thus focuses on engineering matters and tries to bring out just which parameters are of most relevance.

Ames Research Center and its staff provided a fine environment for the Workshop. John Polutchko of Dynatrend, Inc. was responsible for the recording, transcription, and technical editing of the proceedings. Other editing has been very light: the document is essentially a transcript of the informal talks.

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Chapter 1

INTRODUCTION AND SUMMARY

D. M. Hunten

Uranus, third from the Sun of the Jovian planets, is also a remarkable individual. Basic data (from Newburn and Gulkis, 1975) are shown in Table 1-1.

Table 1-1. Uranus Data

Mass	8.739 x 10 ²⁵ kg (14.63 \oplus)
Equatorial Radius	25,900 km (4.06 \oplus)
Mean Density	1.21 g cm ⁻³
Equatorial Surface Gravity	7.77 m s ⁻²
Mean distance from Sun	19.182 AU
Period	84.013 y
Length of day	~10.8 h
Equatorial Inclination	97.93°
Times of Solstice	1946, 1985
Times of Equinox	1966, 2007

The mean density is greater than that of Saturn, despite the smaller total mass; it must therefore be richer in elements heavier than hydrogen and helium. Nevertheless, a very deep atmosphere is probably present; there is certainly no observational evidence for a surface. The rotational axis is nearly in the orbital plane; the interesting consequences for atmospheric circulation have been explored by Stone (1973, 1975). The system of satellites is of great importance to any mission of exploration, but is outside the scope of this study.

The question has been asked, "Is Uranus not just a poor substitute for Jupiter, of interest only because probe entry is easier?" A discussion of this point at the Workshop disclosed a surprising unanimity; clearly Uranus has a great deal of intrinsic interest. Indeed, Jupiter itself was widely regarded as uninteresting until 1968, when the Pioneer 10 and 11 opportunity was announced. The study needed for an adequate proposal "turned on" many people to Jupiter, and a further wide influence was produced by two major conferences: Dallas, late in 1968 (Icarus, vol. 10), and Tucson, 1969

(J. Atmos. Sci., vol. 26). This influence of the Pioneer program may have been just as important to Jupiter science as the measurements made by the spacecraft. The analogy is close with the present situation for Uranus.

Jupiter figures importantly in the present volume as an object which is better measured than Uranus, and on which concepts and models can be tested. An important issue must immediately be faced: the temperature profile obtained from a variety of Earth-based observations, and supported by computations and by the infrared experiment on Pioneer 10, is incompatible with the results of the radio-occultation experiment on the same spacecraft (see Figure 2-6 in Wallace's article). This question was discussed at some length. A variety of spectroscopic and radiometric observations is very difficult to reconcile with the high occultation temperatures, even if a very improbable high-altitude cloud is postulated. But the most powerful argument, due to Gulkis, may be summarized as follows:

The observed brightness temperature of Jupiter at 13 cm is about 280° K; this is the wavelength of the occultation experiment. The atmosphere must therefore be opaque to these radio waves at the level where that temperature occurs, and the occultation experiment cannot possibly probe deeper than the 280° K level. For any reasonable absorber, the atmosphere at the limb must be opaque to a still higher altitude and lower temperature. The radio-occultation temperatures greater than 280° K, and up to 700° K, must be spurious, and doubt is cast on the profile even where it shows lower temperatures.

This argument also suggests a possible explanation of the data, though it has not yet been worked out quantitatively. If the radio waves cannot penetrate the deeper layers of the atmosphere for some reason (absorption or strong scattering), they may instead reach the Earth by an indirect path, perhaps scattered by upper layers of the atmosphere. This process is aided by the great distance of the spacecraft behind the limb: the scattering angles required are only a couple of arc-minutes. (See note, p. 7.)

Recalling that this is only the strongest of many objections to the occultation temperatures, the group agreed that only the "conventional" profiles should be considered. Use of the same physics for Uranus is therefore believed to be justified.

Reference Model

Our reference temperature profile (Figure 1-1) is based on the work of Wallace found in Chapter 2.1 below. Following Hunten and Münch (1973), the helium fractional abundance by number, $[He]/([He] + [H_2])$ is taken as 0.10. Other constituents are discussed below. We think it most unlikely that this number is outside the bounds 0.05-0.20, as discussed below under "composition".

For design of an entry vehicle, the most critical region of the atmosphere is around 1-2 mb, in the upper stratosphere and close to the stratopause. In the framework of radiative equilibrium, calculated for a global average, we can have considerable confidence in the temperatures shown, because we have confirmation for both Jupiter and Neptune. What about departures from this

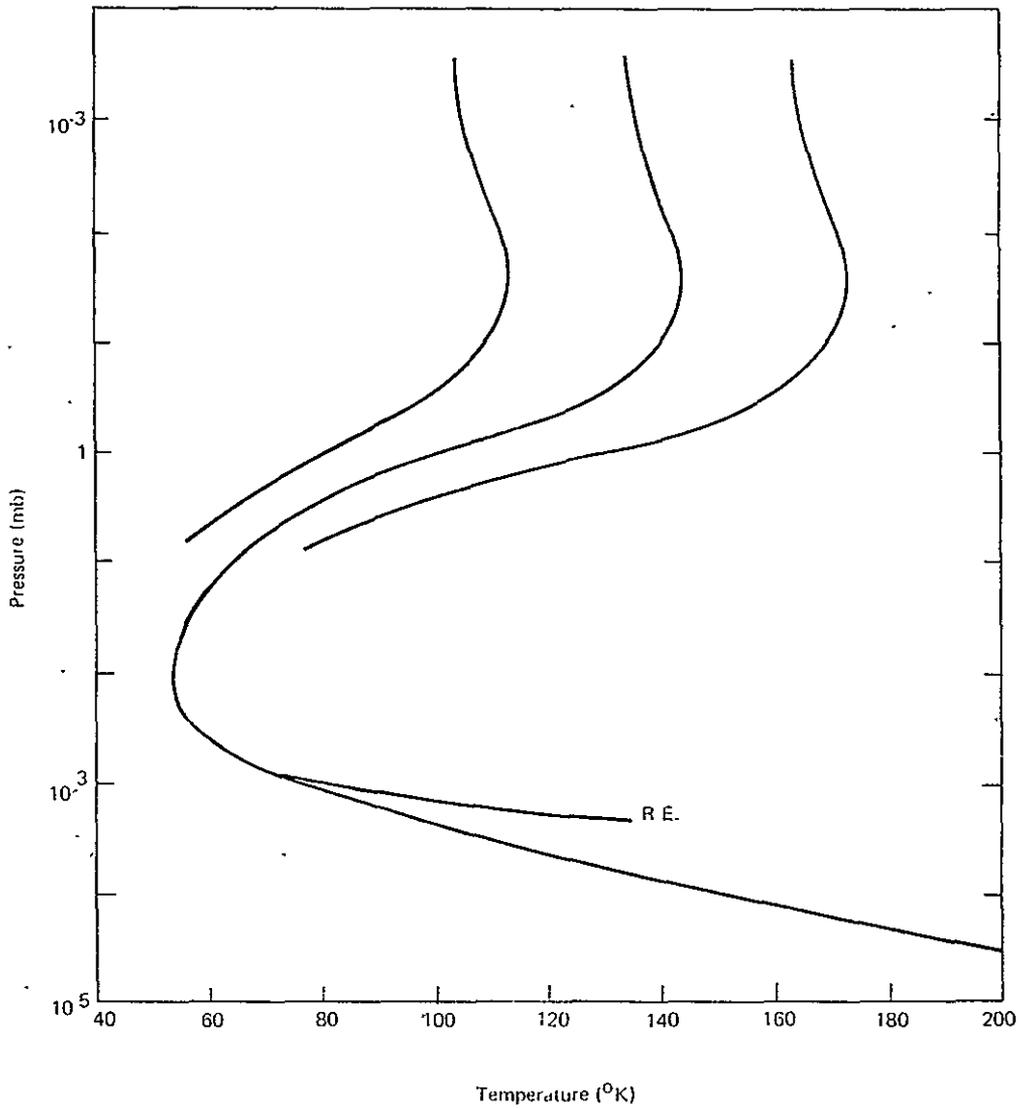


Figure 1-1. Adopted temperature profile for Uranus. Confidence limits of $\pm 30^{\circ}$ K are shown for the stratosphere, decreasing at greater depths. The radiative-equilibrium curve (R.E.) should be replaced by an adiabat, as shown, below the point (750 mb, 70° K).

ideal situation? The rapid rotation of Uranus should give good zonal averaging; we have to depend on horizontal advection of heat for meridional averaging. This question has been considered by Gierasch, Goody, and Stone (1970), who conclude that the process is probably important to pressures as low as 0.14 mb on Jupiter, and greater heights yet on Uranus. Global averaging should therefore be a reasonable approximation, even to the extent of smoothing out the seasonal variation. At the time of encounter for a 1979 launch, one pole of Uranus will be aimed near the Sun. Stone has emphasized that there will be a negligible seasonal change in the troposphere, but the stratosphere is a different matter. Without smoothing by advection, the winter hemisphere might drop to the Gold-Humphreys skin temperature, $2^{-1/4} T_e = 52^\circ \text{K}$, and the summer hemisphere become appreciably warmer than the mean. To allow for such a possibility, bounds of $\pm 30^\circ \text{K}$ for the upper stratosphere are included in Figure 1-1. At lower altitudes the uncertainties are less.

Thermal Emissions

The emission spectrum of a planet, in both far-infrared and radio regions, can be very informative about its structure. Unfortunately, an object at liquid-nitrogen temperatures is very difficult to detect; and the available information is limited. As Wallace points out, the most important part of the spectrum (around $50 \mu\text{m}$) does not reach the ground; aircraft measurements, if feasible, will be highly desirable. Again, we have included a short description (Section 2.3) of corresponding results on Jupiter.

An important point from Wallace's discussion is that stratospheric temperature inversions are to be expected on all the Jovian planets, and are confirmed for some in several ways. These inversions ease the entry problem for a probe; but they complicate the interpretation of infrared data. It is possible to draw seriously wrong conclusions from an analysis that assumes an isothermal stratosphere. Wallace discusses one example, and an even more striking case is that of Titan (Hunten, 1974, 1975). For all five objects, an important heat source is absorption of solar radiation by methane. On Jupiter, Saturn, and Titan, an ultraviolet-absorbing haze is also present, although difficult to include quantitatively in a model. This haze, if present at all, seems to be much weaker on Uranus and Neptune.

Another common feature of a stratosphere is stratification, as the name was originally intended to imply. On Earth, this effect is most strikingly seen in ozone data below 20 km (e.g. Dütsch, 1971). Fluctuations of density about the mean by as much as a factor 2, on a height scale of 1-3 km, are frequently seen. They imply "fingering", or interleaving of layers of air with different geographical origin, in a region that is inefficiently stirred. Stellar-occultation data show what may be an analogous effect in the temperature for both Jupiter and Neptune; the observable result is large "spikes" on the light curve (Wasserman and Veverka, 1973; Wallace, 1975).

Radio emission in the ammonia band at 1.25 cm gives a further probe of the stratosphere. Longer wavelengths have the unique ability to give information on the deep atmosphere. The analysis by Gulkis (Section 2.2) shows that the predicted brightness temperature at 21 cm is much too low in a model

with a solar abundance of ammonia, in contrast to Jupiter where a beautiful fit is obtained. He suggests that the H₂S abundance might be just right to segregate most of the ammonia in a cloud of NH₄HS. Later discussion considered the possibility that the rest of the ammonia might have been incorporated in solution, deep in the planet. We have here another indication of a fundamental difference between Uranus and Jupiter. If borne out, there may be important implications for the origin of the solar system.

Composition

The spectroscopy of Uranus and its implications for the composition are reviewed by Trafton and Owen in Section 2.5. Unfortunately, there is no way to see helium at present, and interpretation of the very strong methane absorptions is controversial. Are they due to a high abundance of CH₄, or primarily to an uncommonly clear atmosphere? There is no doubt that the latter effect is important, but no agreement on how important. Trafton would not venture a guess on the methane abundance. Owen and Cess estimate 2-3% (by number) from analysis of one band. From a different band, Belton and Hayes obtain 0.1%, comparable with the value expected for solar elemental composition. In any case, the amount of methane is probably too small for an important influence on the mean molecular weight, which would be increased 20% by a mixing ratio of 3%.

Ammonia is not seen spectroscopically; the radio evidence is mentioned above (Section 2.2). There are plenty of unidentified lines, and Belton and Hayes suggest a large abundance of N₂. This idea, while not widely accepted, needs to be studied, because the mean molecular weight would be affected.

For the helium abundance we must turn to other evidence. There are strong reasons for believing it to be close to the solar value. A large enhancement in Jupiter is ruled out by the mean density (e.g. Hunten and Münch, 1973). For other objects we appeal to extreme implausibility of any process that could enrich helium relative to hydrogen on a planetary scale. Jeans escape of hydrogen cannot do it (Öpik, 1963; Hunten, 1973). Alfvén has repeatedly urged that unspecified hydromagnetic processes could have separated the elements in the solar nebula; but these ideas have never found wide acceptance. The only other possibility is condensation. Öpik (1962) once suggested that solid H₂ might be buried deep in a cold Jovian interior, leaving the helium in the atmosphere. Such low temperatures are incompatible with current ideas, and with the microwave brightness temperatures. Moreover, the solar nebula is not thought to have been cold enough to condense H₂. And finally, accretion of extra hydrogen (as H₂ or H₂O), with a hot interior, would lead at best to an atmosphere rich in hydrogen. Enrichment of helium is far, far less probable.

Some models of Uranus' interior have postulated a large helium abundance to match the mean density. Such procedures were never intended to represent a real planet; there is every reason to think that the extra mass is in the form of heavier elements (ices, rocks, and metals).

The ideas cited above have been included to illustrate the lengths to which one must go to justify any enhancement of the helium abundance in an atmosphere. Since the solar (or cosmic) abundance is itself uncertain, (Hirschberg, 1973), we have allowed a factor of 2 uncertainty, for Uranus: A smaller value yet is conceivable, but as the engineering consequences are not serious, there is no pressing need for a confident lower bound.

Clouds and Hazes

From the telescopic appearances of Jupiter and Saturn, it is obvious that they have abundant clouds. Uranus is a very different matter, and a few years ago Belton, McElroy and Price (1971) suggested that to first order, the appearance of this planet could be explained if the atmosphere were totally free of particles. Since then, several lines of evidence have turned up that suggest either a faint haze or a deep-lying cloud deck. As discussed by Trafton, and Belton, these data include the limb darkening from Stratoscope 2, strong bands that are not totally black at the bottom, and an ultraviolet albedo slightly less than expected. Belton also points out that Jupiter-like belts have sometimes been reported by visual observers. On the other side, detection of Raman-shifted Fraunhofer lines confirms that most of the light returned by Uranus is scattered by clear gas.

Prinn and Lewis have pointed out that condensation of methane is likely near Uranus' tropopause, and there are several discussions in this report of the related question: Is Uranus' stratosphere deficient in CH₄, as the Earth's is in H₂O? Although both propositions are reasonable, they are not necessarily true, even if temperatures go below the condensation point in a model. Planetary atmospheres are much less spherically symmetrical than our models of them, and there is always the possibility that a "cold trap" has big leaks. One can also imagine small particles being carried aloft by atmospheric motions and evaporating above the trap, or that supersaturation occurs. On the other hand, if condensation does take place, the cloud particles tend to fall out and re-evaporate at a lower, warmer level. This downward flux must be balanced by an upward flux of vapor. If the atmosphere is unusually stagnant, like the Earth's stratosphere, the partial pressure of methane could simply follow the vapor-pressure curve with no cloud at all.

Other Matters

Stone's work on the general circulation is discussed only briefly here, but he has papers in the earlier compendia mentioned in the Preface. Uranus is of great interest as an extreme case that is not realized elsewhere in the Solar System: features are the long thermal time constant, small internal heat source, and axial orientation.

We know little about the interior except what can be deduced from the mean density. The small heat flux encourages us to hope that there is a surface, conceivably even an accessible one. Lewis and Reynolds have attempted to set limits on this possibility. Some insights can be obtained from studies of the satellites and models of the origin of the solar system. But we will need mean densities for the satellites before they can tell us anything. One

of the major strengths of an entry probe is its ability to support measurements of noble gases, even to the isotope ratios. Since these gases reside primarily in the atmosphere, they give strong evidence about the early history of the planet.

Note added June 1975: Since the material on p. 2 about the Jupiter occultation experiment was written, the major reason for its failure has been found (Hubbard, Hunten, and Kliore, 1975). The original reductions of the data omitted the oblateness of Jupiter's limb, and therefore contained an error of a few percent in a crucial velocity component. Although final results are not available yet, it is clear that the temperature profiles will closely resemble the standard ones discussed in this volume. Our confidence in the transference of these ideas to Uranus has been justified.

Chapter 2

WORKSHOP PRESENTATIONS AND DISCUSSIONS

2.1 INFRARED RADIOMETRY AND RADIATIVE-EQUILIBRIUM MODELS

L. Wallace

So little is known about the thermal structure of Uranus that one can't just look at Uranus all by itself. You need to look at the four outer planets, Jupiter, Saturn, Uranus, and Neptune, mostly Jupiter and Neptune, and do a little bit of interpolation.

On Table 2-1, the column labeled T Effective Observed is not really what it says. For Jupiter and Saturn, those are determinations from total infrared flux measurements and probably fairly reasonable except that for Saturn I haven't figured out whether the rings are mixed into that determination or not.

Table 2-1. Temperatures

	T_e (obs)	T_e (calc)	T (occ) ⁵⁾
J	134 ±4 ¹⁾		168 ±17 ⁴⁾
S	97 ±4 ¹⁾		
U	60 ±4 ²⁾	58 - 55 ³⁾	
N	57 ±4 ²⁾	46 - 43 ³⁾	141 ±11 ⁶⁾

- (1) Aumann, et al. (1969) S = S + rings?
- (2) Murphy and Trafton (1974)
- (3) $T_e = 279 \sqrt[4]{1 - A_B / \sqrt[2]{D}} \cdot A_B = .34 - .5$
- (4) Hubbard, et al.; Veverka, et al. (1974) 189 ±16
- (5) [He] / [H₂] = .1 by number
- (6) Wallace (1974)

DR. POLLACK: I believe they are mixed in because the figure is about 90.

DR. WALLACE: All right. The Uranus and Neptune numbers of 60 and 57 should not properly be labeled "observed." They are from Murphy and Trafton (1974) and they are based on measurements at 20 microns. I think they require a model to effectively extrapolate from 20 microns down to the wavelength

Preceding page blank

region where most of the radiation is coming from and therefore, I think are probably not good numbers. That is, the 60° for Uranus and the 57° for Neptune.

DR. TRAFTON: They are somewhat model dependent, but a wide variety of fictitious models as well as realistic ones were considered in the case. We tried augmenting the opacity at the long wavelength in an attempt to drive up the temperature, and find out just how small the effective temperature could be.

DR. WALLACE: The center column on Table 2-1, the effective temperatures calculated, are just equilibrium temperatures; that is, the Sun radiating on the planet and what is not reflected comes out in thermal infrared and, depending on what Bond albedo you happen to like, you can get, for example, for Uranus, between 58° and 55°. For the 58° temperature, I used Bond albedo of .34 or .35, the number that Murphy and Trafton used in their work. On the other hand you can get 55° if you take a Bond albedo of 0.50, and that is a number that Rieke and Low (1974) used in discussing some recent infrared measurements on Uranus. I think they are all rather mushy numbers on Uranus.

The far right-hand column of Table 2-1 gives optical occultation temperatures for Jupiter and Neptune. Those are high altitude temperature at about 10^{-5} atmospheres. The uncertainties indicated of 10% are very conservative, I think, because what is actually measured, if anything can be obtained from the optical occultations, is the scale height. And I have used here a helium-hydrogen mixing ratio of .1 to get those occultation temperatures. If the helium mixing ratio goes up, these temperatures have to be scaled up according to the mean molecular weight. So, I think those occultation temperatures, if anything, are lower limits. If you start tweaking the composition, I think you are going to jack those up, not down.

Now, in the cases of Jupiter and Neptune, they clearly indicate a temperature inversion in the atmosphere. In the case of Saturn, we haven't had a recent photoelectrically recorded optical occultation, but, on the other hand, we know from the infrared spectrum, which shows ethane in emission at about 12 microns, that there has to be a temperature inversion at Saturn also.

If we think that these four planets form some sort of relatively simple progression as you go farther from the Sun, then I think you must expect a similar temperature inversion in the case of Uranus. But as far as I know, there is no direct evidence for any temperature inversion. On the other hand, I think it would be foolish not to figure on one. I don't know what temperature one would get, but if you guess something like a minimum of 140° or 150°, I think that would be reasonable.

Any modeling one can do is pretty much a matter of interpolating in some way between the pressure/temperature point from the optical occultation down to the effective temperature, and some people don't like any such interpolation. But that is about all the modeling process can do, if you have a model that holds up at all. Recently, the models that have been applied to the outer planets have been called into question mostly by the Pioneer 10 flyby.

Figure 2-1 presents curves for Jupiter, Saturn, Uranus, and Neptune. They are for helium, hydrogen, and methane alone in solar abundance. These differ from most published thermal structure calculations in that we have allowed the solar radiation to be absorbed in the methane bands. That gives the high altitude heating above about 10^5 dynes per square centimeter. Below that pressure, the presence of the methane is irrelevant, and all that matters is the effective temperature of the planet and the opacity source, which is the hydrogen-helium mixture. But above 10^5 dynes per square centimeter the solar flux absorbed in the methane bands has to be radiated and between about 10^5 and something like 10^3 , the radiation is by means of the hydrogen-helium mixture. But that mixture is a poor radiator. The temperature has to get up fairly high before it can be radiated. As you go up in the atmosphere to lower and lower pressure, the temperature climbs because the hydrogen-helium mixture becomes a poorer and poorer radiator and it climbs up to the point where the methane, itself, can do the radiating. And that is above something like 10^2 dynes per square centimeter.

These models still do not include factors like emission by ethane.

This type of modeling was suggested by Gillett, Low and Stein (1969) on the basis of the infrared spectrum of Jupiter. There were some incorrect models of this type calculated for Jupiter by Hogan, Rasool, and Encrenaz (1969), but since then, the error has been fixed in Hogan's method and Cess and Khetan (1973) have published models of this type for Jupiter and Saturn. So they are not completely new. Cess's work is mostly published in the Journal of Quantitative Spectroscopy and Radiative Transfer and I think many astronomers haven't caught on to the fact that he has done a lot of work on these models and that they are around.

DR. TRAFTON: Do I understand that the methane-to-hydrogen ratio is of solar abundance in the models of Uranus and Neptune?

DR. WALLACE: It means the carbon-hydrogen ratio from the Sun has been scaled to obtain a methane/H₂ ratio and plugged in here.

DR. TRAFTON: You don't take into account the freezing out of methane?

DR. WALLACE: No. And that would certainly occur. That is right. The temperatures go so low in the Uranus-Neptune models that certainly the saturated vapor pressure is large enough that there would be a freezing out here. I have not put that in at all.

DR. POLLACK: It seems that would drastically affect your Uranus-Neptune model because your whole structure at altitude above the 100 millibar level is really dictated by the properties of methane. In other words, the first is solar absorption and then, secondly, the methane radiation, and there could be drastic consequences.

DR. WALLACE: Yes. I think that from those two models probably the methane mixing ratio would be reduced by the order of a factor of 100 up above, say, 10^5 dynes per square centimeter. And that would push the whole thermal

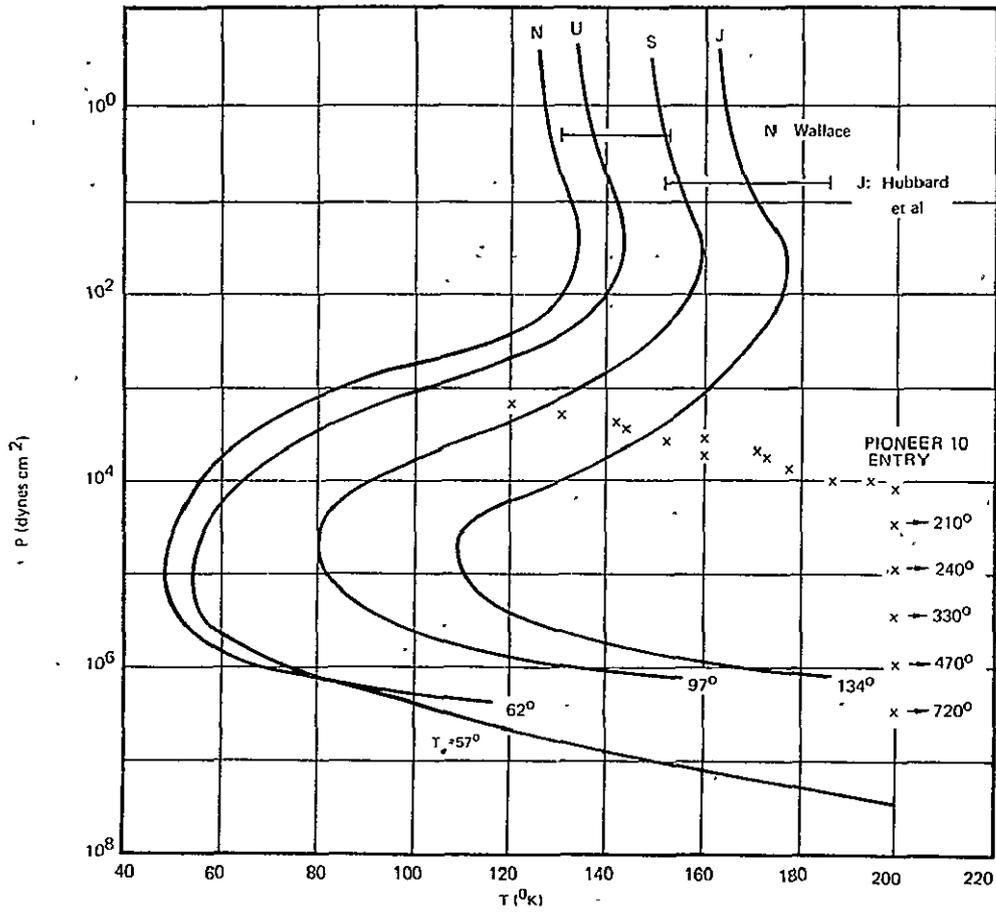


Figure 2-1. Profiles of Outer Planet Atmospheres -- Radiative Equilibrium Models. In the case of Uranus, an adiabatic lapse rate is indicated below the depth where the radiative gradient exceeds the adiabatic. The stellar occultation results (between 1 and 10 dynes per square centimeter) are from Wallace (1975) and Hubbard et al. (1972) and the Pioneer 10 profile is from Kliore et al. (1974).

structure up. That temperature inversion would then occur higher. [This has been confirmed by more recent work, to be included in a paper submitted to Icarus.]

DR. POLLACK: However, I am saying that you go to the limit of having no methane in the upper atmosphere to do any absorbing of solar energy and this, then, would show a flat tropopause value and that is what I am concerned about.

DR. WALLACE: That is quite right.

DR. MICHAEL BELTON: We don't know that to be true. It depends on the balance, say, of whatever is absorbed in the UV; it may come down very, very little.

DR. POLLACK: What I am concerned about here is if you take away the solar heating, there is nothing to raise the temperature if that balance is not absorbed.

DR. WALLACE: It is not fair to say that is the case. But certainly that is a problem. I agree completely, but I am not too concerned about it because there are worse problems than that.

DR. POLLACK: The other comment I wanted to make was on the positive side, and that is I have now put together a vertical temperature profile of Jupiter based directly on observational data in the sense that it is a temperature inversion of infrared spectral data and it looks quite consistent, with your profile for Jupiter.

DR. WALLACE: Let me go back. What Jim (Pollack) said about the uncertainty in the upper part is certainly correct, and there are other chemical constituents which probably ought to go in but we don't exactly know what to do with them, ethane, for example. There may be other heating mechanisms and we don't know what to do about them. That sort of problem is going to affect those profiles at altitudes above the order of 10^5 dynes per square centimeter.

Below that point, I think there are other problems. If there are no internal energy sources in the planet, then all of the energy deposited is solar energy. What I have done in these models is to say that if that energy is not absorbed in what I would call a methane band, I am going to say it is absorbed way down at the bottom, indefinitely deep. Well, that is not sensible. It will be deposited as some function of depth in the real atmosphere and I don't know how to put that in; I haven't put it in, but I think that would mean that the temperature increase below that temperature minimum as you go down may not be nearly as steep as this. I don't know that anyone has done a good job of poking into that, but it needs to be done at some stage and it is not in here. So that is something else that is wrong with these models.

They do tend to hit the occultation temperatures, which is kind of convenient, but that is just, I think, luck because there is so much left out.

The larger problem, however, is indicated by the set of "chicken tracks" running diagonally across Figure 2-1, called Pioneer 10 entry. You will note the temperatures down to 210°, 240°, 330°, 470°, and 720°. That is not a typo error, not 72° but 720°; that is a little higher pressure than one atmosphere. And the Pioneer 10 exit looks pretty much the same.

The agreement with the models is bad, but that is not the important thing. The real problem is with the infrared spectrum. The models don't matter. But you see, now, with the hydrogen-helium opacities that we think are the dominant thermal infrared opacities on Jupiter, if that Pioneer 10 entry profile were correct, you would see down to around one atmosphere of pressure and you would see brightness temperatures on the order of 500° to 700°; and you don't. You see up to maybe 150° or 175°, something like that.

The way to get around that problem, then, is to say, "Well, we will put a cloud in at the order of about 3×10^3 dynes per square centimeter or some opacity source such that you can't see through it to those high temperatures." Well, that cloud or opacity source then means considerable problems for interpreting the methane absorptions observed from earth, the hydrogen quadrupole line absorptions, and I think impossible problems for the 10 micron ammonia absorption. They are fairly broad lines which make reasonable sense if you have fairly high pressure to get the pressure broadening. But if you have to put that ammonia up, way up in the atmosphere and you don't have that pressure broadening, I think you have had it. The clincher comes on this when you attempt to calculate a brightness temperature model.

Figure 2-2 is not computed with the Pioneer 10. This is calculated with that Jupiter model that I just showed you. On Figure 2-2 you see two minima in the brightness temperature curve at around 350 and around 600. Those would be due to the S(0) and S(1) rotational lines in hydrogen.

The broad band spectra that I have indicated on that curve are more or less in general agreement with the brightness temperature variation there, but Jim Pollack has discussed some higher-resolution observed spectra in this general spectral region from Jupiter which showed, I believe, very clearly the S(0) and the S(1) lines.

Now that is the key question; Does that analysis hold up?

DR. POLLACK: Yes, it does, very well.

These results, discussed further below (page 52) show that Lloyd (Wallace) is quite right. Our spectra unambiguously show the S(0) and the S(1) lines and since the optical depth goes as pressure squared, if you make that high cloud assumption there is no way you can get those absorption features to show up.

I might also say that we have carried out an inversion to get the vertical temperature profile from our data, and it fits on very nicely with the similar type of inversion you get from the analysis of the methane seven micron

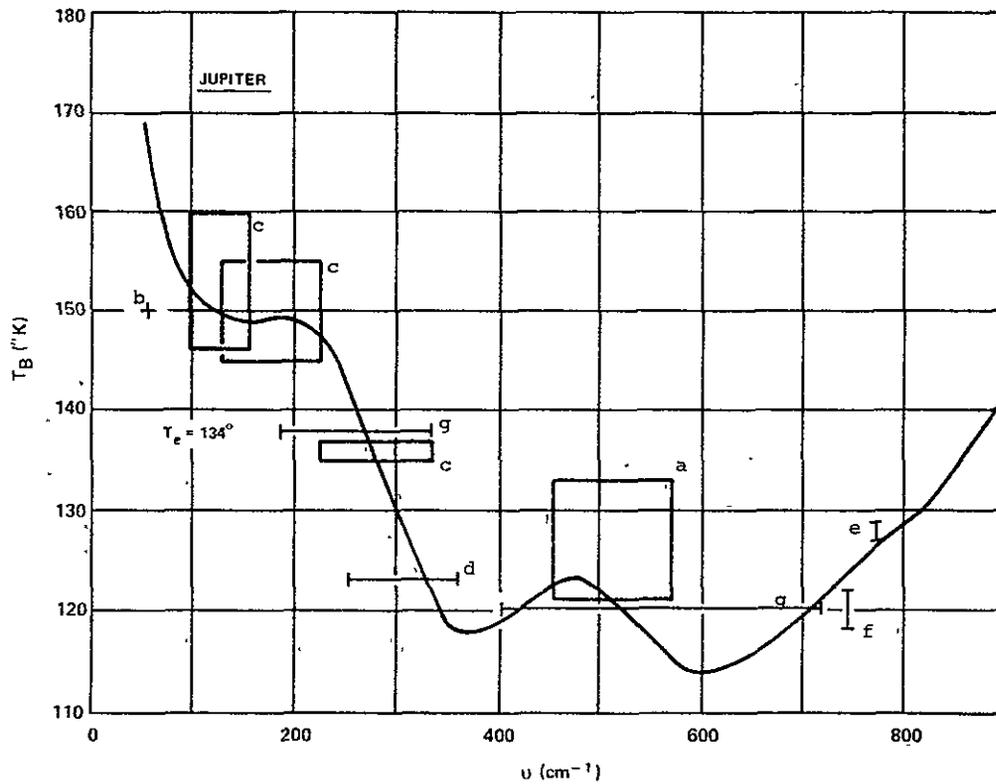


Figure 2-2. The observed Jupiter brightness temperatures are from a) Low (1965), c) Armstrong et al. (1972), d) Low et al. (1973), e) Aitken and Jones (1972), f) Gillett et al. (1969) and g) Chase et al. (1974). The point b) was assumed by Harper et al. (1972) to calibrate other observations.

feature. Such things as location, temperature minimum and the value of the temperature minimum agree very well with the two analyses.

I think one probably could come up with about three or four different things where the Pioneer 10 is in very clear conflict. The S(0) and S(1) lines are one example. I think it would be very hard to understand the ammonia radio temperature spectrum, and there you can't really start by invoking snowball size clouds, I don't think. So, in the absence of that, I think the Pioneer 10 data are going to be very hard to understand. And, thirdly, I think the near infrared and visual spectral observations in themselves are in clear conflict, and perhaps Owen would like to say something on that, in the sense that they give you an estimate as to what pressure level you are looking down to, and that is about one atmosphere; and from the distribution of the rotational lines, they give you some estimate as to what the temperature is. That is not a very good estimate, but it certainly is good enough for the purpose of discriminating between the model that Wallace put up that he prefers, and I think we all prefer, in contrast to the Pioneer 10 which would say that instead of getting a temperature from the distributional rotation lines of about 180°, that you get a temperature more like 400° or 500° which just doesn't fit.

So I think there are a whole bunch of measurements that all agree with one another in terms of what they say the temperature and structure of Jupiter is like, and all very clearly disagree with that given by the occultation data of Pioneer 10.

I think one's a priori feeling, before the occultation experiment occurred, was that it would give the best estimate of the temperature profile of Jupiter, based upon its success in the case of Venus and Mars. In fact, the reverse has occurred. I think, to my mind, there are so many different pieces of evidence that are all against the Pioneer 10 occultation data that the real burden of proof is on that experiment and not the other way around.

DR. WALLACE: But I think your detection of the S(0) and the S(1) lines in this infrared spectrum is the most compelling piece of evidence. Otherwise, if the Pioneer 10 is right, you have to have a magic cloud that looks like an H₂ atmosphere. And that's no good.

DR. OWEN: At the same time, I think one has to be impressed by the consistencies between the entry and exit, and we have a set of data that is telling us something. The question is what is it telling us? Does anyone have an answer to that?

DR. SAM GULKIS: I have looked at it at great length. I wish I had known that you were going to talk about it.

I have taken the pressure-temperature profile that was obtained in the Pioneer 10 experiment and added ammonia in the solar cosmic abundance with a mixing ratio of around 2×10^{-4} , computed the microwave spectrum, and what one sees is a continuum level of about 200°, as compared to measurements of around 130° to 140°, and very sharp lines that should be showing up in the spectrum which are not there. These lines will have a temperature of about 50°, and

would be very easy to detect from the ground, and they are not seen. That was my starting point.

In order to make it cool, you need to add an additional source of opacity. The strongest source of opacity at these temperatures and wavelengths is ammonia. I found that the amount of ammonia necessary to bring the continuum level into agreement with the observations is very large. The ammonia mixing ratio (by number) is required to be ~10%. The opacity of this ammonia-rich atmosphere is so high that the absorption that would have been seen in the occultation experiment is 100 db greater than they actually observed.

DR. WALLACE: Well, the point in starting with Jupiter here was to argue that the hydrogen-helium opacity source that we used to believe should be operative on Jupiter has to be still operative on Jupiter in spite of the Pioneer 10 data. And I think it is a pretty good case that one can make.

As far as the model on Figure 2-2 is concerned, in addition to what I told you previously is wrong with it, the high temperature increase at very low frequencies is certainly appropriate for the hydrogen-helium mixture. It would just increase indefinitely because the opacity is falling off and, presumably, some other opacity -- well, I don't know whether there are actually any good observations at critical wavelengths, but I assume that there might be and the model might be way high and that would mean some additional opacity at short frequencies. I don't know whether ammonia is the sort of thing that would be required to fix it or whether you need something else, too; I just don't know.

DR. OWEN: If you increase the ratio of helium to hydrogen, does that increase the hydrogen translational opacity?

DR. TRAFTON: That increases the opacity in the translational spectrum.

DR. WALLACE: Well, I think the tendency, if I increase the helium on this slide, would be to flatten out the curve.

DR. OWEN: You would remove that hump.

DR. WALLACE: Yes.

Figure 2-3 presents what I think the hydrogen-helium opacities look like. They are messy to calculate, and I am following Trafton's work on those opacities.

The upper two curves on Figure 2-3 are the opacities for the hydrogen-helium mixture, one at 59°, the dashed one, and one at 147°. As you can see, there is quite a change in the two.

Down at the bottom we have a couple of Planck curves, one at 135° and one at 60° for the outer planets.

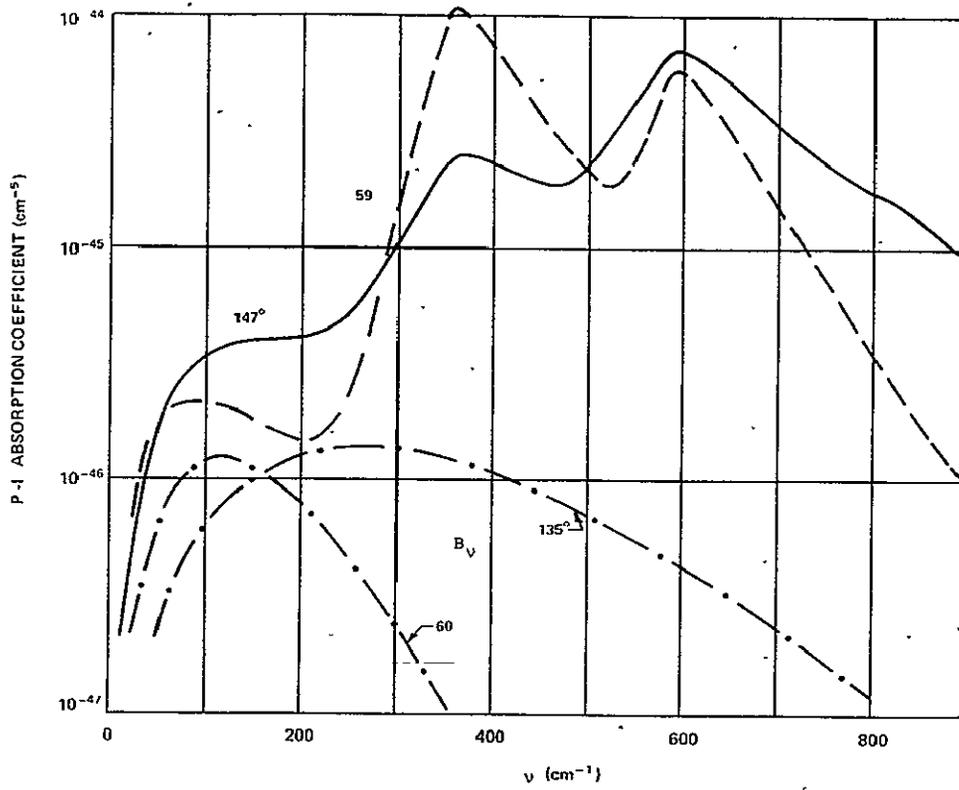


Figure 2-3. The absorption coefficients were calculated following Trafton (1967).

In the case of Jupiter, most of the flux, you can see, is peaking at a couple hundred cm^{-1} . That is the frequency at which you will see the effective temperature. Then, as you move away from that frequency to higher or lower frequencies, then, as the opacity goes either up or down, you will be seeing a different part of the atmosphere, and you will see a different temperature. As the opacity goes up, you will be seeing higher in the atmosphere where it is colder, but not all the way to the temperature minimum, so that the S(0) and the S(1) features would appear as temperature minima. But that would not necessarily be the case if you had a big change in absorption coefficient.

Figure 2-4 presents the same sort of information for Saturn, but I think it is hopeless. I wasn't able to spend enough time rummaging through the literature to figure out how thoroughly messed up the observations are by the rings. So I just plotted it on Figure 2-4 to see what it looked like, and it looks awful. I assume the rings are messing things up, and therefore let's forget it.

DR. POLLACK: The rings, for most of those observations except the ones in '65, do make a very significant contribution, on the order of maybe 30 to 40% of the total flux measured.

DR. WALLACE: Have you ever actually tried to do this kind of temperature comparison with models to see if they made any sense at all?

DR. POLLACK: No, I haven't gone to that step because of the ring problem. But I think it is possible to separate out the two now that we are starting to get measurements on the rings themselves at certain wavelengths. And I think that's what is going to make it possible to separate them. Or we could wait four years until the rings are withdrawn and then recheck it.

DR. WALLACE: There aren't very many observations on the Uranus spectrum which I've shown on Figure 2-5. There is the old Low (1966 b) observation at the order of 20 microns and then there is the Harper, et al. (1972), observation as shown by "C" on the figure.

DR. POLLACK: Do they really detect it?

DR. WALLACE: Yes, I think so. But the problem was they didn't have a calibration. This is at 350 microns. So they assume that Jupiter, at 350 microns, had a brightness temperature of 150° , which is a good guess. The recent observations by Rieke and Low (1974) at LPL are shown as "b" on Figure 2-5. They were rather impressed that the brightness temperatures appeared to be running to lower temperatures as you went to lower frequencies whereas models in the literature would say, "That's the wrong sense. The temperature ought to increase." But I think there are ways around that.

The temperature profile that I showed you previously for Uranus was calculated for an effective temperature of 62° and that gives the temperature profile of the upper curve on the figure. I don't know where I got 62° , it doesn't correspond to anybody's suggestion, I don't think. But it does tend

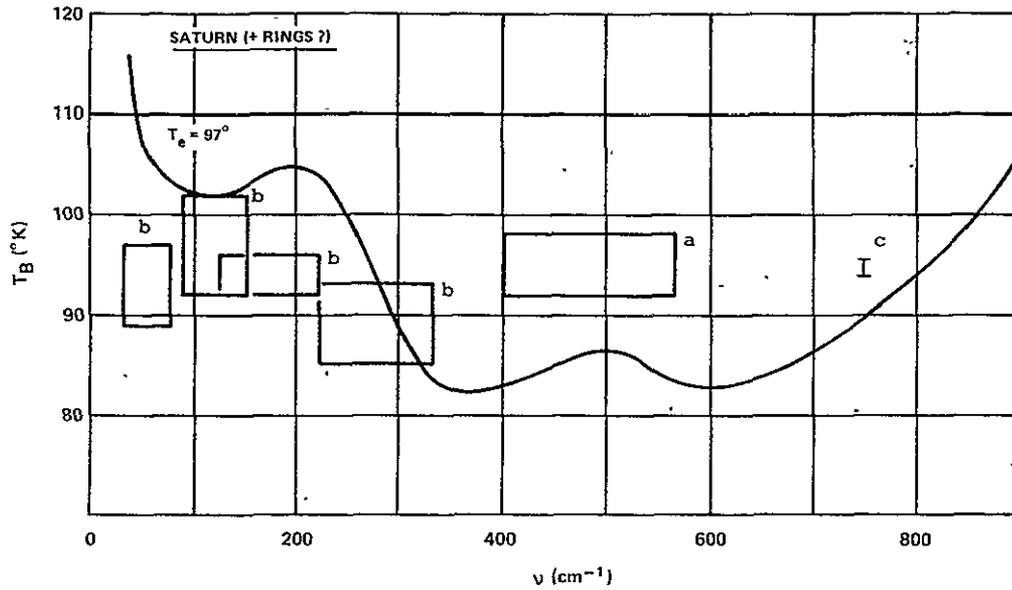


Figure 2-4. Brightness temperatures for Saturn were obtained from a) Low (1966a), b) Armstrong *et al.* (1972), and c) Gillett and Forrest (1974).

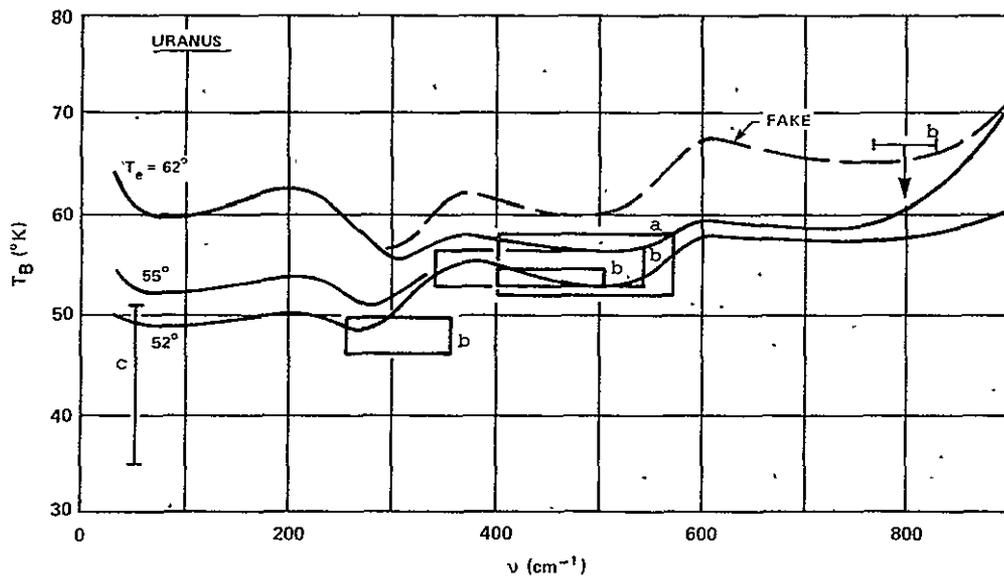


Figure 2-5. Brightness temperatures for Uranus are from a) Low (1966b) and c) Harper *et al.* (1972) and adjusted by Rieke and Low (1974), and b) Rieke and Low (1974).

to go through these observations at around 20 microns reasonably well, but it is quite high compared to the observations at 300 wave numbers.

DR. POLLACK: Excuse me, but I can't help making the comment at this point that if we follow Low and Rieke's deductions as they apply for similar measurements on Titan, they would conclude there is no hydrogen on Uranus.

DR. WALLACE: Well, that is kind of what they imply, but I don't think --

DR. POLLACK: It is exactly the same thing. They measure a certain set of temperatures that continually decline towards longer wavelengths in the case of Titan and, from that, they conclude that there is no significant amount of hydrogen on Titan. There seems to be a similar trend in the case of Uranus so, when you follow the same logic, then there is no hydrogen in the case of Uranus.

DR. OWEN: I don't think that is quite fair.

DR. WALLACE: You are getting carried away. I agree that is the implication, but I don't agree that -- let's proceed just a little further.

Now with this 62° model, referring to Figure 2-5, where you might say there is a maximum in the brightness temperature, in the case of the Jupiter models there is a minimum. This is the S(0) line, which is a temperature maximum, and the S(1) line, which is also a temperature maximum. These models are for Uranus, and it is the same modeling process that was used on Jupiter.

I'll talk about these other curves in a little bit, but can we go to that --

DR. POLLACK: These were computed from your model atmospheres?

DR. WALLACE: Yes, exactly.

DR. OWEN: What is the point up there marked "b" also. Is this also a Rieke and Low point? Is that a limit?

DR. WALLACE: Yes, exactly. It is at 12 microns. The question is: is the 12 micron ethane band poking up?

DR. OWEN: And they didn't detect the planet?

DR. WALLACE: That is correct, they did not detect the planet.

DR. BELTON: Are you going to mention, perhaps, at this point, that even though that is an uncertain point, the 350 microns is the most crucial factor? At least I think it is since it was tied into the Jupiter calibration.

DR. WALLACE: Yes, that is entirely right. The observers, to obtain this brightness temperature, measured a Jupiter-Uranus ratio at 350 microns and so took 150 degrees as the brightness temperature of Jupiter to get that

point. And probably, that 150°, if anything, I would say might be a shade low. But I think 150° is a reasonable number; if you were going to guess, wouldn't you say 150°?

Figure 2-6 is the same sort of presentation for Neptune, i.e., 20 micron observations and, at the point shown, they couldn't see the planet at all. I think this is another one that doesn't help us very much; it doesn't give us any additional insight.

Really, the Jupiter and the Uranus ones are the ones that are helpful.

Let's go back to the opacity subject on Figure 2-3. I noted that in the Uranus model the S(0) and the S(1) lines turned out to correspond to Jupiter's temperature maximum rather than minimum. What is happening is that as you go from Jupiter to Uranus, in those models, the big difference is the effective temperature difference.

One thing that happens is that the peak from the translational opacity up to the S(0) and the S(1) lines is considerably greater. That is, there would be an opacity ratio at the low temperatures, like around 60°, greater than it is at around 147°. Secondly, the peak of the Planck function at the low temperatures is shifted out to around 120 wave numbers as opposed to around 300 cm^{-1} .

What is happening to give that temperature maximum? -- well, look at it this way: you will see the brightness temperature at something like the peak in the Planck function so that if Uranus is around 60°, then you ought to see that kind of temperature at around 100 cm^{-1} . Then, as you go to the larger frequencies, not too much should happen to the brightness temperature up to 200 cm^{-1} because your opacity has not changed very much. But then, beyond 200 cm^{-1} , since the opacity is going up, you see higher and higher into the atmosphere by a very large amount. What you actually see in that model is that when you get up to about 280 cm^{-1} , you are seeing down to about the temperature minimum in the atmosphere. And then when you get to this peak just below 400 cm^{-1} , you are seeing to slightly above the temperature minimum. And that is why the S(0) and the S(1) lines come out as slight maxima as opposed to the Jupiter case.

Figure 2-7 shows the 62° Uranus model that we have been looking at before. What I have done here is to take that 62° model and draw in a somewhat hotter temperature inversion just to see how that would change the spectrum. And we can look at that one, and we can also look at the spectra from these 52° and 55° effective temperature models.

Up in this region, 1 to 10³ dynes per square centimeter, the effective temperature doesn't play any role at all, and temperature at these pressures is just a matter of the absorbed solar flux. In this model, the solar flux is absorbed by the methane and then radiated, and it doesn't matter what the effective temperature is, so that these models tend to coalesce very high, but you don't see any effects of the high altitude part of the temperature profile in these spectra.

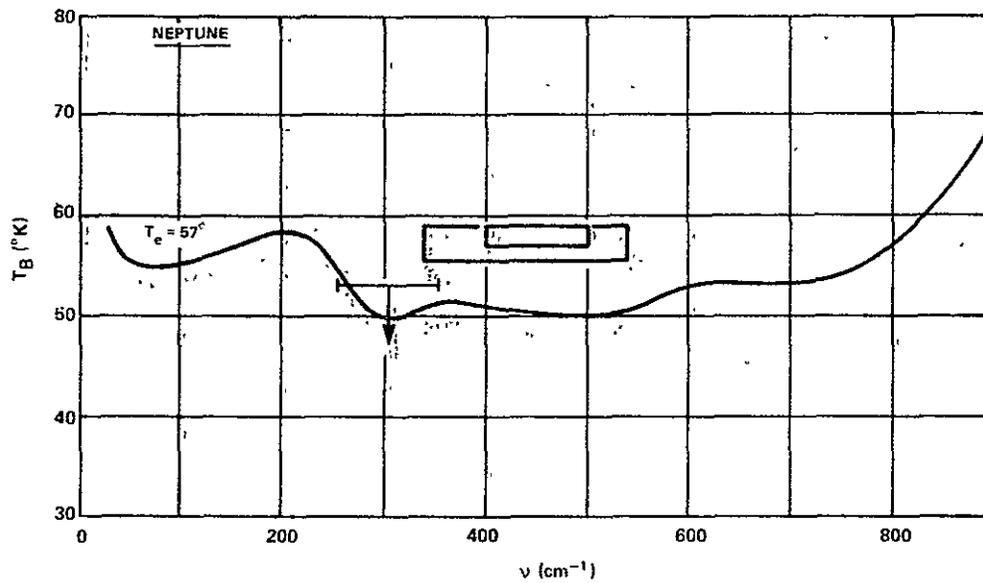


Figure 2-6. Brightness temperatures for Neptune were obtained from Rieke and Low (1974).

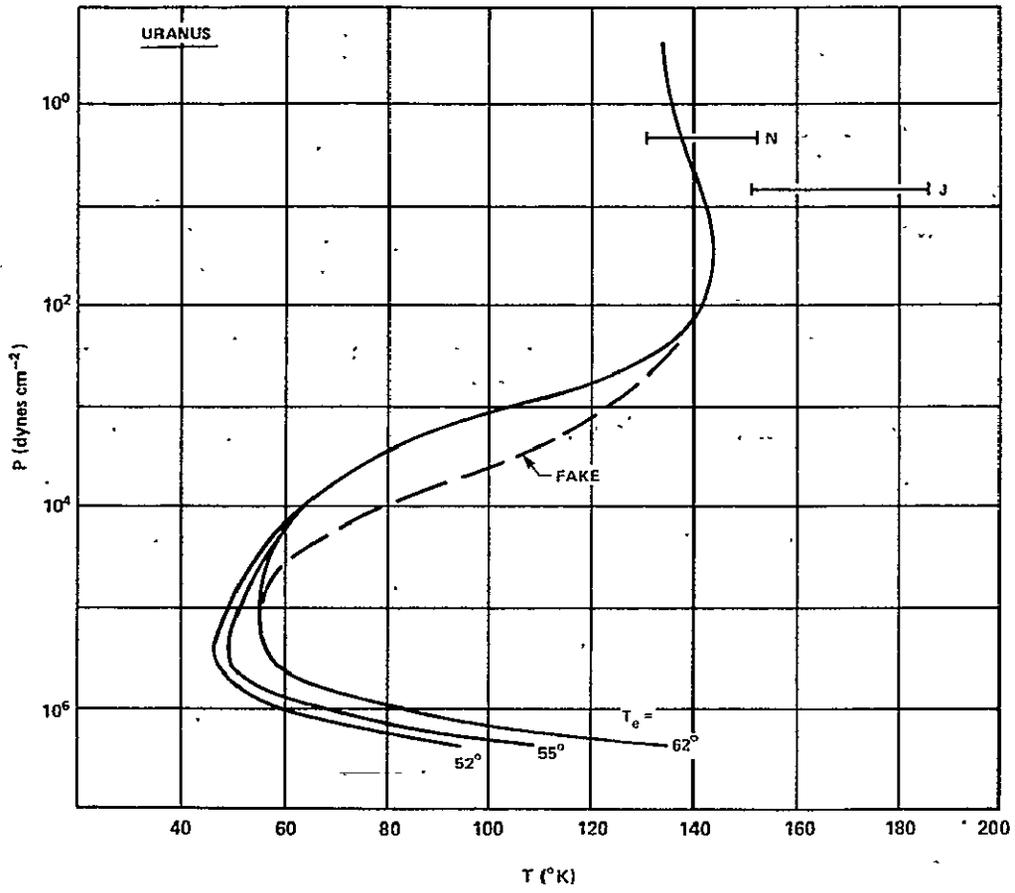


Figure 2-7. The stellar occultation results for Jupiter and Neptune are from Hubbard *et al.* (1972) and Wallace (1975).

We just reviewed the models, and returning to Figure 2-5 the model brightness temperature profiles are shown. You recall that we took the 62° model and then increased the temperature in the inversion region. And that temperature increase varied, but was of the order of 10° to 20° at most. This pushed up the brightness temperature in the S(0) and S(1) lines by 5° to 10°. And, at the same time, we did not change the brightness temperature significantly below 300 cm⁻¹. That is because we did not change the effective temperature, and almost all the flux is coming out in the 50-200 cm⁻¹ region; and it doesn't matter what model you are using, you ought to see approximately the effective temperature where most of the flux is coming out. It is the extrapolation away from that point, of course, that the model is defining.

Now the brightness temperature at 100 cm⁻¹, coming as low as it does, indicates that the model is not quite right. One should see about 62° here, and I think this is looking like around 60°. I think this is because this is a two-stream model. It is not a good angular quadrature, and if I ever actually publish this stuff I will have to do that over again. That ought to fix that because, clearly, that should be up at about 62°.

The models with the lower effective temperatures of 55° and 52° are probably off in the same direction. If it is done precisely, both should be a degree or two warmer in the 100 cm⁻¹ region. But again, as you move away from the peak in the Planck function and into the S(0) and S(1) region, you go through the temperature minimum, up to the peak in the S(0), down somewhat up to the peak in the S(1).

Now with these cooler models, the fit is generally improved. The models do show some dip to hit the point at around 300 cm⁻¹ and are not in any serious disagreement with the "C" brightness temperature point.

Now, the 52° model was used to see what it looked like, but if you will remember, Figure 2-1 where we attempted to calculate solar equilibrium brightness temperatures, it looked like about 55° was about as low as you could get. So I don't think 52° is realistic. That means that we probably are stopped from putting a model through the error bars, but I think the 55° model is in entirely reasonable agreement.

DR. OWEN: And aren't you still assuming solar values for hydrogen and helium?

DR. WALLACE: Sure. That is a variable one can play with, although I think the tendency, if you increase the helium, would be to make life a little more difficult in getting agreement here. But it depends on how much you increase it.

DR. BELTON: The point is that no matter what the opacity is down there, you are still going to come up with that 55°.

DR. WALLACE: Yes, that is right. That is not model dependent. The solar equilibrium effective temperature is as low as you can get. If you get an internal heat source, you can push it up, but you can't push it down.

DR. STONE: How about the albedo? You don't know that accurately.

DR. WALLACE: No. Well, the two numbers I picked out of the literature were 0.35 by Murphy and Trafton, and Rieke and Low liked around 0.50 and that gives you something like 55° to 58°.

So I think there is serious disagreement with this point. The models do tend to come up a little high below 300 cm^{-1} , but not much. The "C" point is based on the 150° guess for Jupiter, which is reasonable, but may be a little low. So I am satisfied that with the hydrogen-helium opacity models, one doesn't run into serious difficulty with the infrared brightness temperature observations. I think it is all right. It is not in very good agreement, but there is no disagreement, I don't think.

The other thing I would like to close on is that if Uranus does have this kind of an effective temperature, it is radiating most of the flux between 50 and 250 cm^{-1} , and it would be very nice to have infrared observations in this region as opposed to out on both sides. Very nice; so we could really get a handle on the effective temperature.

I was telling Trafton that I didn't entirely agree with his determination of the effective temperature from the 20 micron information. The reason for that is that as you can see with these models, the part of the brightness temperature curve below 300 cm^{-1} comes from below the temperature minimum, and is simply the effective temperature. However, out at 20 microns, whatever temperature you happen to see is coming apparently from above the temperature minimum and is very dependent on the temperature structure model that you think is correct. So, for that reason, I think any attempt to look at a 20 micron observation and guess or estimate the effective temperature at around 100 cm^{-1} is very mushy, it is so heavily model dependent that I don't think one should trust it very much; and particularly his suggestion that there has to be an internal heat source on Neptune, I think, is suspect. They used the same kind of 20 micron point to estimate what the effective temperature was there and came out with an effective temperature that was considerably greater than the solar equilibrium temperature.

DR. TRAFTON: Of course, there is independent evidence that Neptune has an internal heat source, just on the basis of the observed strength of the methane bands and the strength of the hydrogen quadrupole lines.

DR. WALLACE: Well, that is a different matter altogether.

DR. POLLACK: Can they indicate on internal heat source?

DR. TRAFTON: Well, since Neptune is further from the sun, methane should be frozen out deeper into the atmosphere but the same methane-to-hydrogen ratios indicate that is not the case.

DR. BELTON: You are saying that methane is not frozen out?

DR. TRAFTON: The methane-to-hydrogen ratio is about the same on the two planets in spite of one being much farther from the sun.

DR. WALLACE: I don't understand that line of reasoning, but principally I wanted to say that the more thoroughly we can map out this part of the infrared spectrum, the more heavily we can constrain the model makers.

DR. POLLACK: To put it another way, in the case of Uranus, the place that you are really gaining radiation from the Planck function is at these very long wavelengths.

DR. WALLACE: Right.

DR. POLLACK: And hardly any energy is coming out at the 20 micron band.

DR. WALLACE: Agreed.

DR. FREDERICK W. TAYLOR: If the brightness temperature for 20 microns and 100 microns is about the same, does that mean it is coming from the same level in the atmosphere?

DR. WALLACE: No. Assume a thermal inversion. At 100 cm^{-1} , the peak of the Planck function, you see a brightness temperature equal to the effective temperature arising from below the minimum in the temperature-pressure profile. Then, since the opacity at $20 \mu\text{m}$ is much larger than that at $100 \mu\text{m}$, the emission at $20 \mu\text{m}$ comes from much higher in the atmosphere, from above the temperature minimum.

DR. BELTON: That is an interesting point. You can tell, then, if hydrogen-helium is the dominant opacity from observations of this because if it is not the dominant opacity - let us say something else controlled the opacity down to 100 cm^{-1} - then the hydrogen would not necessarily go through the inversion. In other words, you might expect the S(0) and S(1) to be in absorption if something else was producing a lot more opacity down there. So that sounds like an observational test that can be done from the Earth. Otherwise, it gives you a very nice sampling right through the pressure range.

DR. TAYLOR: I have done calculations like that, too, and I found more contrast than these figures show.

DR. WALLACE: In what sense? In the temperatures?

DR. TAYLOR: Yes. Using a model.

DR. WALLACE: What kind of model? I mean, you know, it depends on what kind of model you use. I doubt that my model is any better than yours, but I will bet it is different. You see how much increased contrast I got with the "fake" model.

DR. HUNTEN: While this work is preliminary, it does emphasize that we simply cannot afford to go on thinking of atmospheres in the outer solar system

that don't have warm stratospheres. We ought to have learned that from our Titan experience, but sometimes it takes more than one example to bring the thing home. And, of course, this very issue is extremely important to the entry people. I think it is one thing that we can help them with a great deal; if we can agree that this is a high probability for the thermal structure of these atmospheres, it will ease some of their worries.

DR. POLLACK: Before we go on, I wanted to make the statement that I think your model atmospheres are very valuable and certainly give us our best crack right now as to what those profiles look like. For that reason, I would like to make the suggestion that perhaps it would be possible to look at this question of the freezing out of methane and try to answer these questions as to whether the stratosphere does contain methane.

DR. WALLACE: Yes, one can do that but how do you fix the other uncertainties in the model? Do you want me to put ethane in also?

DR. POLLACK: No, I would be happy with just answering this question on methane.

DR. HUNTEN: Well, let's remember one thing. There are two or three ways [see Chapter 1] of getting methane past a cold trap. Maybe the stratosphere is dry; but maybe it isn't.

2.2 MICROWAVE RADIOMETRY AND IMPLICATIONS

S. Gulkis

I want to start off talking about the measured microwave brightness temperatures of Uranus and the errors in the data set. This discussion is mainly for anyone who is interested in using this set of data.

Table 2-2 summarizes practically all of the radio brightness temperatures which are available along with their quoted errors. These errors are due to the statistical uncertainties of the measurements. The measurements include wavelengths from .14 centimeters to 21 centimeters.

Table 2-2. Uranus Disk Brightness Temperatures
(Table Compiled by B. Gary)

λ (cm)	T_D (K)	SE † (K)	Ref	Correction Factor	Estimated* Temperature (K)
0.14	95	±28	1	1.07	102 ±31
0.14	115	±52	2	1.00	115 ±52
0.21	122	± 8	3	1.00	122 ± 9
0.33	105	±13	4	1.10	115 ±15
0.35	130	± 6	5	0.97	126 ± 7
0.35	111	± 7	6	1.22	135 ±11
0.82	131	±15	7	1.12	147 ±18
0.95	125	±13	6	1.09	136 ±15
1.28	156	±15	8	1.00	156 ±16
1.65	201	± 8	9	1.00	201 ±10
1.95	181	± 7	6	1.03	186 ±14
2.03	202	± 6	10	1.00	202 ± 8
2.07	179	±13	11	1.00	179 ±14
2.7	212	± 8	9	1.02	217 ±10
3.12	158	±17	12	1.08	171 ±20
3.71	189	± 7	13	1.00	189 ±10
3.75	159	±14	14	1.08	172 ±17
6.0	210	±13	9	1.09	229 ±16
11.1	195	±30*	13	1.00	195 ±30
11.13	180	±40	15	1.00	180 ±40
11.3	130	±40*	16	0.99	129 ±40
21	280	±60	17	1.00	280 ±60

†Uncertainty due to signal/noise ratio.

*Uncertainty due to both signal/noise and calibration source uncertainty.

I want to spend a couple of minutes talking about systematic errors which are not listed in the table.

In practically all cases, the measurements are a ratio to something else, and the derived brightness temperature depends on what that ratio is and what has been chosen for a reference temperature. Most of the measurements are ratios to Jupiter and the quoted temperatures are dependent on the assumed temperature of Jupiter. Since different authors assume different temperatures for Jupiter and because Jupiter's brightness temperature is a function of wavelength, the quoted temperatures must be put on a uniform scale for comparison purposes.

A second possible source of error is the assumed figure of Jupiter. Many authors assumed that it was round. That also affects all of these numbers.

The next to last column of the Table 2-2 shows the estimated corrections required to put the observations on a uniform scale. The corrections range from 0 are up to a maximum of 22%, but there are a lot of 8% and 9%. Most of them have to be increased.

The last column contains the best estimate of what the temperatures look like on a uniform scale without taking into account the figure of Uranus at this point.

DR. STONE: What about the radius of Uranus? Are all of these reduced to the same radius?

DR. GULKIS: All of those are reduced to the same radius.

DR. BELTON: How much was the oblateness correction?

DR. GULKIS: Well, for Jupiter it is like 6% and for Saturn 12%. I have listed on Table 2-3 American Ephemeris and Nautical Almanac (AENA) values and values by Dollfus and Danielson, et al, for the dimensions of Uranus.

When people say they use the AENA values, I want to show that there is a problem there, because the AENA is inconsistent itself. It lists an equatorial radius of 23,535. It also lists the semi-diameter at 1 AU of 34.28". This turns out to be 24,846 km. So when someone says they have used AENA, we have to ask ourselves: Which one did they really use? Generally there is no way of telling from the publication.

The most likely thing that someone would do when reducing their data is actually to take the semi-diameter. Most observers would work from that rather than this more obscure number which is listed. So I presume that this is what most people did.

Table 2-3. Dimensions of Uranus

	EQUATORIAL RADIUS, km	S.D. AT 1 AU sec of arc	$\frac{1}{f}$	SOLID ANGLE $\times 10^{-10}$ (19.182 AU)
AENA	23,535	34.28 (24,846)	16	2.36 2.21 (Flattening Included)
Dollfus	25,400	35.04	33	2.39
Danielson <u>et al.</u>	25,900	35.73	100	2.54

AENA - American Ephemeris and Nautical Almanac - Explanatory Supplement
P. 491

Dollfus - ICARUS 12, 1970, P. 101

Danielson, Tomasko, Savage - AP. J. 178, 1972, P. 887

Now, a second complication that comes in is whether or not the observer took into account the oblateness, which is also listed in AENA. Depending on whether they used it or not, you would wind up with a solid angle of 2.36×10^{-10} or 2.21×10^{-10} steradians.

The more recent values are those of Dollfus and Danielson, which show equatorial radii from 25,400 to 25,900. The oblatenesses are quite a bit different here.

I suspect that the last column in Table 2-2 has to be reduced by about 5% now in order to make it consistent with these recent values.

DR. WALLACE: Where did Danielson, et al, get an oblateness number? I thought they just used a reported number.

DR. GULKIS: No, they reported an equatorial and polar diameter. After correction for telescope motion, the oblateness went from 16 to 100. There is almost no oblateness at all.

DR. OWEN: Is the time range of these observations sufficiently great that there could be a real variation that would change the aspect?

DR. GULKIS: There could be. I will point out what I think are probably the most "reliable" observations.

Figure 2-8 now shows the uncertainties both due to receiver sensitivity and some estimate for what I think are uncertainties in calibration.

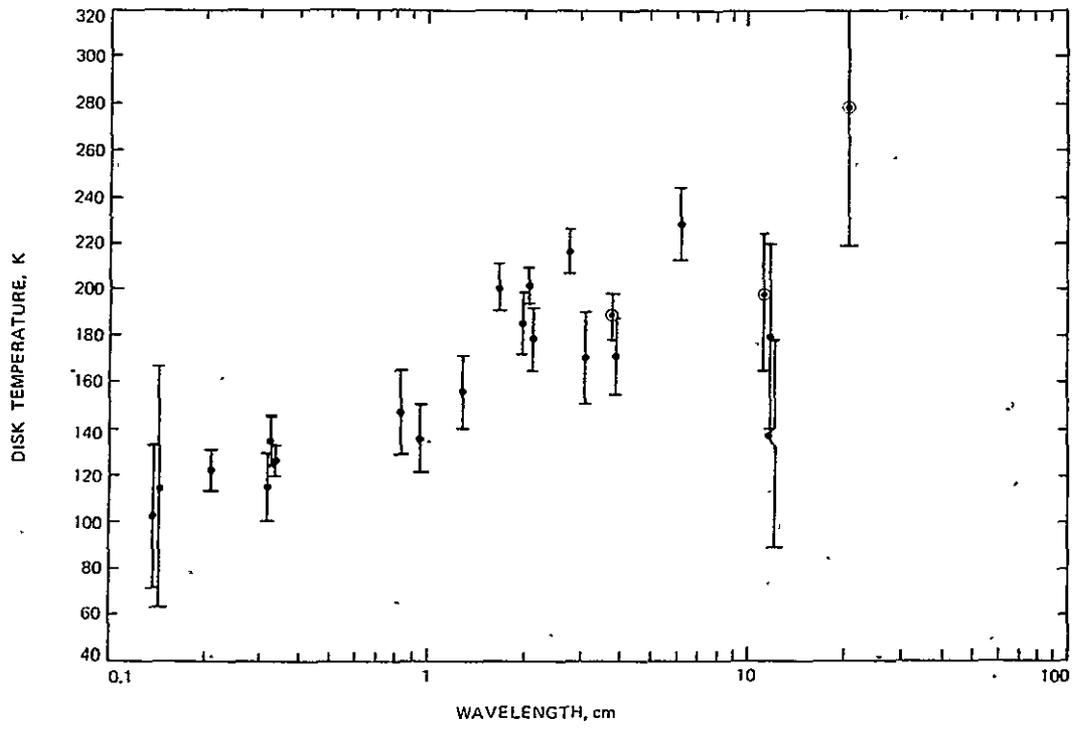


Figure 2-8. Uranus disk brightness temperatures.

Three of the brightness temperature measurements given in Table 2-2 are interferometer measurements of high quality. These measurements are the ones at 3.71 cm, 11.1 cm and 21. cm. The other measurements are made with single beam instruments.

You can see that the general trend is for the brightness temperatures to be between 100° and 120° at millimeter wavelengths, then to rise up to 200° K at longer wavelengths. The problem now is to try and interpret these data in terms of some kind of model.

I have attempted to fit the data with a solar cosmic abundance model. Figure 2-9 shows a deep convective model. The solid line shown is the pressure-temperature profile. It is normalized to a model by Trafton (1967). His radiative solution is shown, and convection begins at the point shown by the horizontal line. I have essentially normalized to this point, made it go convective at greater depths, and added ammonia to it with mixing ratio of 2×10^{-4} . There are some deviations in the upper part of the curve, but I call your attention to the fact that there is little radio opacity up there so it doesn't make any difference what structure you start with. I could have taken the convective and joined it directly with Trafton's radiative model; the final results would have been nearly the same.

DR. BELTON: Why did you use $G = 777 \text{ cm/sec}^2$? That's about 10% too low, isn't it?

DR. GULKIS: That takes into account rotation. That number is reduced slightly.

DR. TRAFTON: But aren't the microwave measurements whole disk? I mean, they include not just the equator but the regions where it is so difficult for us to assess?

DR. GULKIS: They are whole disk.

DR. STONE: Is that a mean?

DR. GULKIS: It might be; I am not sure. All of this is brand new because I have done it all, really, just getting ready for this meeting and I picked that one out of our review (Newburn and Gulkis, 1975). I don't think that makes any difference.

I think Figure 2-10 is a very, very important picture. When you compute the brightness temperatures on any model like that, this is the answer that you get. I would like to call attention to the fact that two other people have computed this. One is in the paper by Belton, McElroy and Price (1971). Kuzmin and Solov'ev have also calculated the brightness temperature for this model and arrive at the same answer.

We all get the same answer. The only difference is that we have a lot of data now which wasn't available at the time the other calculations were done.

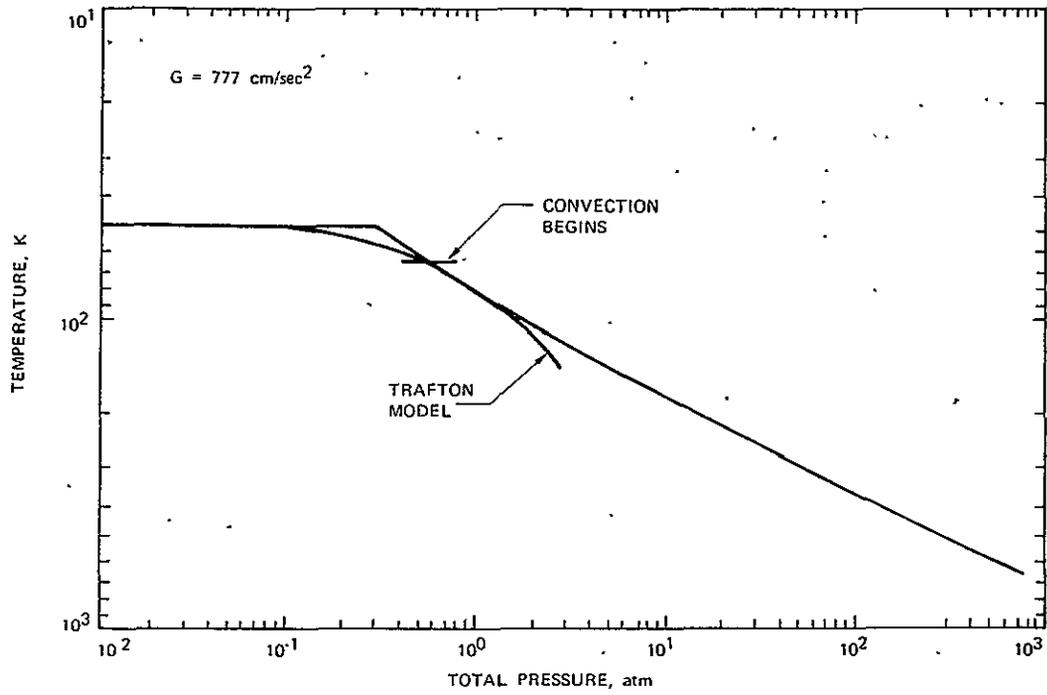


Figure 2-9. Uranus pressure-temperature profile for $\text{He}/\text{H}_2 = 0.1$.

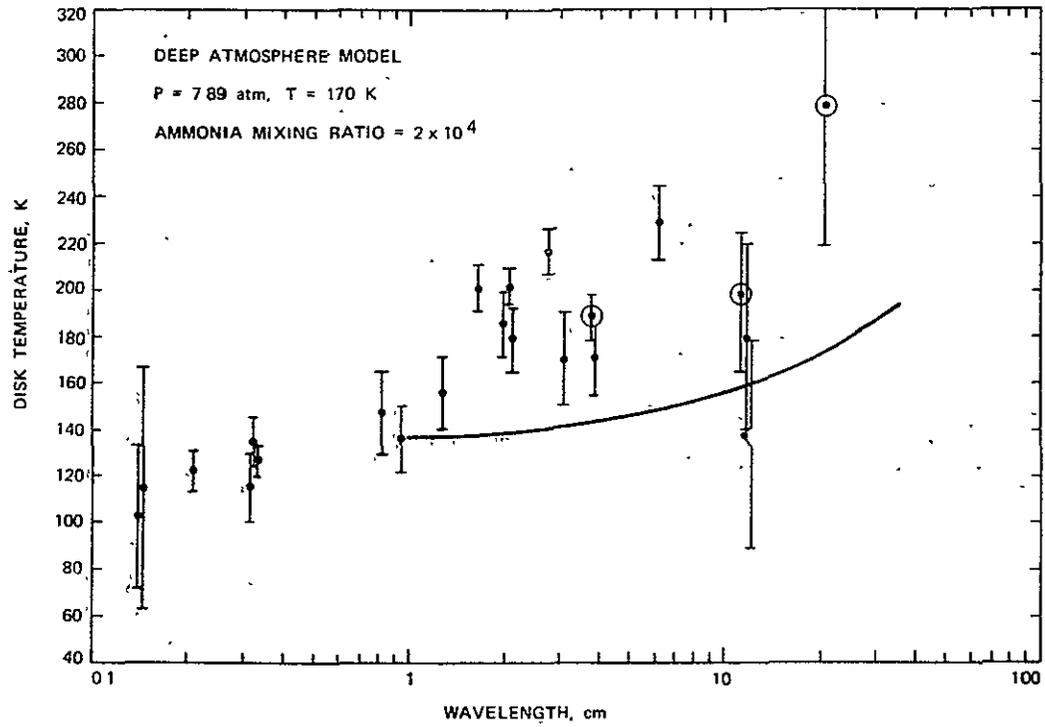


Figure 2-10. Uranus disk brightness temperatures.

The basic problem is that Uranus is much too warm for any model like this. All of the measured brightness temperatures are above the model.

DR. STONE: Sam, does what you assume below ten bars make any difference at all in that curve?

DR. GULKIS: What do you mean by changes? Every time you change the model, you change the results. Do you mean changing the gravity by ten percent?

DR. STONE: No; changing the lapse rate.

DR. GULKIS: If you change the lapse rate, you change this, yes.

DR. STONE: I am talking about lapse rates at pressures higher than ten bars.

DR. GULKIS: It will change, but only very slowly. Let me try and show that with some of the later figures.

Some of the parameters which I have changed are: I have changed the entire pressure structure by a factor of 10 and I have changed the mixing ratio by a factor of 20.

DR. POLLACK: You are using an adiabatic lapse rate; is that correct?

DR. GULKIS: That is right.

DR. POLLACK: So any change that you would come up with, Peter (Stone) presumably, would be a less steep lapse rate which would make the curve even flatter.

DR. GULKIS: Yes.

DR. STONE: This doesn't have anything to do with dynamics, but, yes, you are right.

DR. WALLACE: Why don't you calculate curves between 1/10 and 1 centimeter on Figure 2-10?

DR. GULKIS: I show that on my last figure. I didn't include it on Figure 2-10 because at the shorter wavelengths hydrogen opacity is important; I did not attempt to include that until my last computer run which was on September 27. I will show it later.

Belton, et al., computed a brightness temperature at one millimeter, and it was 100°. And my computation at the same wavelength was 88°.

Now getting back to the longer wavelengths, the cosmic abundance model does not explain the radio observations. It suggests two things. One is that there is either less opacity in the upper atmosphere or there is an additional source of emission.

DR. BELTON: Or a radiation belt?

DR. GULKIS: That would be a possibility. I might add that the interferometer measurement at 3.8 cm wavelength found that a uniformly bright disk fits the observed emission.

There appears to be no excess radiation coming from outside the disk.

DR. LEWIS: Is this supported by polarization measurements, too? Can you see the polarization?

DR. GULKIS: No. Nobody has ever seen any. The measurement is hard to make, but I wouldn't rule it out.

DR. LEWIS: Is there only one interferometric measurement?

DR. GULKIS: There is only one interferometer measurement where they actually measured the disk. These measurements up here (at 11 and 21 cm) which were interferometer, didn't have enough resolving power to resolve it.

DR. OWEN: But if you use Jupiter as a model, when you get down to a couple of centimeters, you are really not getting much contribution from the radiation belts anyway.

DR. GULKIS: You wouldn't expect so unless the magnetic field were a lot stronger. In the case of Jupiter, that is right. In the case of Jupiter, all of the emission at two centimeters comes from the atmosphere and not from the radiation belts. The radiation belts start falling off at about seven centimeters.

DR. BELTON: In the interferometer, the disk size was the same size as the 25,900 km, something like that?

DR. GULKIS: The uncertainty on that measurement was just about the uncertainty in the table that I put together. If you went to the edge of their error bar - yes, it just included the Danielson number.

DR. POLLACK: Sam, one thing that comes to mind, too, is on your absorption coefficients that you are using in the case of Uranus, you are dealing with higher pressures than you are in the case of Jupiter and Saturn. And I wonder, are the absorption coefficients in your wavelength dependence known at the pressures that you are applying that formula to?

DR. GULKIS: They are known at the pressures but not the mixing ratios, and that turns up to make a difference. There is a set of measurements down in Australia which were done by Morris and Parsons where they went up to 300 atmospheres. And what we did in computing this was to derive the absorption coefficient theoretically, using Ben Reuven line shapes, and then fudge that result to the Morris-Parsons laboratory data. It is about a ten percent correction.

DR. WALLACE: Ten percent is pretty small.

DR. GULKIS: Yes, it is very small; that is right.

DR. POLLACK: In the laboratory measurements, what was the dominant foreign gas?

DR. GULKIS: It was hydrogen. The difference is their mixing ratio is one part in 300.

DR. POLLACK: Well, I wouldn't worry about it as long as it is hydrogen.

DR. LEWIS: In the reduction of the interferometer data on the disk size, did they assume uniform brightness for the disk or did they fold it in with the limb darkening or what?

DR. GULKIS: No, there is no limb darkening folded in. What they actually do with the interferometer is measure the Fourier transform of the brightness distribution. They overlay a uniform disk to that as a model, so that it fits well.

DR. WHITEHEAD: But if the disk is not a uniform value, it would make a big difference, wouldn't it?

DR. GULKIS: It depends on how non-uniform it is.

DR. WHITEHEAD: Partly to be considered is the fact that for much of the time period that these observations were taken one of the poles was pretty much being illuminated most of the time. It was that season of the Uranus year when a big chunk of it was seeing sunlight all the time; that would affect it.

DR. GULKIS: If you had, say, very, very strong limb darkening where the planet appeared to be 3/4ths of its size, that would show up very strongly. You would see that it wouldn't fit the uniform disk model.

DR. JOHNSON: I think it is most important to find out what the real radius of Uranus is. You can still tell the difference between that and radiation belts even at that type of resolution, I should think.

DR. GULKIS: The measurements very definitely rule out anything like Jupiter's radiation belts. But it is conceivable that you could have a very limited radiation belt right above the ionosphere and it wouldn't show up.

DR. LEWIS: But again, at two centimeters on Jupiter, you wouldn't see the belts at all.

DR. GULKIS: You have to change the parameters, but you can shift the synchrotron spectrum by either increasing the energies or increasing the magnetic field.

Let me show an ionospheric emission model. Such a model would still be consistent with the interferometer results.

Let's look at Figure 2-11. If you assume that Uranus has a very dense ionosphere, which is overlaying the atmosphere which I just showed you, and computed the spectrum one can come up with a very good fit.

The problem here is the same as it has been in the case of both Jupiter and Uranus. The emission measure has to be up 10^{24} , close to 10^{25} .

DR. POLLACK: What is it for Jupiter?

DR. GULKIS: 10^{20} maybe.

DR. POLLACK: There is your challenge, Don Huntten, I think.

DR. GULKIS: Can you do that, Don? I rule this out as a pretty implausible model.

DR. HUNTEN: I agree, even with a pure sodium atmosphere.

DR. LEWIS: It is easier with cesium, Don.

DR. GULKIS: One does get a good fit, and I thought I would point that out.

Let me go now and look at some of the things that you have to do to the atmosphere to make it fit.

DR. GULKIS: In order to see deeper in the atmosphere, can you simply reduce the mixing ratio? Figure 2-12 is similar to the last two curves. This is the original curve which I showed you with a mixing ratio of 2×10^{-4} . I am reducing the ammonia mixing ratio by a factor of 20. Essentially, no improvement. You simply cannot take the ammonia out of that model and get it to fit the observations by doing it that way, and it really is because it is optically thick in here. This is the problem area. By taking the ammonia out down deep, you don't change anything.

DR. POLLACK: What is the pressure and optical densities here?

DR. GULKIS: This is about 3 bars. I conclude from this that changing the mixing ratio is not the answer.

Figure 2-13 is a reduction in the pressure by a factor of ten. I really need other people to help me rule this one out. I would guess that if you reduce the pressure by a factor of ten that you are now violating all of the other measurements which say that the hydrogen abundance is as great as it is.

Certainly, a reduction in pressure helps to bring the model back in line. It brings up the curves quite a bit steeper.

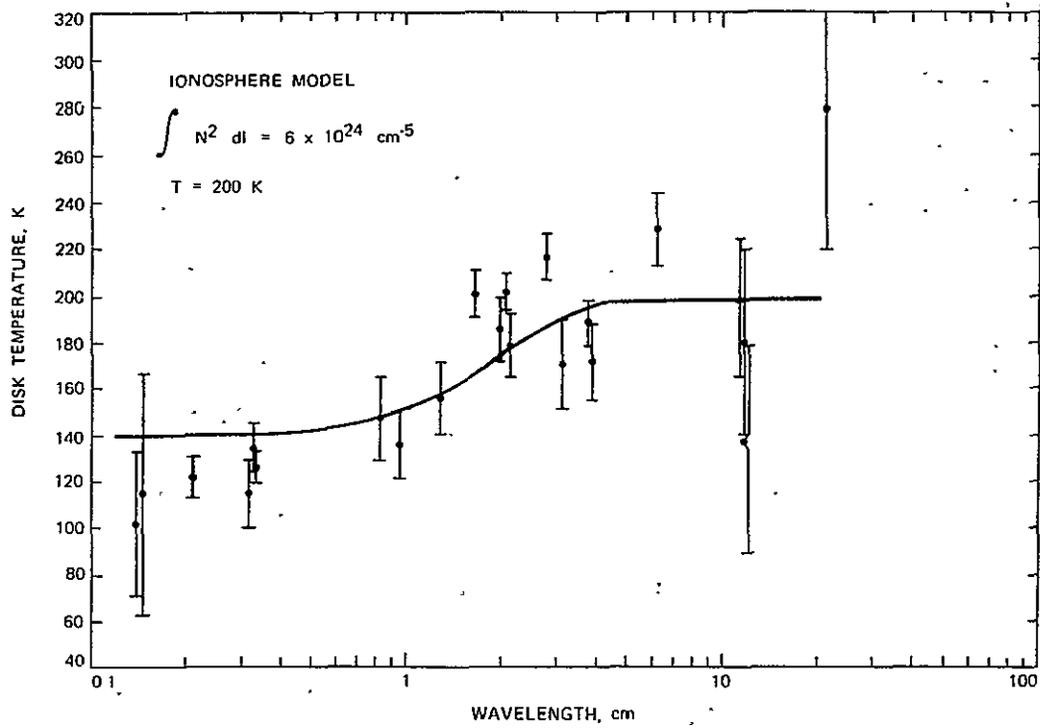


Figure 2-11. Uranus disk brightness temperatures.

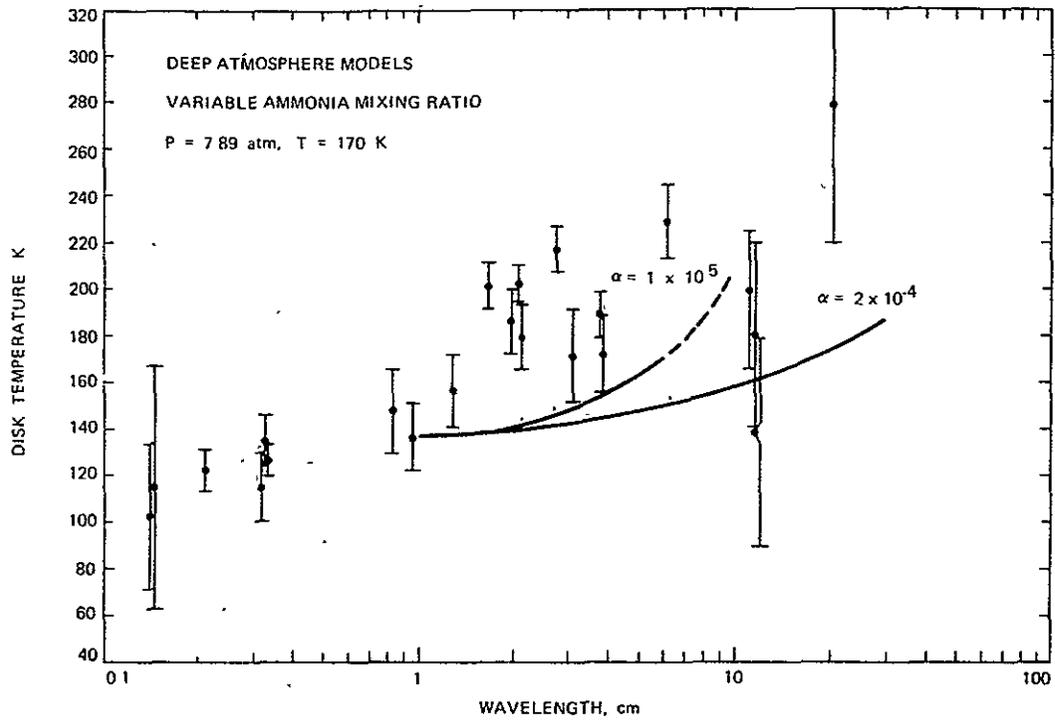


Figure 2-12. Uranus disk brightness temperatures.

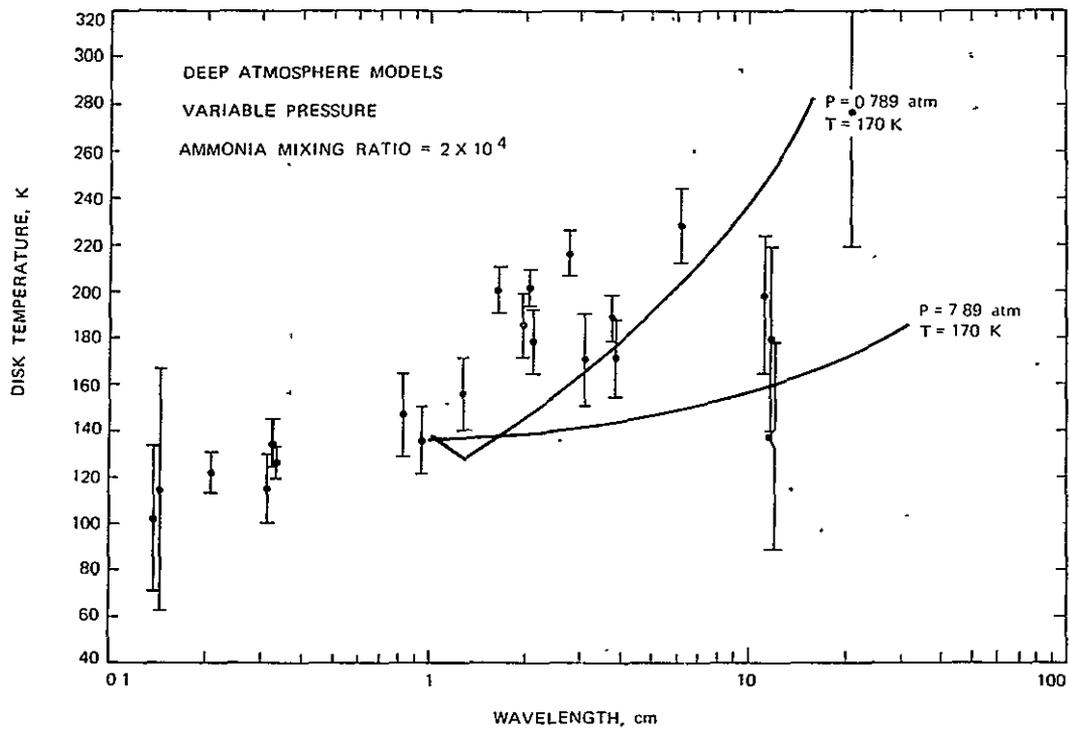


Figure 2-13. Uranus disk brightness temperatures.

DR. STONE: What do you mean by reducing the pressure here? I mean, what in terms of temperature-pressure profile?

DR. GULKIS: You see, I have the same profile but, at one particular point, which I have just chosen to be 170° , it is .789 atm and 7.89 atm.

DR. STONE: You just scale the whole curve up.

DR. GULKIS: Just scale the whole curve up.

DR. WALLACE: The problem with your pressure of 0.8 atmospheres and temperature of 170° is that combination says the effective temperature is something like 170° and it is much too high.

DR. POLLACK: That is right. There is another way of saying that and it is that that pressure and temperature point would be perhaps close to what you picked in the case of Jupiter, so that gives you some feeling of how far off you are.

DR. WALLACE: But I would guess at that pressure the temperature would be more like the effective temperature. So I would tend to think that the lower pressure-temperature point made more sense to me. But I agree it doesn't help the observation any.

DR. GULKIS: I have tried the pressure and the mixing ratio, I think, without success. The next thing that I came up with occurred to me as I was reading a paper by Prinn and Lewis. They suggested that Uranus might be overabundant in sulphur in which case the H_2S reacts with the ammonia and is a very effective way of drying out the ammonia in the upper atmosphere. And so, I thought rather than using the ammonia saturation curve, that I would try to use the ammonium hydrosulphide curve. On Figure 2-14, I have pressure along the abscissa, and altitude is measured from some arbitrary zero. Shown is the pressure of ammonia and the constant mixing ratio region on the right. The hydrogen pressure is running along on the right.

In the first model which I tried, I went along the curve up until the point where the ammonia saturated, at about 160° . And then, I followed along the saturation curve.

The thing which I tried next was to run up the curve until I got onto the ammonium hydrosulphide curve and set that curve by taking the H_2S pressure and the NH_3 pressure to be equal. If you don't do that, it shifts around a little bit. But I took them to be equal, and I followed it down to some arbitrary point. I don't know if I have to do this, but I did it because I thought that I needed a little bit of high altitude ammonia to make the spectrum meet. So I then jumped back onto the ammonia curve. If there is some way of getting rid of the H_2S high in the atmosphere then you would naturally fall back onto this curve.

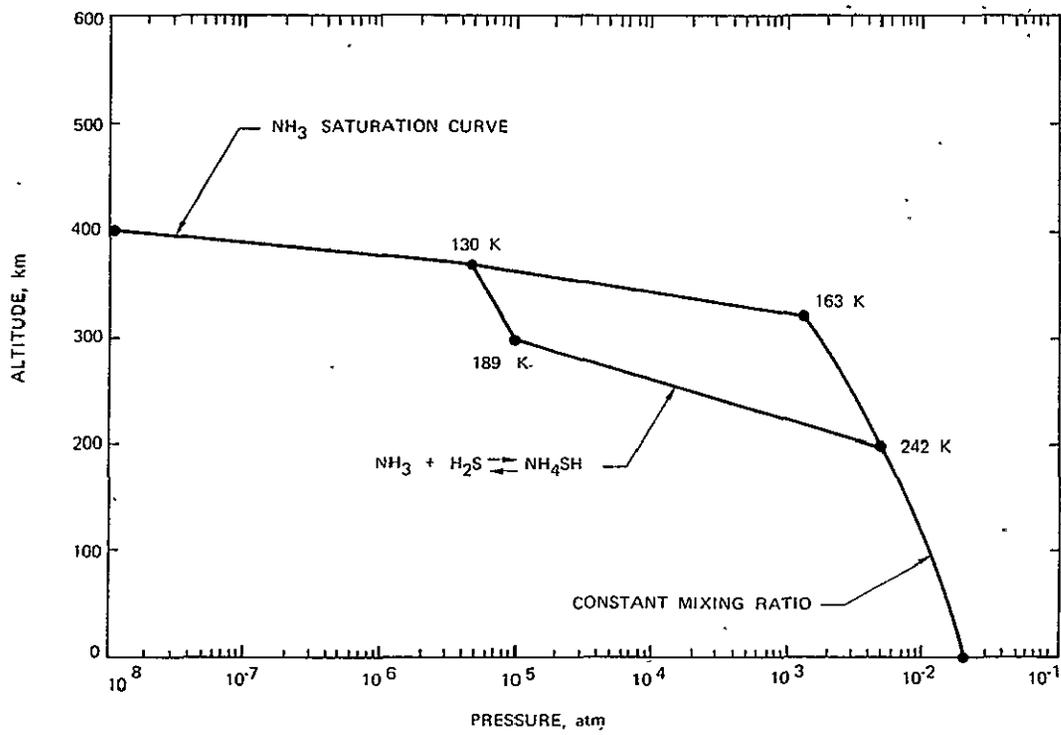


Figure 2-14. Uranus Atmosphere – Ammonia Distribution.

DR. LEWIS: If your H_2S abundance is 99% of the ammonia abundance, you get a behavior just like that. The probability, of course, of hitting that particular ratio is slim.

DR. GULKIS: Figure 2-15, from 1 centimeter on out, shows you what happens when you follow the ammonium hydrosulfide curve. You bring the continuum level up into this region by removing the high altitude ammonia. I think that I would have liked to have seen it rise a little bit more, but I haven't fudged it. I just kept it the way it came out.

On the lower wavelength end I also now added what I thought was the translational opacity due to hydrogen, and one gets the curve shown.

You can see what is really happening is the effect of the hydrogen goes on up here, and the brightness temperature would continue to rise and it merges in. So, it is included all the way across now.

DR. WALLACE: That is very nice to see. It kind of had me worried. I couldn't picture in my head what was going to happen in that transition region, but it looks as if there is no big problem.

DR. GULKIS: The ammonium hydrosulfide is condensed.

DR. BELTON: The second question is: What about the peak, just below 2 cm, that the data seem to suggest in Figure 2-15? Is there some other substance that could be showing up at that wavelength?

DR. TRAFTON: Did you use quadratic extrapolation of the pressure-induced profile?

DR. GULKIS: Yes. I didn't derive this absorption coefficient. I actually got it from Goodman, from his thesis, and he said that he derived it from you. But I did cross-check the numbers I used against some figures that you had given, and they seem to fall in within a factor of 5.

The temperature dependence is also important.

DR. BELTON: I have two questions. One, I didn't quite hear where you got the information on the opacity when you use ammonia and H_2S ?

DR. GULKIS: No opacity due to the H_2S . I only used that as a drying agent to dry out the upper atmosphere.

DR. BELTON: Oh, I see, it is still just ammonia.

DR. GULKIS: Still just ammonia. This model has ammonia in it and hydrogen in it, and that is all.

DR. BELTON: Does NH_4HS have any opacity down there? Would you expect it to have? You would expect that molecule to have opacity, wouldn't you?

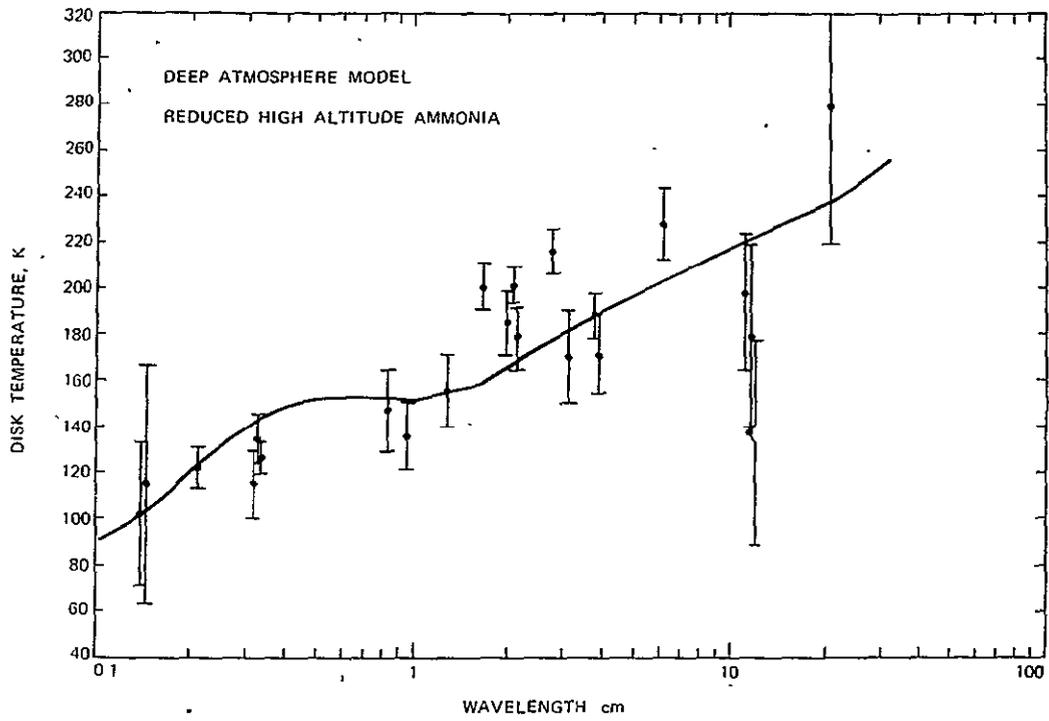


Figure 2--15. Uranus disk brightness temperatures.

DR. GULKIS: The problem I have had with that is the pressures are so great where all of this is taking place that I find it difficult to make anything change within a small wavelength interval without reducing the pressure.

DR. BELTON: It is impossible without something like a low pressure, something from the stratosphere.

DR. HUNTEN: Remind us which are the three best points.

DR. GULKIS: (3.8 cm, 11.1 cm, 21 cm)

DR. WALLACE: What do you really think the observations are saying here?

DR. GULKIS: Those 3 points are recent.

DR. WALLACE: So, you wouldn't go out of your way to draw a hump through that pattern.

DR. GULKIS: No, but I can't really exclude any of the data. They are all valid data.

DR. WALLACE: Yes, but you wouldn't go out of your way to draw a hump in it, I gather; you would be happy with some kind of a smooth -

DR. GULKIS: Well, yes, for right now, I am happy with this. I would say that is about all the data would bear. If we had some good measurements at longer wavelengths and knew that it went up, that would be very important.

DR. OWEN: That is a pretty easy region to work in, isn't it?

DR. GULKIS: Yes, it is fairly easy to work in. We should see a very great improvement in here.

It is vastly different from Jupiter because the first model which I showed you fit both Jupiter and Saturn, and the identical model here is off by a long way.

DR. POLLACK: What sort of sulphur content relative to ammonia do you need?

DR. GULKIS: It is underabundant by about a factor of five. Presumably, if you bring it up by five, you get the conditions that I am talking about. But I don't know how sensitive it is; I don't know whether three would do it.

DR. POLLACK: You mean five times as much sulphur as the cosmic abundance in order to do that?

DR. GULKIS: Well, I would say five times as much as the nitrogen. Is that right, John?

DR. LEWIS: Up by a factor of five relative to nitrogen.

DR. GULKIS: Relative to nitrogen.

DR. LEWIS: I wouldn't favor this particular compositional model as being plausible.

DR. GULKIS: This region right here is why I jumped back to the ammonia curve. I thought I had to jump back to it. I think had I followed the ammonium hydrosulfide all the way down, it would have probably come in maybe across here and that might still be an acceptable fit; I don't know at this point.

DR. OWEN: At lower levels where the temperature is quite high, wouldn't you also get some scavenging of the ammonia by water? So you would have to look at the ammonia -

DR. LEWIS: Not at the 260° level.

DR. OWEN: That is what I am talking about. The end point there is at 280°, if you believe it, and I am just wondering if that curve might begin rising a little more steeply.

DR. POLLACK: Oh, it could, yes.

DR. OWEN: As you get down to these lower levels.

DR. GULKIS: All the models of this type will start rising steeply at the longer wavelengths. They all do it. Unfortunately, I don't think that we can get any earth-based measurements out in this region because it is too hard. You really have to get them from a spacecraft.

DR. LEWIS: However, if the scavenging by water is important, that means that the mixing ratio of ammonia, as ammonia evaporates, is even higher than in Sam's model. That means even more opacity.

DR. OWEN: But that is deeper still.

DR. GULKIS: The opacity due to the water is negligible.

DR. LEWIS: What about a gaseous hydrogen-sulphide if you do not have an excess of H₂S over ammonia? Not that I mean to advocate it.

DR. GULKIS: I haven't investigated that possibility.

DR. LEWIS: A gaseous H₂S in the upper atmosphere would reach out and absorb everything.

DR. OWEN: Well, it wouldn't get into the upper atmosphere, though, would it, because it has a vapor pressure limitation also.

DR. LEWIS: It is slightly more volatile than methane.

DR. HUNTEN: Does H₂S have a microwave absorption?

DR. GULKIS: Yes, it does, out at shorter wavelengths, but I don't know how strong it is.

DR. POLLACK: It wouldn't hurt to have one centimeter tested.

DR. GULKIS: There is nothing at one centimeter, no.

DR. OWEN: But, again, this model doesn't say you have gaseous H₂S left over, does it?

DR. GULKIS: No.

DR. OWEN: So what you have got is the ammonia-sulphide plus some residual -

DR. POLLACK: Well, the point is that you have to have the H₂S abundance almost precisely identical to ammonia to get both of them to stay in the cloud -

DR. LEWIS: You are picking two numbers on logarithmic scales which span several decades and you ask that those two numbers come equal to each other.

DR. POLLACK: That is why you tend to think that, if you want to go this way, probably what would be more reasonable to do would be to have an excess of H₂S and then you have to ask what are the consequences.

DR. GULKIS: I just might remind everyone again that I am not saying that H₂S is there. I am saying that the upper atmosphere is very much more transparent than any of the cosmic abundance models would predict, and that we are seeing deeper into it, if this is atmospheric emission, and that this is one thing that seems to work. Because it takes the ammonia out at the right places. And that is pretty hard to do, I think. You really have to take it out right in this region where the temperature is still pretty low and the ammonium hydrosulphide works nicely because it starts dropping off at 240°.

DR. WALLACE: There is perhaps a totally different approach here, now. If the internal heat source on Uranus is small or negligible, then that steep temperature rise which shows up in the Trafton-type models might not really occur. That is, if the solar flux, which is deposited in the atmosphere, is deposited relatively high, it is vaguely possible that the atmosphere just might roll over and go isothermal instead of going convective. It is possible. I don't know, but it is something that Mike Belton has been toying with in his mind for a long time and it needs pursuing.

But if something wild of that sort were happening, it might mean that the temperature would not increase so rapidly as the pressure increases. It might not go convective; maybe. So that the temperature profile in the atmosphere just may not increase so rapidly.

DR. GULKIS: Another equally good fit to this data could be made by removing ammonia and running into a solid surface at 200°⁰, which would make the temperature level off at the same value.

DR. TRAFTON: But isn't there enough radioactive material present in the interior of the planet to assure convection? Do you know, John?

DR. LEWIS: My recollection, going back to the old days in which Bill Hubbard was just beginning to work on this a little bit, the radioactive heat source is about 10^4 , 10^5 times smaller than the solar constant at Jupiter, so -

DR. STONE: You could get an isothermal layer, yes.

DR. HUNTEN: Suppose you run into a liquid surface, what becomes nicely liquid at 200°⁰ Kelvin?

DR. LEWIS: You ought to have a vapor pressure of ten bars at 200°⁰ Kelvin.

DR. BELTON: You could put in another gas, as well.

DR. LEWIS: Methane?

DR. BELTON: Or something else, to effectively thin down the ammonia.

DR. HUNTEN: It would be interesting if you could run up a model like that.

DR. GULKIS: It is very easy to do. The conclusion you draw, of course, is still the same but you have got to deplete the upper atmosphere down to the 200°⁰ level and then, if indeed you would just go on up in here and straight out - I will run one of those.

DR. WALLACE: Well, there is another thing you could do. You could take a brush and draw your fit through the data; pretend you are God and just sweep it through, and then say that there is nothing tricky on the chemistry happening, it is just ammonia and hydrogen, and deduce from that what you guess the temperature pressure profile looks like. That might be amusing.

DR. BELTON: You mean do an inversion?

DR. WALLACE: Do an inversion.

DR. GULKIS: It is not unique, though. And that is the problem. I could come up with a whole spectrum of models. The one I showed you in which the pressure was simply reduced is one model which comes close. And I could do other models.

DR. TAYLOR: Lloyd (Wallace) is suggesting you fix everything but the temperature?

DR. STONE: Sam, if you look back at your Figure 2-12 where you had the conventional Jupiter-Saturn theory plotted with the various radio emissions, there is some wavelength there where they start to diverge. If you assume that, then, all right, we do understand what is going on to the left of that, what parts of the atmosphere can you say are the ones that have to be in error to give us that divergence? Can you or can't you say?

DR. GULKIS: Yes, I would have to look at that. The question really centers around whether or not I needed the ammonia distribution which I actually used. Do you remember the saturation curves which I showed you and I showed it dropping back onto the ammonia? (Figure 2-14).

If, indeed this is the right representation, everything above that, pretty well fits what we know.

DR. STONE: So what you are saying is essentially down to the 130° level, the figures look all right, and we can sort of understand what is going on down to that level.

DR. GULKIS: It is not inconsistent.

DR. STONE: And it is below that level that the problems arise.

DR. GULKIS: Yes. We are really removing all of this ammonia here.

DR. STONE: So you are getting down to fairly high pressure at 130°.

DR. GULKIS: Yes. The abscissa on Figure 2-14 is essentially ammonia partial pressure.

DR. STONE: So at 130°K the total pressure is 6-7 bars.

2.3 AIRCRAFT OBSERVATIONS OF JUPITER'S ATMOSPHERIC STRUCTURE

James Pollack

Let me begin with Figure 2-16 and review the variation of the pressure-induced opacity of hydrogen with wave-number. The opacity is much flatter when the absorbing hydrogen molecule collides with a helium atom (H_2-He) than with another hydrogen molecule (H_2-H_2). The other point to notice is that in hydrogen-rich atmospheres, we have more opacity near the $J=1$ line, which is the line at 587 cm^{-1} , than you do near the $J=0$ line, which is the one at 354 cm^{-1} .

DR. HUNTEN: This is at Jovian temperatures?

DR. POLLACK: Yes. The opacity curves refer to a temperature of 130°K . Now the observations consisted of taking spectra in the 16 to 40 micron range with approximately a resolution of a half micron at the longer wavelengths and about a quarter of a micron at the shorter wavelengths.

The experiment itself was done by Jim Houck of Cornell University and I did the theoretical analysis of the observations.

Figure 2-17 presents the observations. You can see there is a minimum in the brightness temperature at 28 microns, at the center of the $J=0$ line and another minimum near 18 microns, which is close to where the $J=1$ transition occurs.

Now there may actually be a slight but significant displacement between the center of the $J=1$ line and the 18 micron brightness temperature minimum. The way that I interpret this apparent shift is that optical depth unity is passing through the tropopause, the temperature minimum, into the stratosphere near the center of the $J=1$ line. In a similar fashion, but to a much greater degree, we can expect that in the case of Uranus with Wallace's models, the brightness temperature spectra will show an emission maximum at the position of the rotational lines. In the case of Jupiter, this effect shows up only very close to the center of the $J=1$ line.

As was pointed out, there is a feature, evidently in our spectra near 23 microns. There are several possibilities that we have for explaining it. One possibility is that it could be due to silicate dust in the atmosphere. Silicate dust has a restrahten feature near that wavelength with a similar width.

Another possibility which is quite intriguing, if it were true, is that the observed feature is due to one or several organic compounds. A number of organic molecules display features in the 23-micron region.

Which of these is true? We don't really know at this point.

DR. OWEN: Can you check whether the observed feature is due to silicate dust by looking for the features at shorter wavelengths?

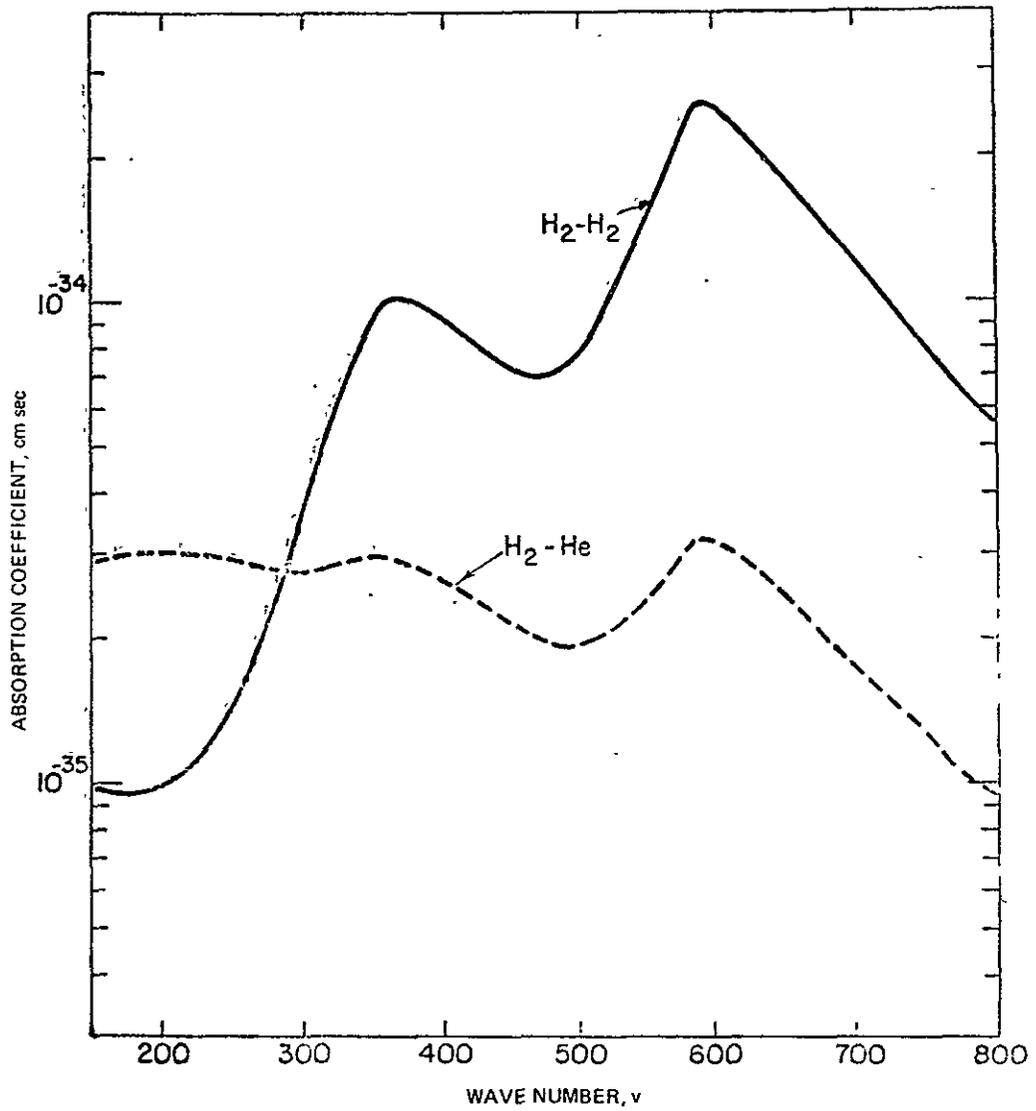


Figure 2-16. Pressure induced opacity of hydrogen.

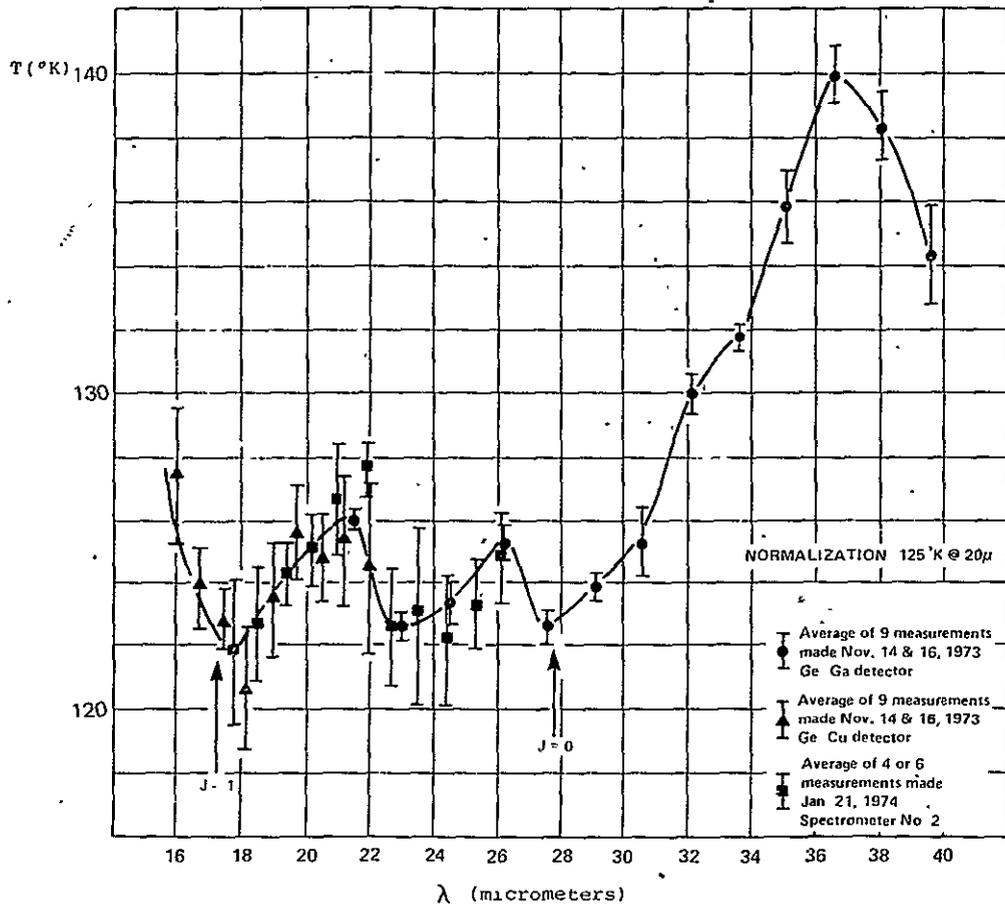


Figure 2-17. Brightness temperature of Jupiter.

DR. POLLACK: Yes. There should be a second reststrahlen feature near 10 microns. Unfortunately, as you know there is a lot of ammonia opacity, in this spectral region, and that makes it very difficult to detect this second feature. Steve Ridgway, at Kitt Peak, has some very nice data at the 10 micron region and who has been making statements that he needs additional opacity. Presumably, the 10 micron dust feature could be consistent with that requirement, although I am not sure in detail whether it would be or not; thus, in the case of silicate dust, the way one would try to confirm its presence would be by carrying out a careful analysis of the ten micron data.

I will now describe the procedure for analyzing the aircraft data. What we do is to pick a given helium-to-hydrogen ratio and with that ratio we perform an inversion of the flux information to derive the vertical temperature/pressure structure of the atmosphere. We do this for a number of hydrogen-to-helium ratios, and then we can, in fact, get some information on that parameter by determining the ratio for which the final flux residuals are a minimum. When I speak about flux residuals, what I mean is the difference between what we calculate for the best temperature/pressure profile we can fit to the data versus what the actual observations are. So, in fact, it is a measure of the difference between the observations and the inversion for a particular hydrogen-to-helium ratio.

As you might expect, in the case of very helium-rich atmospheres, we get enormously high flux residuals which would indicate that Jupiter's atmosphere is not very helium rich.

DR. BELTON: Would you please remind me what the center of the absorption lines are?

DR. POLLACK: Yes. The S(0) is at 28 microns, so in fact, one of the brightness temperature minima occurs at this location. In the case of the S(1) point that I was referring to earlier, it is actually slightly to the short wavelength side of a second temperature minimum....

DR. HUNTEN: I was squinting at the error bar, Jim.

DR. POLLACK: Right. One must allow for the error bars in assessing the reality of the apparent shift.

DR. BELTON: I suppose that the center is set at 17 microns.

DR. POLLACK: Yes.

DR. WALLACE: Gee, that is not -

DR. POLLACK: Yes, I agree that we need to do more work there.

DR. WALLACE: No, I think it is beautiful.

DR. POLLACK: Oh, okay.

DR. TAYLOR: What exactly are the error bars, Jim? Are they the one sigma within the sample or is it instrument noise?

DR. POLLACK: They effectively are just the random internal errors, based upon what the standard deviations we derive from measurements made at the same wavelength at different times. So they are just random internal errors.

The absolute calibration, I think is based upon observing Mars in the 20-micron region. But that would have the effect of moving the whole curve up or down without changing the shape of it.

DR. TAYLOR: On the bars, are there statistical variances within the sample? Is it one sigma?

DR. POLLACK: Yes, they are all one sigma. Let me see if they are. Yes, they make sense. They should be on the order of about a degree or so.

Returning to the question of the hydrogen mixing ratio, we have carried out a statistical analysis of the flux residuals and find that the fraction by number of the atmosphere that is H₂ and .9 plus or minus .1.

Thus, we find that the hydrogen mixing ratio is, indeed, very close to the solar value.

Now, Figure 2-18 shows you for .8, .9, and 1.0 hydrogen mixing ratio by number, the results of our temperature inversion, with the results joined to measurements made on either side of the region of vertical space that we actually sensed. We sensed from about the .1 atmosphere to roughly the .6 atmosphere pressure level and that is indicated by the vertical lines on the graph. So you see, we slightly go through the temperature inversion.

On the left-hand side we join our results onto inferences of the temperature structure that have been obtained by Glenn Orton from methane band spectral observations in a similar way to the way we proceeded and the results are very consistent with one another such as the value of the temperature minimum. Our temperature minimum is about 115° and Glenn obtains a value of about 118°. So, in fact, they are in very good accord.

It is this sort of consistency that tends to enhance my belief in the formal results.

Considering now the higher pressure portion of our sensed region, we find that the lapse rate becomes adiabatic at approximately the .4 atmosphere pressure level.

I might say, by the way, that the temperature minimum is about at the .2 atmosphere pressure level.

So the convection zone begins at about the .4 atmosphere pressure level, which, I think, is consistent with the various equilibrium calculations

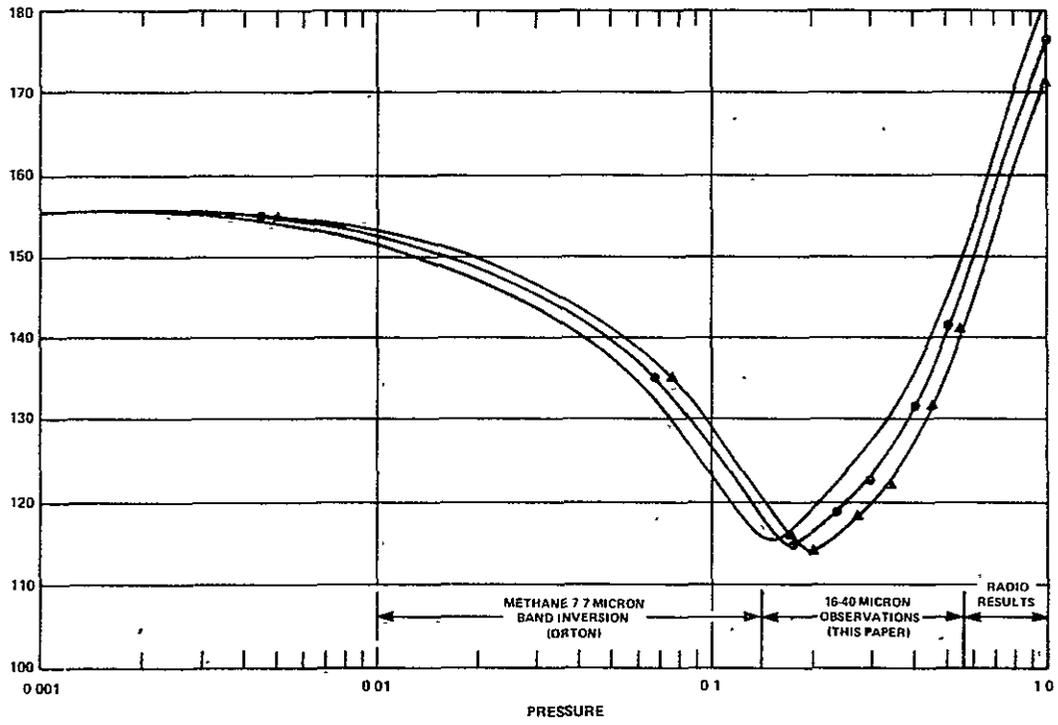


Figure 2-18. Jovian temperature inversions for three hydrogen mixing ratios.

that have been done. I know that it is very close to the value that Ohring and I found when we computed our model atmosphere.

DR. BELTON: It looks as if it is right on Trafton's values, too.

DR. POLLACK: The reason I mentioned the calculation by Ohring and myself is that in our radiative equilibrium calculation we included ammonia opacity whereas Larry didn't in his calculations.

DR. WALLACE: Is it very important, though?

DR. POLLACK: It does change the location of the convection zone slightly in the sense of bringing it to somewhat lower pressures.

Another interesting side point to our derived profile is the fact that the effective temperature of Jupiter just due to the amount of solar energy it absorbs is 102° so if the temperature minimum is 115° , it is another proof, if you like, that Jupiter is emitting excess radiation, in case somebody doesn't believe that by now. One other statement that is relevant for our later discussion, is as you can see from the case of Jupiter, observations of this type are very valuable in providing information both on the temperature structure and on the hydrogen-helium ratio. Naturally, the same types of measurements can be applied to other outer planets. We are hopeful of being able to perform such measurements on Uranus this coming February and March from the C-141. However, it would be very useful if this group gave an endorsement to this possibility as a way of emphasizing and its importance to the people who will be scheduling the C-141.

DR. BELTON: I think it is one of the most important things we need to do.

DR. HUNTEN: We can all easily agree to that. Any more discussion?

DR. WALLACE: I think the brightness temperature plot (Figure 2-17) showed a drop in temperature at 40 microns. Is that true? Will that fit the models? I didn't think they would.

DR. POLLACK: The way I would interpret the drop here is due to ammonia opacity.

There is a slight problem from the theoretical point of view and that is this: the only ammonia opacities that are available right now are those by Gille. Gille gets his ammonia opacities by taking the sum of the strength of the lines that are present in a given spectral region. Unfortunately, the spectral region near 40 microns is right at the tail of where the ammonia opacity really starts up, so that the short wavelength tails of the stronger lines may, in effect, be more important than the lines whose centers are located here.

DR. WALLACE: I thought someone had done some work on the ammonia opacity since then, either Dr. Trafton or Dr. Taylor.

DR. TRAFTON: I did some theoretical work on it. You are talking about the rotational model?

DR. POLLACK: Yes.

DR. TRAFTON: And Goodman looked at the pure rotational aspect.

DR. POLLACK: Have you done anything on pure rotational band?

DR. TRAFTON: No. When I have used rotational opacity, I have used Goodman's model, which I think is still unpublished.

DR. WALLACE: But it is in a thesis?

DR. POLLACK: Yes, under John Dickel. Does he do a better job, do you think, than Gille does?

DR. TRAFTON: For the rotational band, it is my memory that he does; but he doesn't make any basically different assumptions. I don't think there is that much difference between them.

DR. POLLACK: Because, as I say, the problem that I run into is that it is inadequate to simply sum the strength of lines within a region; allowance should also be made for the tails of strong lines lying outside the interval under consideration. Does Goodman actually do that?

DR. TRAFTON: He assumes Lorentz tails.

DR. POLLACK: But he does take tail contributions into account?

DR. TRAFTON: My memory is yes, he does. You should consult his thesis.

DR. TAYLOR: What you need are individual line parameters, then you can do anything you like with them.

DR. WALLACE: The other question was when you calculate your models for that purpose, do they actually look good in comparison when they are overlaid on the data? Do you follow me?

DR. POLLACK: Yes, I understand your question. I haven't plotted them up yet, but I am going to do that and that is certainly a relevant question. I can say this, that the mean fractional difference between our predictions and the observed fluxes is something like 4%.

DR. WALLACE: That is very good.

DR. POLLACK: So that is another thing that gives me confidence that what we are doing makes sense.

DR. TAYLOR: This is a remark rather than a question, I think that it hasn't been brought up before and I think it is very interesting and important

that your data agree rather well with the kind of things you would expect with other work like Lloyd (Wallace) without the inclusion of aerosol opacities.

DR. BELTON: By the way, Pollack did ask for support from the group this morning.

DR. HUNTEN: There has been no dissension from the position that we would like to see some measurements made of Uranus by the C-141 airplane?

DR. WALLACE: It is very important, but I think we ought to encourage all of the infraredders in the general scientific community to consider this problem, and pray that they get as much support as they can use. It is probably the most important thing that can be done.

DR. POLLACK: The only reason I specifically asked for some degree of support from this group for the C-141 is just that the scheduling is coming up very shortly and this will help insure that we get some time when Uranus is available.

DR. REYNOLDS: As a matter of fact, I have spoken with Bob Cameron and he has assured me that this will be a high priority item to be considered in their schedule. So, it is important.

DR. HUNTEN: It seems to be the unanimous opinion of this group that that would be a very, very good thing to do.

2.4 INTERIOR COMPOSITION AND STRUCTURE

John S. Lewis

The interior of Uranus is a very easy subject to summarize, since the sum total of our knowledge is astonishingly small.

First of all, let me say a few words on the bulk density of the planet. The bulk density of Uranus, as you all know, is incompatible with solar composition. It requires that Uranus be composed of material which is intrinsically, substantially denser than solar material. From this point of departure, one turns to the cosmic abundance tables, with the requirement of producing reasonable compositional models of the planet. These compositions are then used to generate structural models for the interior, using equation-of-state data on those compounds which are the most abundant constituents of solar material, after the hydrogen and helium have been depleted.

Several published models of Uranus and Neptune have been circulated for some years now. Unfortunately, the revisions in the radii of Uranus and Neptune a few years ago left essentially all those models stranded high and dry. Makalkin (1972) and Podolak and Cameron (1974) have published recent models, and Reynolds and Summers have revised their earlier computed models for Uranus and Neptune (1965) to fit a radius of 25,000 kilometers, which is a close approximation to the specific numbers which are now discussed. They have treated the internal structures in a rather general way so that the minimum number of limiting assumptions are made regarding the internal structure.

Figure 2-19 gives their three models of Uranus. I think you will notice that they are remarkably devoid of detail. This is an advantage because the simpler the assumptions, the easier it is to construct a quantitative model.

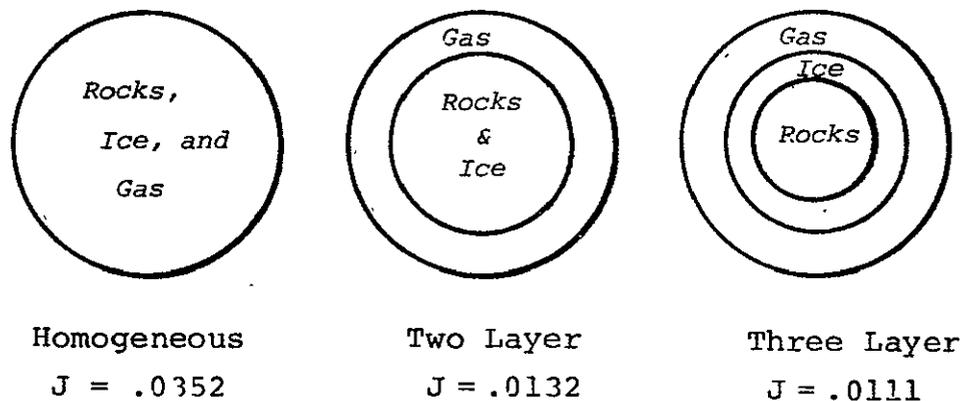


Figure 2-19. Structural models of Uranus.

We see on the left the homogeneous model for Uranus and in the middle the two-layer model for Uranus. For those of you who have a flair for the complex and the highly sophisticated, there is also a three-layer model for Uranus shown on the right.

How do we arrive at one, two or three layers? If one sorts the abundances of the most abundant elements of the sun - (hydrogen, helium, oxygen, carbon, nitrogen, neon, iron, magnesium, silicon, sulfur, argon, calcium, aluminum, sodium, nickel) - into classes according to volatility and according to the types of materials formed, whether it be rocky material or icy material, or gas; then they fall quite nicely into the three groups of Table 2-4. The first of these groups is what we might call, following the terminology of chemists of a century or two ago, the permanent gases, hydrogen, helium and neon. Second are the condensable gases of what we might call ice-forming elements - water, methane and ammonia. Of course, the division between these categories depends on the temperature regime being considered; there is no complete dichotomization. Then come the rock-forming materials.

Table 2-4. Classes of Solar Abundant Elements

Permanent Gases	Ice Forming	Rock Forming
Hydrogen H ₂	Water H ₂ O	Iron Fe
Helium He	Methane CH ₄	Magnesium Mg
Neon Ne	Ammonia NH ₃	Silicon Si
		Oxygen O
		Sulfur S
		Calcium Ca
		Aluminum Al
		Sodium Na
		Nickel Ni

Among the rock-forming elements the most abundant are iron, magnesium, silicon; and the most abundant of all the rock-forming elements, oxygen, which of course is an essential ingredient of rocks. Next come sulfur as sulfides; then calcium, aluminum, sodium and nickel.

The models that Reynolds and Summers produced back about ten years ago, updated to fit the present radius estimates for both Uranus and Neptune, give the run of pressure versus depth for an isothermal planet for each of the three models above. From each one of these results for the composition and the equation of state of the material versus depth you come out with an estimate for the principal moment of inertia for the planet. One can then compare that result to estimates for the value of J (or J₂, depending upon which constant you use) derived from observations of the satellites of Uranus and Neptune.

For Uranus models we will take the revised results from Reynolds and Summers for the new, larger, 25,000-kilometer radius of Uranus. They put together a certain list of abundances taken from a paper of Cameron's which is about ten years old, and assembled the layers in the planet out of materials from those three different composition classes in the sense that rocks go in the center, then ices, and then gases in the three-layer case; or a homogeneous mixture of rocks plus ice in the center with gases on the outside in the two-layer case; and for the homogeneous case, all three of them mixed together.

The way in which the equations of state of mixtures are generated is that the partial molar volumes of the constituents are conserved, taking individual equations of state for the separate pure components.

Obviously, this is a very simple modeling approach but it does give you estimates of how the rotational moment of inertia of the planet depends upon the way in which it is stratified for a given bulk composition.

For the homogeneous model of Uranus, a value of $J = .0352$ is calculated; $J = .0132$, for the two-layer case; and for the three-layer case, $J = .0111$. Why is there so little difference between the two and three-layer cases? Because the amount of mass which is involved in forming the rocky core is a relatively small fraction of the total mass and changing its radial distribution by this amount has less impact on the structure of the planet than if the homogeneous model were used. Characteristically, the changes in distance from the center for the dense material between the homogeneous and 2-layer models is much larger.

The historically-favored value for J_2 deduced from studies of the Uranus system, is $J_2 = .013 \pm .001$ which seems to fit the two-layer model quite nicely but remember, please, that the hosts of approximation which have gone into this are somewhat damaging to the certainty that you can attach to an interpretation like that. Also, Whitaker and Greenberg (1973) find $J_2 \approx .005!$

Since the work on the structures of Uranus and Neptune by Reynolds and Summers, there have been several minor but significant changes in the data which one would input into the calculations. This is a zero-degree Kelvin isothermal model. So that is another thing where one would like to modify the treatment of the interior structure of the planet.

DR. WALLACE: Wouldn't the temperature be very important?

DR. LEWIS: At pressures of a few megabars the densities are quite insensitive to temperature, actually. This would be far more important in the run of density and pressure versus altitude in the outer part of the planet where the material is still reasonably compressible and the thermal pressure is an important contribution. Not at a couple of megabars. It is just that things are squeezed so close together that you are on a very stiff part of the potential curve.

DR. REYNOLDS: At high pressures the thermal expansivity is low.

DR. HUNTEN: I guess this is equivalent to the question of what you do about an atmosphere. You must define the temperature there.

DR. REYNOLDS: Well, in this case the atmosphere appears to me to be a very small part of the mass of the planet.

DR. LEWIS: Yes, it is a negligible fraction of the radius of the planet.

DR. POLLACK: It is an important correction that is being made. Cameron's models do include temperature and we are working on some improved models that also include temperature.

DR. LEWIS: I think that the detail of conclusions that can be reached from the models that have been done is very limited. I would mostly like to give a shopping list of problems which are in need of solutions before more detailed modeling can be done, in which one can have a high degree of confidence.

There are several factors to consider. First of all, the abundance of helium used in these calculations was a little bit on the high side. If one were to bring that abundance of helium down a trifle, it would change the way in which the total mass of the planet is partitioned between the three different layers.

The second consideration is that all the elements more abundant than iron, magnesium, silicon, calcium, and aluminum were neglected and these tend to be very largely rock-forming elements. Therefore, the mass fraction of rock-forming elements was a bit understated in these models.

Third, there is the important point that the old abundance of iron was used; and that has risen by a factor of five. That is very important.

Next, sulfur was treated as an ice-forming element rather than a rock-forming element, and this choice is by no means clear; it could behave either way. However, if it is treated as a rock-forming element then the effect again will be to increase the amount of rocky-type material.

Of course, if one increases the total mass fraction of rocky material inside the planet, taking a larger fraction of the total mass as rocky material, then the weight fraction of gaseous material, or very low density material, must also be increased at the expense of the ices. All of these corrections would point in the direction of changing the rock-to-ice ratio appreciably, because it is ice that goes down in abundance as the rock and the gas both go up.

Finally, methane was treated as an ice-forming element, which is completely in keeping with the idea of a very low temperature interior for the planet but not necessarily correct if the interior temperature of the planet is moderately warm. In that case, the methane would largely be present in the atmosphere. If that happens, then the total mass of the icy layer would decrease, the density of the gaseous layer would increase and one will also get a shift in the relative abundances of the three different layers.

DR. POLLACK: Can you be a little bit more specific as to at what point you think that would happen?

DR. LEWIS: Temperature?

DR. POLLACK: Yes.

DR. LEWIS: Not really. All I can say is that any attempt to predict phase behavior in a system with a pressure above ten kilobars is almost doomed to failure, even where you have data for non-ideality up to two or three kilobars. You can't push the extrapolation much beyond that.

I intend to spend ten or fifteen minutes on the effects of high pressure on phase equilibrium.

But first, let me summarize my last point. All the effects listed involving behavior of the heavy elements would act in the same direction, namely, if we were to correct the abundance data, this would all have the effect of lowering the value of J deduced for the planet. The opposite would happen if we were to allow methane to join the gas phase, i.e., it would tend to increase the value of J.

This means we should not be terribly concerned about the exact values of J on Figure 2-19. The large difference between the homogeneous model and the two- or three-layer model is approximately preserved, but the exact numbers are changed substantially.

DR. POLLACK: I have a comment, and a question to ask you. The comment is that on your shopping list a very important thing to include is our knowledge of the rotational period of Uranus, which is very poorly known as I understand it. It is based on a very old spectroscopic analysis of the Doppler shift. That is something that it certainly would be very nice to have repeated with more modern instruments.

My question is: Is there any place where the oxygen would tend to get tied up, say all in the water phase, and not be available to oxidize any of the rocky materials?

DR. LEWIS: I don't believe so. That falls into the category of what I am about to talk about. If the question remains afterward, remind me.

We have raised several questions here regarding the effects of higher internal temperatures on the structure and the effects of changing the elemental abundances. Now let us go on to the physical or the chemical behavior of these materials at pressures beyond a megabar, i.e., deep interior pressures. Incidentally, the central pressures deduced for Uranus by these three models are 2.5, 6.4, and 13.5 Mb respectively. Thus, we must ask what is likely to happen in a system which contains these species at pressures of a few megabars.

Now, what temperature range are we talking about? If these are reasonable pressures then what are reasonable temperatures? How do we calibrate our intuition?

Certainly central temperatures of a few thousand degrees Kelvin are entirely possible without the planet being convective or without there being large-scale conductive transport of heat vertically through the atmosphere. For our purposes, the temperature gradient in the atmosphere could be only slightly subadiabatic and still have the same central temperature. One could have a strictly adiabatic model in which convective heat transport was the predominant means of transporting heat.

DR. POLLACK: I don't quite understand that remark. Are you speaking about what would happen as a result of it being radioactive?

DR. LEWIS: I am saying the temperature gradient may be almost indistinguishable from that which we would find for a Uranus which had a large heat source from its own collapse, as it would be for one which has only a radioactive heat source. That small difference in temperature gradient simply means the difference between being almost exactly adiabatic and being very slightly superadiabatic.

DR. STONE: I didn't understand that. Don't you want a small deviation from isothermal to drive out heat flux?

DR. LEWIS: I am saying that the issue is whether convection takes place. I am convinced there is no way you can make Uranus without having a significant internal heat source from radioactivity. You can't get densities like this unless you have heavy elements present. That heat source from radioactivity will be down by about a factor of 10^4 relative to the observed heat flux coming out of Jupiter, for example. The question is: Is that sufficient to drive convection? I am simply stating that it probably is not. It is touch and go. We do not know enough physics to answer that question. In the case of Jupiter, it is very easy to conclude that the planet is transporting heat by convection. But if Uranus is not convective, then what is the temperature gradient? I submit the temperature gradient is still close to adiabatic because there is an internal heat source. And the time scale for conductive loss of heat from the deep interior here is going to be billions of years away..

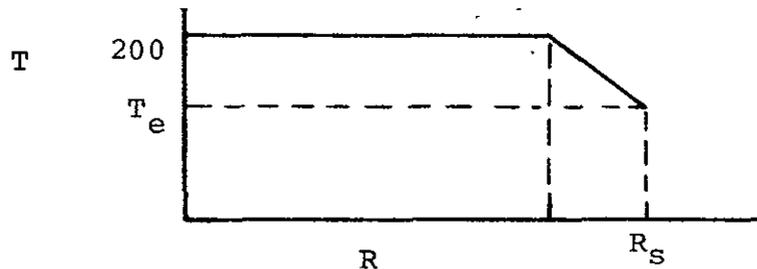
DR. STONE: There could still be a large skin where it was adiabatic?

DR. LEWIS: Oh, yes. By making this general statement, that the atmosphere at some point is not adiabatic, I am not excluding the possibility that there are layers which are convective.

DR. POLLACK: Just to understand your remark a little bit further, suppose I postulate that the atmosphere once you get down to the 200° level just stays flat at 200° all the way down to wherever you join the icy, rocky core. So, it is going to be 200° right at that point.

DR. LEWIS: Let's sketch a temperature profile so we have something behind those figures.

Reproduced blackboard sketch



DR. POLLACK: Yes, that is right. Now, are you telling me that -

DR. LEWIS: I am telling you that this is impossible.

DR. POLLACK: What is the basis for the claim that you are making? Have you actually carried out calculations on it?

DR. LEWIS: Just a rough one. All you have to do is estimate the total amount of heat that is liberated in the production of Uranus and then estimate the time scale for conductive loss of that amount of heat from the interior. The time scale for conductive loss based on either the conductivity of ice or of rock is far longer than the age of the solar system. The collapse energy of the planet must be there. Whether the planet is presently convective or not is irrelevant. I submit the temperature gradient is going to be close to adiabatic one way or the other.

DR. HUNTEN: So this is based on your feeling as to how Uranus was assembled in the first place?

DR. LEWIS: Not as to how it is assembled; just the fact that it was assembled. Even if Uranus accreted very slowly, the compressional heating of the interior would bring it close to the adiabatic gradient.

DR. REYNOLDS: I would say that you are probably right. It is now probably close to adiabatic with the heat sources available in the interior - and most of the materials, particularly in the mantle, are probably going to have low melting temperatures. Certainly any kind of inflow added to whatever you had would probably suffice to melt the thing. And if it is melted it could then convect. Liquid convection is very efficient. And the temperature would then drop from the liquid melting and it presumably couldn't go too much below that because conduction is such a slow process.

DR. LEWIS: The point is that one has an initial thermal spike, one has a more nearly constant production of heat thereafter, but slowly decreasing with time. The planet will cool down and stop convecting at some specific point in time. It can never cool much below the adiabatic temperature gradient, because, as soon as it stops convecting, all heat produced internally by radioactivity is simply stored. Now that the planet is producing heat as a closed system, that heat cannot get out because of the low conductivity.

Therefore the temperature gradient rises up and touches the adiabatic gradient, giving rise perhaps to non-steady convection.

The point is that the amount of heat transported by conduction is negligible. Thus the temperature gradient can not become appreciably sub-adiabatic.

DR. STONE: That is the thing I don't understand because this flux must be proportionate to that gradient.

DR. LEWIS: The crucial question, which cannot be resolved from what we presently know about the planet, is what is the adiabatic gradient in this mixture? Nobody knows. I am saying that the odds are excellent that we are very close to the adiabatic gradient. The reason I just gave is that if one has a planet which is originally convective due to the loss of the collapse energy of the planet, then, as soon as the interior is cooled by convection to a point where it ceases to convect rapidly, each volume element of the interior will thereafter store the heat produced by radioactive decay inside itself. The time for it to get it to raise its temperature enough to start convecting again is very short compared to the age of the solar system.

DR. BELTON: Isn't it possible that the energy could go into some other sink?

DR. LEWIS: We are assuming that the planet collapsed and has been cooling since then, that the major heat source was the collapse of the planet. If it's cooling, then any phase transition would be in the direction of releasing heat.

DR. BELTON: Does it always go that way?

DR. LEWIS: Yes. LeChatelier's Principle.

DR. POLLACK: Suppose you start out with a very cold Uranus, would the radioactivity be enough to raise you up to three thousand degrees?

DR. REYNOLDS: No.

DR. LEWIS: The answer would be in round numbers something like a thousand degrees. I don't know what it would be exactly.

DR. REYNOLDS: It's something like a few thousand degrees per gram for chondrite-type material but that is too high. If you prorate that across the ice and the gas, it cuts the temperature down by a sizeable amount.

DR. LEWIS: The gas is not a large fraction of the planet and has a very low heat capacity. It is the ice that matters. I would change Dr. Reynolds' estimate. He has a ratio of ice to rock of about 4:1 and I would change that to about 2.5:1, based on modern cosmic abundances. And that gives you a little over a thousand degrees?

DR. POLLACK: So the answer to my question is your rockbottom number is a thousand degrees.

DR. LEWIS: Yes, if you put the planet together in a refrigerator at 0° Kelvin and piped compressional heat out of the interior.

Now, what happens when you take this chemical mixture and subject it to temperatures of, let's say, a thousand degrees and a pressure of a megabar? What is the nature of the chemistry?

One very important effect is the influence of pressure on solubility. How does the solubility of a mineral in water depend on pressure and on temperature? Clearly the solubility increases as the temperature goes up. But the thermodynamic activity of the mineral also increases extremely rapidly - in fact, exponentially - as the pressure goes up. And when we get to pressures of about ten thousand to a hundred thousand atmospheres the activities of the solids, instead of being the conventional low-pressure value of one, can be 10^4 or 10^6 , which means that their solubilities are 10^4 or 10^6 times higher than they are at ordinary pressures.

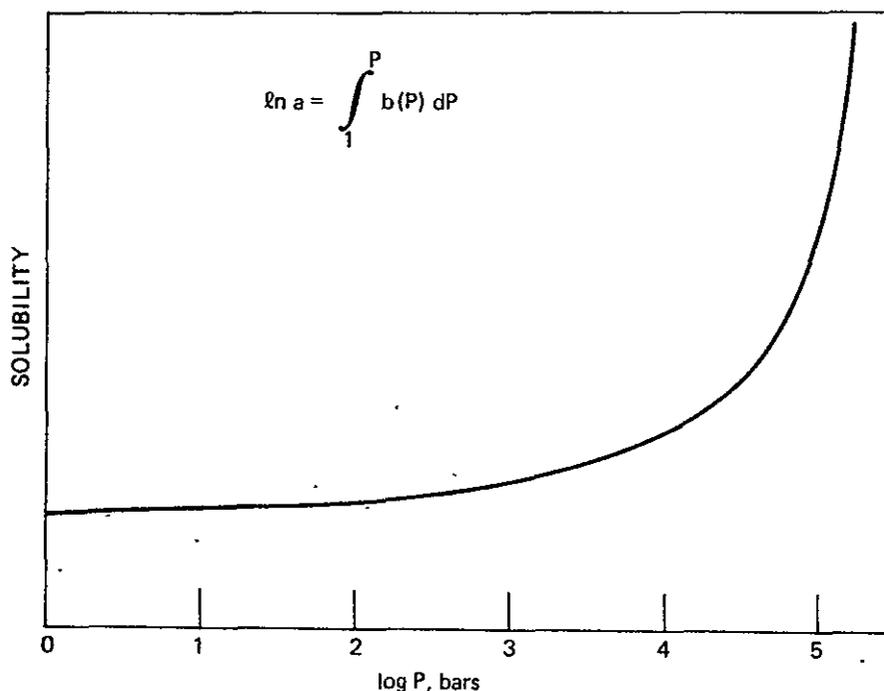


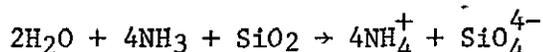
Figure 2-20. Mineral solubility in water as a function of pressure.

The solubility increases very slowly as the pressure approaches a hundred bars, and then begins to turn up and goes exponential. Thus in laboratory experiments with mineral systems at 500° Kelvin one can put a mineral such as enstatite (which is MgSiO₃) in the water at low pressure, then compress it up to a pressure of ten kilobars, and the mineral dissolves! (See Figure 2-20.)

What I suggest is that when one subjects minerals containing magnesium, silicon and iron (which can conventionally be represented in very simple forms in terms of the chemical moieties MgO, FeO and SiO₂) to extremely high pressures and moderate temperatures, they can react with and dissolve in the fluid medium.

The chemical systems involved here are sufficiently complex so that the particular reactions I will write are far from the whole story. I am well aware of that, and I hope you will remember what I say before you quote any of this. For instance, consider water plus dissolved ammonia plus silica: H₂O + NH₃ + SiO₂. We will all recall that silica does dissolve in water to make silicic acid. Therefore, in a basic medium which contains ammonia, it ought to dissolve abnormally well to form a solution of ammonium silicate. This is in fact the case; in the laboratory one can take an aqueous ammonia solution and quartz, dissolve ten weight percent, fifteen weight percent quartz, even at pressures as low as ten kilobars, then decompress it and allow silicates to crystallize out.

Now, this particular reaction above may give rise to ammonium ions plus silicate ions, and we will have an aqueous solution of ammonium silicate. Therefore,



We could write similar reactions for the other chemical species, MgO and FeO. We also should recognize that in reduced systems ferrous iron becomes exceedingly stable in aqueous solutions, and ammonia abets this tendency because of the formation of ammoniates. You are probably familiar with the geological evidence that the oceans of the early earth were formed in the presence of ferrous, but not ferric, iron and contained enormous quantities of dissolved iron at one atmosphere pressure and room temperature. Increase the temperature and increase the pressure enormously and you further stabilize these stable ammoniates solvated iron. At very high pressures, one might find that complexes of the form Fe(NH₃, H₂O)₆⁺⁺, involving octahedral coordination of ferrous iron will become very important. It will be virtually impossible to think of a mechanism for precipitating Fe⁺⁺ out of solution, since it would be an extremely stable solute.

Thus I think that there is good reason to suspect that the rocks will cease to be distinguished from ices at very high pressures. If this is indeed the case, the distinction between the two- and three-layer models should be reconciled in the direction of dissolving the third layer into the second to make something that looks like a two-layer model.

I think perhaps this chemical argument is as strong as any of the structural calculations that have been done on the planets of Uranus and Neptune, in suggesting that a two-layer model is a sensible way to approximate the internal structure of these planets.

DR. HUNTEN: But there could be a big kinetic factor involved here, if the planet were assembled in three layers in the first place.

DR. LEWIS: If it were assembled in three layers in the first place, it would be stably stratified and the only thing that could ever drive mixing would be the fact that all of the radioactive heat source is in the absolute center. In that case, it is very problematical what would happen.

However, the silicate component under these circumstances, would almost certainly contain hydrous silicates. And if that is the case, then the heating of that silicate layer by itself in the interior will already lead to a fairly substantial phase mixing of the two, the rocky and the icy material.

If the planet is put together layer by layer in a strictly inhomogeneous accretion kind of approach, then it is conceivable that one can develop a way in which the planet could even have a metallic iron core. I think this is really a very improbable alternative, but it is one that still has to be kept in mind.

The principal question at this point regarding the deep interior of the planet has become the nature of phase equilibria in the system, water, ammonia, methane, MgO, FeO, SiO₂, and FeS, at pressures which cannot be reached in the laboratory. The complexity of this system plainly exceeds our grasp.

Let us now try to discover rosier hunting grounds: the gas-ice interface. What is happening there, and what observations could we make on the atmosphere which might possibly bear on processes at this interface?

All I can do is suggest that this may be a tractable problem, both theoretically and experimentally, whereas the other one is clearly not.

If we artificially postulate a planet which has a global ocean of silicate soup with an atmosphere of hydrogen, helium, neon and whatever vapors happen to be in equilibrium with the surface of the ocean, then what would be the nature of the interface? We would have to do a model in which real equations of state for gas mixtures were used, because these mixtures are strongly non-ideal and the assumption of additivity of molar volumes is definitely not applicable in this region. If we could do that, then we could attempt to run a pressure-temperature profile down to this region subject to an adiabatic constraint, or subject to an isothermal constraint, either one, and examine whether there was an interface here. If the temperatures here are above the critical surface of the system, then there cannot be a distinct phase transition. Rather the fluid may alter from a sub-critical fluid to a super-critical fluid without any phase boundary being present. That conclusion would have to await the results of quantitative modeling of atmospheric structure, and that is something that for the moment cannot be done.

One can do laboratory studies of the equations of state of these individual materials and the binary system of these materials up to pressures of about 10 kilobars without much trouble. Several of the binary systems have been done adequately, but what concerns us here is, unfortunately, infinitely more complex than that because we have a system in which there are at least seven components of comparable abundance. This means that any simplifications we may like to make by ignoring certain components would totally invalidate the results. We would have to do laboratory simulations on the complete system.

I think interpreting phase equilibrium data for a seven-component system, under these circumstances, is likely to take all petrologists on earth the rest of their lives just to design the experiments. Perhaps I am showing an unaccustomed streak of pessimism here, but it is very hard to imagine ways in which the theoretical and laboratory work that can be done now on earth can reflect usefully upon the internal structure of Uranus and Neptune. About all we can do is ask what spacecraft experiments, what entry probe experiments, could possibly shed light on this internal structure. How deep can we get, and how deep need we get in the atmosphere, in order to learn something useful about the interior.

Clearly, one kind of useful thing that can be done is getting down into the atmosphere toward what is either the bottom or the top of the convective layer, depending upon which view of the planet you take, to a pressure of a few bars, to see what the gas composition is down there.

We would like to look for hydrogen, helium, and neon. A probe probably would not get deep enough to see water vapor, but we would certainly like to look for methane, ammonia and argon, and hydrogen sulfide could be sought. We have already heard from Sam Gulkis' point of view one reason why looking for the abundance of hydrogen sulfide would be a rewarding experiment. Certainly it is easy enough to do if there is a sensitive and general compositional experiment aboard.

I certainly would claim that a mass spectrometer experiment to analyze the atmosphere with the maximum possible flexibility is an essential ingredient of any attempt to understand the deep interior. But even having such data, it might be very difficult to extrapolate downward in the atmosphere much beyond the region measured because of the complexity of the phase equilibria as a whole.

DR. POLLACK: One question that I think is particularly interesting in terms of that gas rock-plus-ice interface is the question of whether any of the more volatile materials such as methane might preferentially get into the gas stage?

DR. LEWIS: Right. Whatever the nature of the interface, there has to be partitioning of each component between the two phases. I think the partitioning of methane is one case in which we are likely to get a fairly efficient enrichment of methane in the outer layer relative to the inner. The solubility of all this stuff in the liquid depends upon there being ionic species and

there being molecules which have large dipole moments. Methane does not fit either recipe. It has what we could call hydrophobia, like gasoline or asphalt. Such materials would be excluded to some degree from the interior.

Of course, at these high pressures there is some solubility of methane, so all we can say is that the methane-to-water ratio in the interior will be less than the methane-to-water ratio in the atmosphere.

DR. JOHNSON: John, in terms of spacecraft experiments, you should not neglect getting better values for J. That value is not at all universally agreed upon.

DR. LEWIS: In terms of the deep interior, getting a better value for J is perhaps more important than anything you can confidently plan on getting from an entry probe.

DR. BELTON: Is it possible under any circumstances to have a body made primarily of ices with a very small atmosphere on Uranus and still have the mean low density?

DR. LEWIS: No rocks and no gases?

DR. BELTON: Yes.

DR. LEWIS: That depends on whether you believe the value for J or not.

DR. BELTON: Forget about J. Let's say it is not rotating.

DR. LEWIS: You would have to go to high temperature, or you would have to put in light gases homogeneously mixed.

DR. REYNOLDS: Like a hot methane planet.

DR. LEWIS: The mean atomic weight for methane is substantially less than water but more than you would need to get the density right.

DR. REYNOLDS: I think it would be very difficult to do. You see, if you just take ices and put it there, it is going to compress to more than 1.2 mean density. You have to have hydrogen and helium in there to make the mean density come out right and then you have to have a thick atmosphere. So it would depend on equations of state. I haven't done that calculation recently. I would think it would be very difficult to do.

DR. POLLACK: So what you say is that definitely we need the gases, and I guess the distinction you probably can't make on density grounds is the question as to whether you need rocks as well as ices.

DR. REYNOLDS: If you need rocks as well, then it is going to be much more difficult. We are just taking the case of could you do it with ices alone, and even that, I think is very problematical.

DR. POLLACK: I was asking the other question as to whether you could establish that there is in fact a rocky component to it just on the density ground?

DR. REYNOLDS: You can make any final density with hydrogen and platinum if you mix them in the right proportions to get the right mean density.

DR. BELTON: What would be so different from the Uranus model, 50,000 kilometers across, and the Ganymede type models?

DR. LEWIS: The difference is that the entire range of internal pressures is completely different and the chemistry depends sensitively on what the total pressure is.

DR. BELTON: But the arguments would still apply to the outside.

DR. LEWIS: I am not exactly sure what you have in mind when you say that because, remember, putting an atmosphere on top of such an object gives you a new boundary condition for the surface temperature. And if that atmosphere is warm enough to melt all of the ices and the surface pressure is high enough so that the density of the gas approaches the density of the liquid, that is an appreciably different structure.

DR. BELTON: When I look in the literature I find that maybe there will be a surface, maybe there won't, depending on the kinds of things that you have outlined. Maybe in megabars, something like that. I don't know anything about uniqueness in those kinds of situations. It is possible it is not a megabar, but a hundred bars? Ten?

DR. LEWIS: I guess the central point I was trying to make in talking about the chemistry is that because we expect a large number of components with comparable abundances, the chances of our coming up with a single model to describe all of the observations is extremely remote. The complexity of the system simply defies our understanding.

DR. GULKIS: How does Uranus fit into the nebula formation hypothesis? Is it an anomaly?

DR. LEWIS: No, it is not an anomaly because, as I say, you can do anything by constructing sufficiently arbitrary accretion scenarios.

I would say that the philosophy behind the model that Ray Reynolds has presented here is close to being mine in that he has preserved the ratio of the icy-to-rocky components as being solar. Any way that I can see to make Uranus and Neptune involves forming a lot of solid material which has a rather low formation temperature, and it sticks together helter-skelter until it finally builds up a planetary core big enough to cause gravitational gas capture from the nebula. Then it draws in a certain amount of solar composition gas and captures it to form an atmosphere.

In effect, this approach is appealing to me because that is the mechanism that I think of as contributing to the formation of Uranus and Neptune. There may be very different ways.

For example, one may impose an operator which extracts silicates from the outer part of the solar nebula quantitatively and leaves nothing but ices. Then these ices may accrete to form a planetary core and capture gas as before. In that case, rocks are wholly absent.

DR. JOHNSON: You are presenting sort of a forward calculation as one of the types of things that Sam might be interested in and the inverse question is: is there any plausible reason why Uranus and Neptune didn't turn out like Jupiter and Saturn?

DR. LEWIS: All I can say is that I have read the Perri and Cameron paper and I have done similar calculations myself on gravitational capture from the nebula and as far as I am concerned they seem to be on the right track. If they change their temperature-pressure profile for the nebula a bit, and cool off the nebula, then planetary cores with respectable dimensions, namely a few earth masses, will then become capable of capturing gas. Then the scenario becomes much more sensibly related to all the other planets.

DR. POLLACK: How do you get just a few earth masses and they get many earth masses; what is the difference?

DR. LEWIS: Because they assume this is about the hottest possible nebula. That means that the ratio of the thermal energy to the gravitational potential energy is large and they need an extremely massive planet to capture gases.

DR. POLLACK: Oh, I see.

DR. BELTON: I tried to understand the literature on this business of formation, in the Cameron paper you just mentioned and the one by you, and it seemed to me that you, as a group, are of two minds about methane and argon as far as Uranus and Neptune are concerned -- whether they should really be sort of called one of the permanent gases or whether they should fall in the ice forming group.

DR. LEWIS: I think it is important for me to emphasize that when I talked about compositional models for Uranus, I gave a list of four or five which I think are all at least plausible. They just consist of going down the condensation sequence to different points.

DR. BELTON: But it is possible, for example, that it was sufficiently warm during the condensation that the methane that we see in the atmosphere may be in solar proportions.

DR. LEWIS: This is important, Mike: If the temperature in the nebula were high enough so that no ices were condensed at Uranus' heliocentric distance, then you would produce a planetary core of rock alone. But because of

the high nebular temperature, the mass of rock which one would have to accrete in order to capture some gas onto it then becomes prohibitively large. If, on the other hand, the nebular temperatures were low so that ices were condensed as well as rocks, then the mass of condensed material is larger and the thermal energy of the gas is smaller compared to the gravitational potential energy, and capture becomes much easier. That makes it much easier to make a planet.

DR. HUNTEN: Besides which there are satellites.

DR. LEWIS: Besides which there are satellites; but, unfortunately, in Uranus' case we don't know their densities.

DR. POLLACK: Just to follow up on Mike's remark, to my own mind, Uranus is particularly an exciting planet to look at its atmospheric composition just because it is on a very interesting transition point between an ordinary terrestrial planet and a full-blown Jupiter where you may have some very interesting compositional anomalies show up in the atmosphere that will give you some very important clues on the formation of it.

DR. LEWIS: That is right. You cannot anticipate what the results will be. But getting down there in that atmosphere and analyzing it is potentially the source of a wealth of information.

DR. HUNTEN: You told us what a probe might do for Uranus' interior. Could you tell us what the interior might do for the atmosphere, so to speak? What I have in mind is: what kind of differences and in which direction could you expect relative to solar composition in the atmosphere. Could methane be either enhanced or depleted?

DR. LEWIS: I see no mechanism for depleting methane relative to solar abundances. There are mechanisms, certainly, by which the methane-to-hydrogen ratio could be strictly solar. In other words, one might condense a planet core which contained ices devoid of methane and then captured onto this an atmosphere of solar composition gas. Or one could add condensed methane to the solids to raise the methane-hydrogen ratio in the atmosphere.

Do I have any strong feelings as to whether the methane-hydrogen ratio is solar or greater than solar? I am entirely prepared to believe it is greater than solar, but the data so far do not force me to believe that. In fact, there is nothing about any particular theory that is sufficiently compelling to force me to believe it to the exclusion of others.

DR. HUNTEN: One other question keeps popping into my mind when I read and hear these descriptions of accreting solids out of the nebula and then accreting gas of solar composition is that there obviously is an incompatibility. Is the mass of the nebula supposed to be so great that you don't affect its composition at all by accreting these cores? That could be a way of depleting the atmosphere of solid materials.

DR. LEWIS: That mechanism will only work if there is no substantial radial mixing in the nebula such that one could transport materials from one portion -- take solid methane out here but condense it in another place. Well, that is an unresolved issue.

DR. POLLACK: I think that remark by Don is one of the questions that I have on the Cameron model and that is its infinite reservoir.

DR. LEWIS: In the case of Jupiter, it may well be that a massive planetary core formed and left the nebula in that vicinity totally depleted of solids. It then captured a "solar composition gas", which in point of fact, was not solar composition but was the complement of the material which was condensed to form the planet, so that the total ended up solar composition; but we know for sure that the reservoir of hydrogen and helium around Uranus and Neptune must have originally been larger because the planets did not capture all that hydrogen and helium.

DR. POLLACK: How do you know how much was around Uranus and Neptune?

DR. REYNOLDS: If the nebula was originally homogeneous then you know that Uranus and Neptune are not solar composition.

DR. POLLACK: I thought John's remark had to do with the other gases besides hydrogen and helium.

DR. LEWIS: Are you talking about methane, for example?

DR. POLLACK: Yes. I was following Don's question about whether methane could be depleted in Uranus's atmosphere. It seems to me possible that all of the methane could be in that core for example.

DR. LEWIS: I am saying that it does not seem possible to keep the methane in the core.

DR. POLLACK: But how about, on the other hand, ammonia?

DR. LEWIS: Ammonia you can bury, yes.

Sam (Gulkis), I can't give you the dependence of the ammonia abundance on altitude but I can surely enough describe a mechanism that will hide lots of ammonia in the deep interior because of its high solubility.

The question of explaining the change in abundance of ammonia with altitude, then, remains just as difficult as before.

But this partitioning of ammonia between the gas-like and the liquid-like regions would almost certainly put the ammonia mostly in the liquid.

DR. REYNOLDS: What you are saying is that it is hard to predict much from the interior; but, after the fact, you can think up a lot of explanations.

DR. LEWIS: It depends on what you know about the deep interior aside from just the radial mass distribution. You certainly are not going to get a compositional analysis versus depth. All you get is bulk density, and how do you interpret that? It has been shown in the case of the earth that, even having detailed seismic data on the structure of the deep interior, it takes decades for the arguments on the composition to quiet down. There are some crucial issues, such as the difference between the upper and lower mantle of the earth, which are still not fully resolved.

DR. REYNOLDS: And the composition of the outer core, as well.

DR. LEWIS: I consider that resolved to my satisfaction. But it is worthwhile to keep in mind what that situation would be with only the radial mass distribution. That, in itself, is woefully inadequate.

DR. OWEN: I'd like to emphasize one of the things you said earlier: that we surely would want to investigate the satellites of Uranus, Saturn and Neptune because they give another chance to see what the small accretions were like.

DR. LEWIS: If we just knew what the densities of the satellites of Uranus were, we might be able to say, "Well, good grief, the densities are all the same as that of rock so the characteristic temperature around Uranus at the time of formation was very high," and this would strongly condition our ideas about the planet. Or we might look at them and find that, "Good grief, the densities are that of pure ice with no rock present;" how do we account for that?" If we just had that information, we would be in far better shape than we are now.

DR. BELTON: Can't you say something like that now on the basis of observations just from the spectral albedo?

DR. LEWIS: If you asked me, "What is the most volatile rich kind of satellite you can make," I would say that it is a mixture of rocks plus ice with full retention of methane. And what would the surface of that look like? It might well be black, like asphalt.

DR. WALLACE: How do you look for invisible satellites?

DR. JOHNSON: If you had either albedo or mass numbers, you could put limits -- and we have gone through this same thing at MJU. Right at the moment, all we have are the original magnitudes.

DR. OWEN: And some spectral information.

DR. BELTON: What strikes me is that when you assume that the albedo is high, you get the smallest radius and you still get a low density.

DR. JOHNSON: That assumes also that the masses are known. And the mass, even for Titania, which is the only one that has actually been measured, is terrible.

DR. BELTON: He is being polite, really.

DR. OWEN: Think of the density change that Titan just went through.

DR. BELTON: What is the latest on that?

DR. LEWIS: The density of Titan? The uncompressed density of Titan is probably down to one gram per. cubic centimeter.

DR. OWEN: The observed density just jumped below two last summer.

DR. LEWIS: It is in the process of exploding.

DR. OWEN: It is a lot easier to observe than the satellites of Uranus.

DR. TRAFTON: The low density may just mean a thicker methane atmosphere.

DR. LEWIS: The old business of taking superficial measures such as the albedo as determinants of composition is dying by the wayside in the studies of the asteroid belt because the dark objects in the belt don't seem to be dark because they are rocky: they seem to be dark because they are like carbonaceous chondrites. They have soot in them. This is not a certainty, but all the evidence that is available does point in that direction right now.

DR. JOHNSON: The lowest-density satellite of Jupiter, Callisto, has the darkest surface of any of the satellites.

DR. POLLACK: You can also make an inverse statement and that is that the more dense satellite, which is probably made up mostly of rock Io, has the highest albedo.

2.5 SPECTROSCOPIC DATA

Laurence M. Trafton

I will summarize the results of spectroscopic measurements of Uranus. Jim Pollack has already made my first point; namely, that the period of rotation of Uranus is uncertain. The photometric data give contradictory results: see Moore and Menzel and the discussion by Alexander (1965). The best determination of the rotational period is the spectroscopic one by Moore and Menzel (1930). They find a value of 10.8 hours, which is in pretty good agreement with the spectroscopic measurements by early observers. But they point out that the error in this value could easily be as much as plus or minus a half hour. Consequently, we must consider the rotational period to be rather uncertain.

DR. WALLACE: I thought people had in recent times attempted to re-measure it.

DR. TRAFTON: I am not aware of that.

DR. BELTON: Guido Munch once told me that he tried it and that it was impossible.

DR. POLLACK: It makes you a little pessimistic about the early observations, doesn't it?

DR. TRAFTON: We are now attempting to measure it at McDonald Observatory, using a new technique (Deeming and Trafton 1971).

I. COMPOSITION

I now turn to the question of Uranus' composition. It has long been known that hydrogen and methane are important constituents of the Uranian atmosphere. The green color of Uranus is largely due to the absorption by the methane bands. Herzberg (1952) discovered the presence of hydrogen from the pressure-induced feature at 8270Å. At the time, he thought he also had discovered the presence of helium, from the apparent weakness of the double transition relative to the single transition in hydrogen. But it now appears that this ratio is simply the result of a methane blend. Also, Welsh (1969) has since pointed out that the overtone pressure-induced transitions depend largely on quadrupolar induction. Helium not having a permanent quadrupole moment, does not encourage the overtone transitions in hydrogen. There is no direct evidence now that helium exists in the Uranian atmosphere.

Recently, Encrenaz, Hardorp, Owen, and Woodman (1973) set an upper limit on ammonia in the Uranian atmosphere, finding it to be about 20% of the ammonia visible in Jovian atmosphere, 2-1/2 meter magats. They find no compelling evidence for ethane or ethylene; HD has not yet been detected, although there is work yet to be done at high resolution.

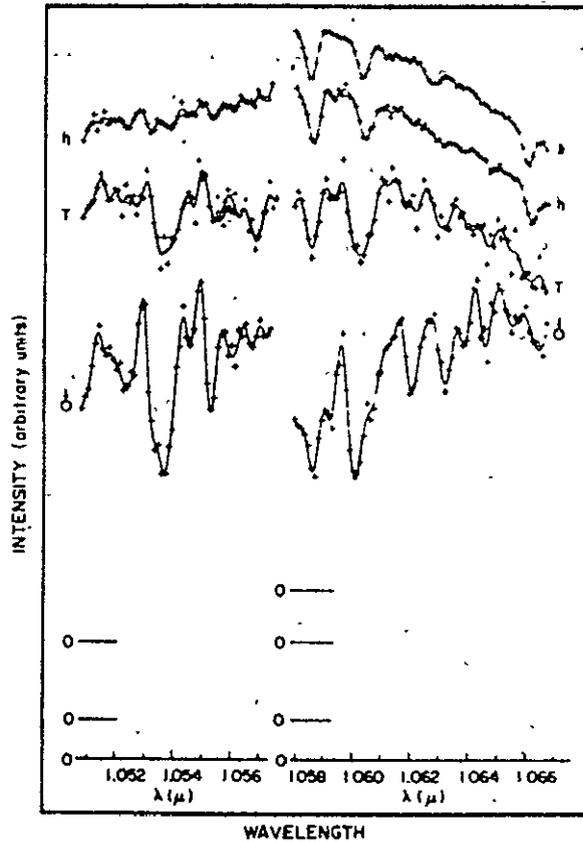


Figure 2-21. Unidentified features near 1.06μ in the spectra of Uranus (U) and Titan (T). The spectra of Saturn (h) and the moon (D) are shown for comparison.

There is, however, some evidence for another gas from the unidentified absorption features at 1.06 microns. These also exist in Titan's spectrum and are exhibited in Figure 2-21.

At a resolution element of 4\AA , this figure compares the spectra of Uranus, Titan, Saturn, and the Moon. In spite of the greater strength of these features in Uranus' spectra, the abundance of the gas giving rise to these features is probably considerably less than in Titan's atmosphere. This implies that this gas is unlikely to be methane.

The chain of reasoning follows: in the Uranian atmosphere, because of the large hydrogen abundance and the freezing out of methane at altitude, the effective pressure for methane is probably on the order of one atmosphere. In Titan's atmosphere, the effective pressure for methane is only 10 mb, so that

the 1.06 micron features are almost certainly off the linear part of the curve of growth and pressure strongly influences the absorption. If we could decrease the effective pressure of these features in Uranus' spectrum, we would find no change in the spectrum until they move off the linear part of the curve of growth. Their strength implies that their lines are already close to saturation, if not already saturated. Further reduction in pressure will weaken these features until they resemble those in Titan's spectrum. Any further reduction in pressure must then be accompanied by an increase in column abundance of the gas to keep the features from becoming weaker. Considering that the pressure is two orders of magnitude less in Titan's atmosphere, we are probably safe in concluding that the column abundance of the gas responsible for the 1.06 micron absorption feature is less in Uranus' atmosphere than in Titan's.

Now, in the case of methane, Figures 2-22 and 2-23 show considerably more absorption in the 0.9 micron band complex of methane and in the $3\nu_3$ methane band at 1.1 microns, respectively, for Uranus than for Titan or Saturn. As I mention below in the discussion of the $3\nu_3$ bands, the abundance of methane visible in Uranus' spectrum may be about 30 times that for Saturn, or roughly comparable to that visible in Titan's spectrum. It is unlikely to be less than that for Titan so the 1.06 micron features are unlikely to arise from methane. This conclusion is reinforced if the large methane abundance deduced from the Kuiper bands at 7500Å (Owen, 1967) of 3.5 ± 1 kilometer amagats or from the blue bands (Encrenaz et al. 1973) is valid.

DR. OWEN: Let me just mention for what it is worth, and it probably isn't very much, that methane in the laboratory has a strong feature about this wavelength.

DR. TRAFTON: Jerry Woodman has obtained spectra from methane at path lengths up to 300 meter amagats at JPL. There are some coincidences between these features and those of Uranus' spectrum; there are also prominent anti-coincidences. Therefore, the centers of the 1.06 micron features do not correlate well in wavelength with those of a laboratory spectrum of 300 meter amagats of methane. The same is true for ammonia, ethane, ethylene and hydrogen sulfide. We cannot definitively exclude methane, however, because the laboratory sample contained a significantly less quantity of methane than is spectroscopically visible on Uranus. Furthermore, the spectral resolution was not comparable, being about one angstrom.

Concerning other unidentified gaseous constituents, notice that the ratio of the spectra of Uranus to Neptune in the strong bands is opposite to the ratio near the wings of those same strong bands in the following sense: The strength of the methane absorption at the center of these bands is greater for Uranus than it is for Neptune and it is the other way around near the wings of these bands. Since the curves of growth don't cross, Doppler broadening being negligible in the Uranus atmosphere, Wamsteker (1973) argues that this is evidence for another gas component besides methane, noting that it may well be the far wing of the pressure-induced dipole absorption of hydrogen.

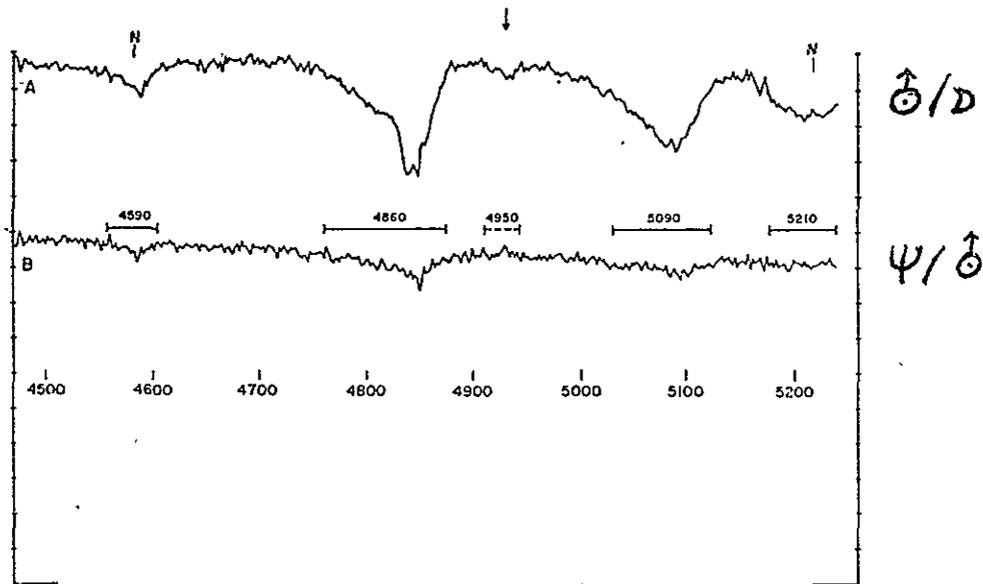


Figure 2-22. Ratio spectra Uranus/Moon and Neptune/Uranus for $\lambda 4500$ to $\lambda 5210$ showing stronger methane absorption in Neptune's atmosphere (from Encrenaz et al. 1973).

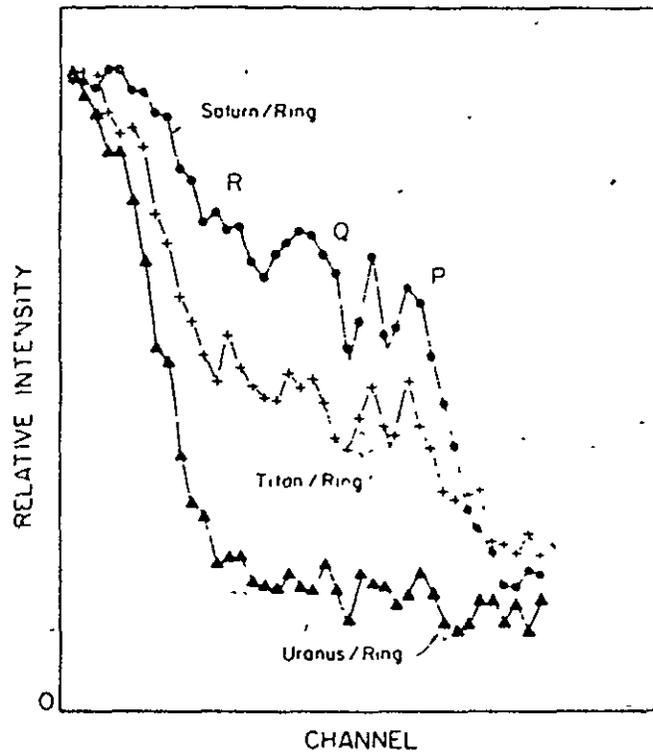


Figure 2-23. Methane absorption in the $3\mu_3$ band region of the spectrum for Uranus, Titan and Saturn. The spectra are ratioed to Saturn's ring spectrum to remove instrumental response and telluric water absorption. Note similarity in structure.

DR. WALLACE: I never did understand that; do you? I mean, this argument for another constituent?

DR. TRAFTON: The curve of growth for another constituent could be completely different. It could lie, for example, on the linear part of the curve of growth at the same time as the first constituent lies in the square root part of the curve of growth. In the first case it is independent of the pressure and in the second case it is dependent on the pressure.

One can go one step further and look at the methane bands in the blue. Those weak bands are observed to be stronger for Neptune than for Uranus (Encrenaz et al. 1973). This is shown in Figure 2-22 which illustrates the ratio spectrum for Neptune divided by Uranus. On the other hand, the strong methane bands in the red are stronger on Uranus. This indicates something is there besides hydrogen and methane, even if it is only particulate scattering. Because of the large wavelength difference between the red and the blue, you could invoke an aerosol to explain this difference. For example, if there is more aerosol scattering and methane visible in Neptune's atmosphere, the strong methane bands would appear to be diluted while the weak ones would still appear enhanced. This might also explain a similar behavior in the strongly and weakly absorbing parts of the strong bands.

The vapor pressure of methane decreases rapidly at temperatures below 90° Kelvin, indicating that methane largely freezes out of the layers of the Uranus atmosphere colder than about 90°. Frozen methane may provide the aerosol.

II. METHANE

The large abundance of methane determined from the blue bands results from laboratory spectra obtained at very large paths. Encrenaz et al. (1973) have suggested abundances as high as 50 to 100 kilometer amagats of methane, which is considerably higher than the abundances derived from the Kuiper bands. But it is probably true that the very large abundances derived in the blue end of the spectrum are too large because they were not interpreted in terms of a scattering atmosphere. Recently, Belton and Hayes (1974) have pointed out that multiple scattering in the blue combined with a continuum opacity increasing to the red can explain the strength of the blue bands without invoking such large methane abundances.

Figure 2-24 below illustrates the absorption in the 0.9-micron methane band complex for Uranus, Titan and Saturn.

The zero for Uranus is at the bottom, the solid line is the zero for Titan and Saturn, and the dotted line on the right is the zero for the comparison of a ring spectrum. The three bands shown are the $\lambda\lambda$ 8400, 8600 and 8900 bands, respectively. Note the strength of the methane absorption for Uranus.

The only methane band whose quantum states are positively identified is the $3\nu_3$ band, shown in Figure 2-23. The Q branch is shown and you can see segments of the R branch, and the P branch, which is superimposed on something else. I don't know what the strong feature immediately to the right of the R branch is. I wonder whether it is the Q branch of methyl deuteride. Laboratory spectra of this isotopic molecule obtained at the same resolution are desirable for settling this point.

If we make simplifying assumptions, like neglecting scattering entirely at this wavelength and neglecting superimposed absorptions, we can roughly estimate the ratio of pressure abundance products between the Uranus atmosphere and the Saturn one, which has already been analyzed. This leads to a value of very roughly thirty times larger for Uranus. If the effective pressures are similar, or if the absorption is only weakly dependent on the pressure, then the amount of methane visible would be 30 times larger than for Saturn or 1.6 km - amagat. I emphasize that is only a back-of-an-envelope calculation.

Figure 2-25 presents a spectrum of Uranus, $5\nu_3$ methane band (Encrenaz et al 1974). Toby Owen (1966) originally suggested this identification for the band. He, and later Teifel and Kharitonova (1970), performed a crude analysis of the band, deriving a rotational temperature in the vicinity of 60° Kelvin. This value is discrepantly low when compared with the rotational temperature of the hydrogen quadrupole lines.

DR. POLLACK: Also, don't you have a problem with 60° from the vapor pressure effect that you mentioned before?

DR. TRAFTON: Yes. Methane should be frozen out at 60° , so there would be a problem with this rotational temperature for methane.

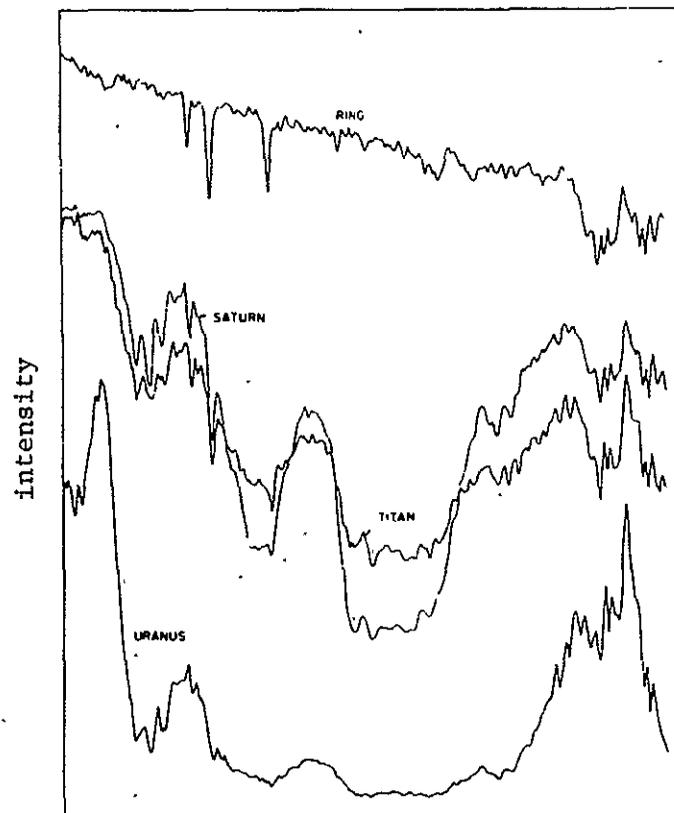


Figure 2-24. Methane absorption in the $\lambda 8400$, $\lambda 8600$, and $\lambda 8900$ bands for Uranus compared with those for Titan and Saturn. The $\lambda 8900$ band is the strongest of the three. In the absence of scattering, this band would be black in Uranus' spectrum.

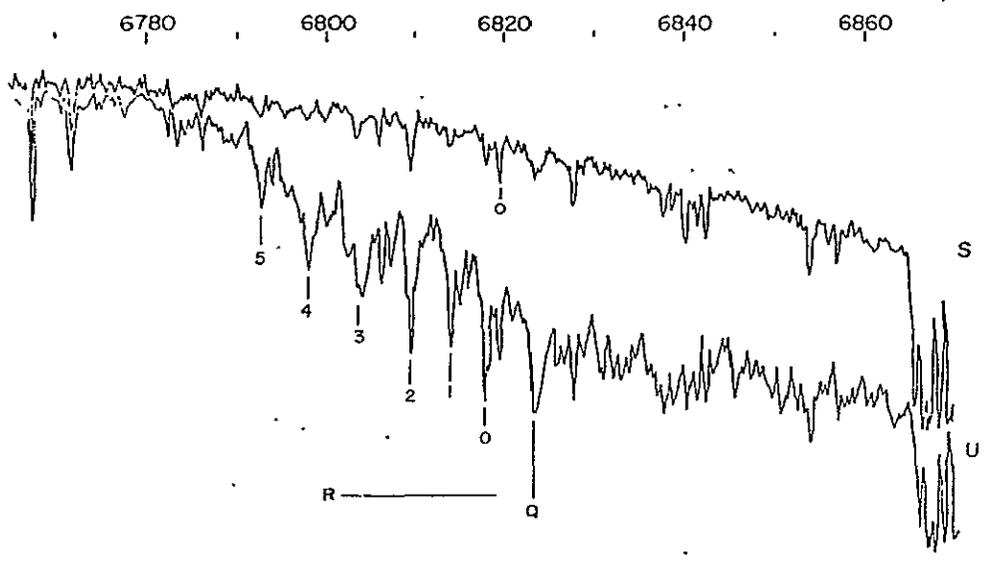


Figure 2-25. The probable $5\nu_3$ methane band in Uranus' spectrum (below) compared with Saturn's spectrum (above). (From Encrenaz *et al.* 1973).

DR. BELTON: What is the resolution on the $5\nu_3$ spectrum?

DR. OWEN: I don't know exactly. It was a photographic spectrum taken at Mount Palomar.

DR. POLLACK: I think it is very interesting that you see so much methane in these absorption bands because, presumably, we have got to look deeper than 90° Kelvin level in order to start really seeing methane, which means that you really are looking deep into the troposphere.

DR. HUNTEN: I keep bringing up this point about possible supersaturation. Let me amplify it a little.

When you are talking about very, very small partial pressures of something at an extremely low temperature, the probability of having a particle grow at these pressures is not like that for water in the Earth's atmosphere. Here you have far smaller partial pressures at a much lower temperature. It really is a different ball game altogether in terms of both the nucleation of particles and the rate at which they can grow.

One shouldn't just blithely make the assumption, as we tend to do, that supersaturation is impossible.

DR. POLLACK: The thing on supersaturation is that you have to speak about rather large factors because the vapor pressure curve is going down exponentially. At a 60° level, it would probably be orders of magnitude down. In the case of Earth, if you had pure water around you and you didn't have any nuclei on which they could start to form, then you can get supersaturation up to about 300%, but-

DR. WALLACE: You would need about a factor of 100 on those models that I was showing this morning.

(See Chapter 1, Editor.)

DR. TRAFTON: Just to add to your point, the further complexity in this case is that it is mixed with a lot of hydrogen, and perhaps other gases too, and that may affect the nucleation properties and supersaturation at very low temperatures.

More recently, Belton and Hayes looked at a high resolution spectra of this same band, doing a quick analysis of it, and they obtained a higher rotational temperature, around 100° . They attribute this result to improved resolution of some blends in the lower J manifolds of this band. Assuming, in essence, an effective pressure of around two atmospheres, they derive a methane-to-hydrogen ratio roughly from solar to about three times the solar abundance.

DR. BELTON: We didn't assume a pressure: the lines are just on the bend of the curve of growth, like the $1.05 \mu\text{m}$ band on Mars.

DR. TRAFTON: I think there is a lot of subjective interpretation there.

DR. OWEN: Since I am responsible for this line identification, I would also like to say that I am very uneasy about it. This splitting doesn't look at all like a $3\nu_3$ splitting to me, now that I have seen it at higher resolutions and I am really nervous about that P branch. It looks very narrow and doesn't seem to have nearly enough absorption.

DR. TRAFTON: There is a possibility that different overtones look different.

DR. OWEN: Also, where is R(6)? It seems strange that it is completely absent and I am not very happy about it.

DR. BELTON: Since we are being subjective about this, once you look at the stronger lines, my conclusion was that it is very similar to the $3\nu_3$ splitting. I agree it is very subjective. Perhaps there are other components; that would be my thought. And I don't think that R(6) is any problem.

DR. TRAFTON: I think the mere fact that this band has survived another analysis tends to add additional weight to Owen's original identification for this feature, i.e., that it is the $5\nu_3$ band.

DR. BELTON: I think what is required is not so much another observation of the planet right now, what is required is an observation in the laboratory.

DR. OWEN: Exactly. It has been required for about eight years.

DR. TRAFTON: Good point.

DR. POLLACK: I might mention on that that there is some very important work being done by Boese at Ames where he is trying to go through quite a number of the spectral regions, including this one, I believe, and taking a look at the laboratory spectra for a range of pressures and even temperatures, so I think it may provide something.

DR. OWEN: Excellent. I think we should endorse that effort as well.

DR. TRAFTON: Clearly this is a more accessible band than the one at 1.1 microns. We can get to it with photo-tubes of much higher quantum efficiency. So if we do have positive laboratory studies and quantum identifications of these bands, this would be the preferable one to study.

DR. BELTON: I would like to see if they can do it by taking a simplified approach. The treatment of the band in that context gives you a very nice picture as far as excitation is concerned, the rotational temperatures and distributions of strengths, and it kind of comes out well. If you take the ball park number that is available from the laboratory spectra right now, you get roughly, with the reflecting layer type approach, the same amount as you get from the low resolution spectra in this spectral region. So I think the whole thing hangs together rather well.

DR. TRAFTON: Clearly, a refined analysis is needed. If for no other reason, this being an overtone is going to create differences in the stretching forces and that is going to perturb the shape of the band, make it look different from the one at 1.1 micron. Also, the "junk" methane spectrum that is supposed to form the background for these manifolds may be more of a problem for the $5\nu_3$ band.

III. HYDROGEN

Turning now to hydrogen, we have three categories of spectographic observations, the observations of the quadrupole lines, pressure-induced features, and the Raman spectrum.

A. Quadrupole Spectrum

Figure 2-26 presents typical observations of the quadrupole lines which I obtained for Uranus. Shown are the $S_4(0)$ and $S_4(1)$ lines. I would like to direct your attention to the weak features in the continuum on either side of the $S_4(0)$ line. They show up in each observation. If we assume that those features are weak methane lines, i.e., that they have a half width similar to that of the lines in the ν_3 and $2\nu_3$ methane bands, they imply an upper limit on the effective pressure of 1.9 atmospheres. If this is the case, and we take into account the amount of hydrogen which the equivalent widths of these lines imply, we can set an upper limit to the non-hydrogen component of the Uranian atmosphere. The result is that hydrogen is probably more abundant than the sum of the other gases in this atmosphere, depending on refinements in the computation of the hydrogen curve of growth to include the effect of pressure shifts. These I will get to in a minute.

I should mention that the equivalent widths of the 4-0 quadrupole lines in the literature range from values of around 30 milli-angstroms to 60 milli-angstroms with most observers getting values near 30. Only Price (1973) has gotten values as high as 60 milli-angstroms; his data are shown in Figure 2-27.

They seem noisy compared to other observations, probably because the observation was photographic. The $S(1)$ feature is not symmetrical, and the width of the $S(0)$ feature is significantly greater than the instrumental width. So I would tend to give low weight to these observations. I am inclined to accept a value of the equivalent width of around 28 to 35 milli-angstroms based on the results of my monitoring of these lines since the apparition of 1970.

Figure 2-28 shows an observation of the $S(0)$ line of the 3-0 overtone obtained during the 1973 apparition. The right hand figure is the ratio spectrum of Uranus to the moon; it lacks the telluric features. For comparison, Figure 2-29 shows the corresponding $S(1)$ line also bracketed by telluric absorptions. The ratio spectrum appears to reveal a Uranian line at $\lambda 8149.8$ blended with a solar line which has not been cancelled, owing to the doppler shift. This is probably methane since the wing of the $\lambda 7900$ band is strong at this wavelength.

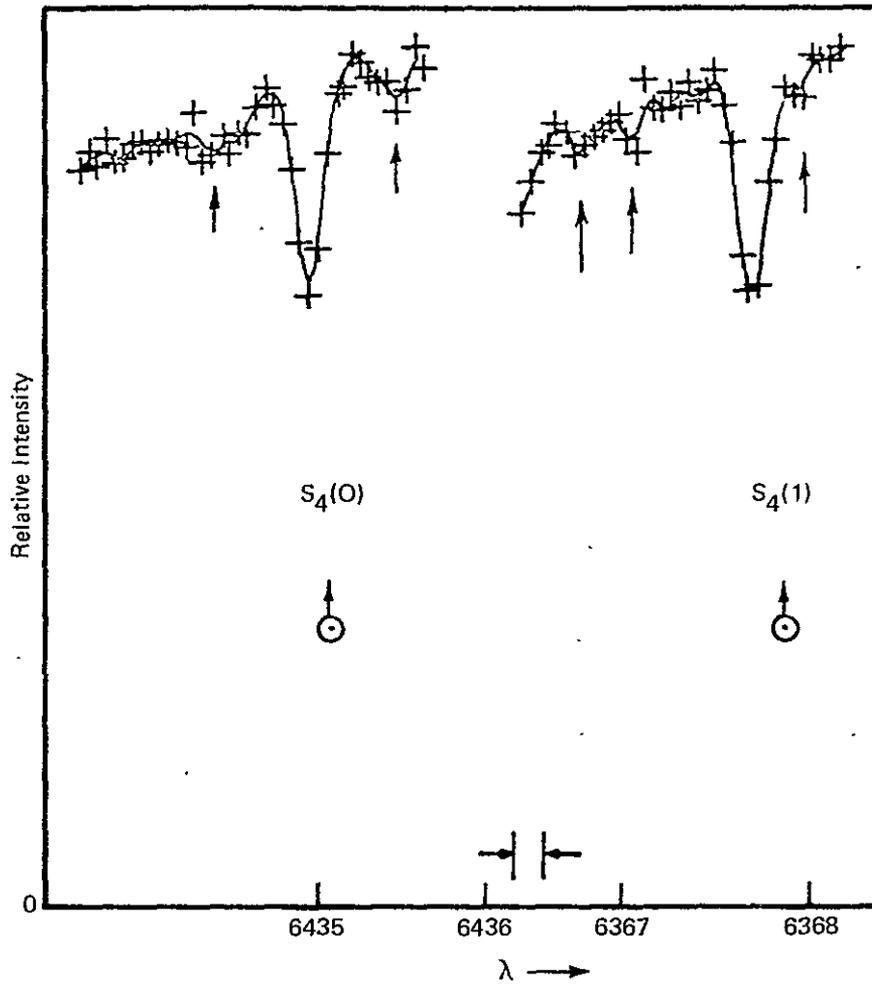


Figure 2-26. Spectra of the (4-0) H_2 quadrupole lines for Uranus. Other weak features are also apparent in these spectra.

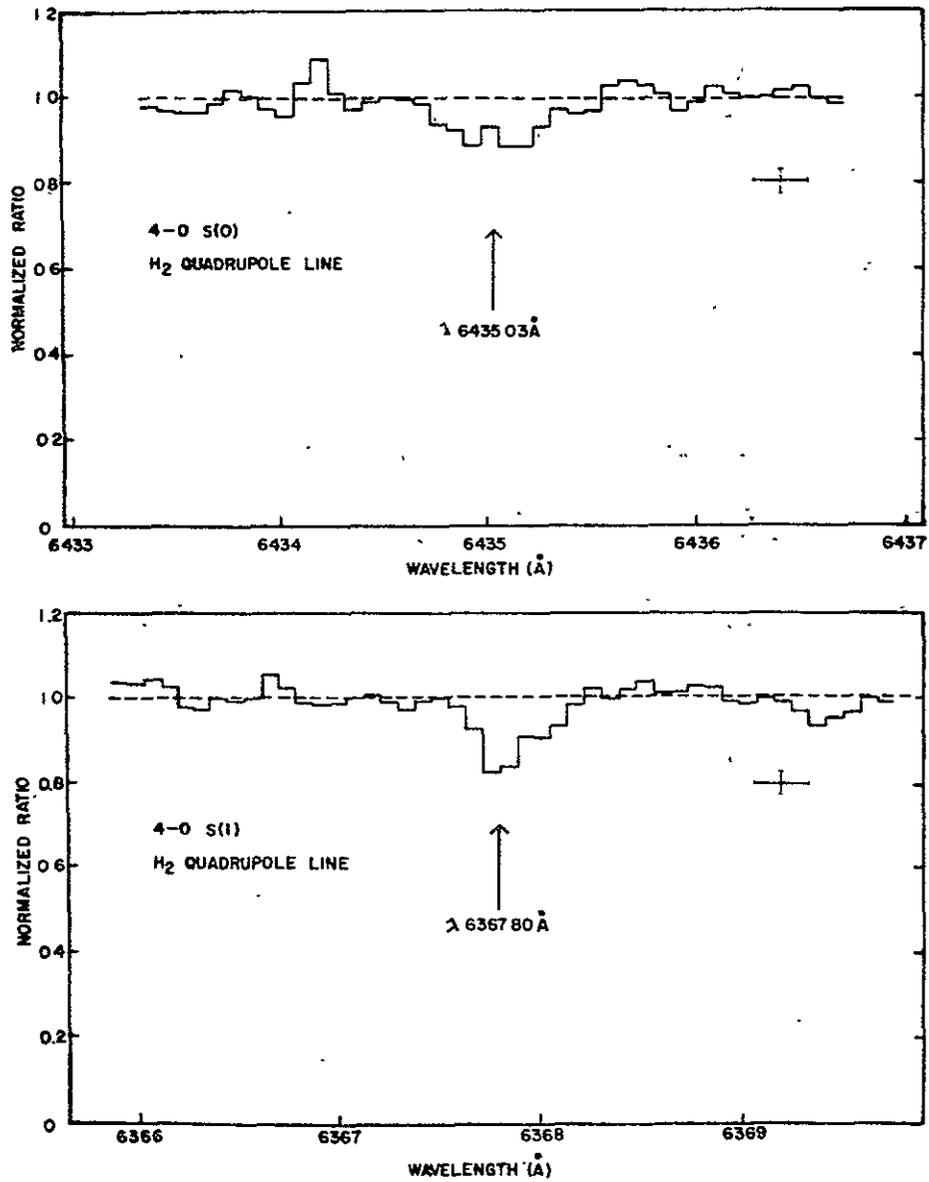


Figure 2-27. Normalized Uranus/Moon ratio spectra for the (4-0) H₂ quadrupole lines from Price (1973). Horizontal error bar indicates spectral resolution; vertical bar indicates rms error of an individual continuum data point.

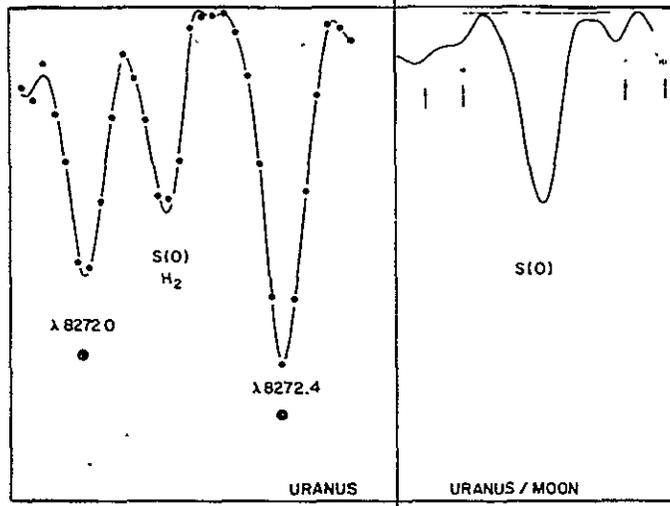


Figure 2-28. Scanner spectra of Uranus' $S_3(0)$ H_2 quadrupole line (left) and the ratio spectrum to the moon (right). Zero intensity is at the bottom. The author obtained this and the following spectrum in April-June of 1972 using the coude scanner of the 2.7 m telescope at the McDonald Observatory.

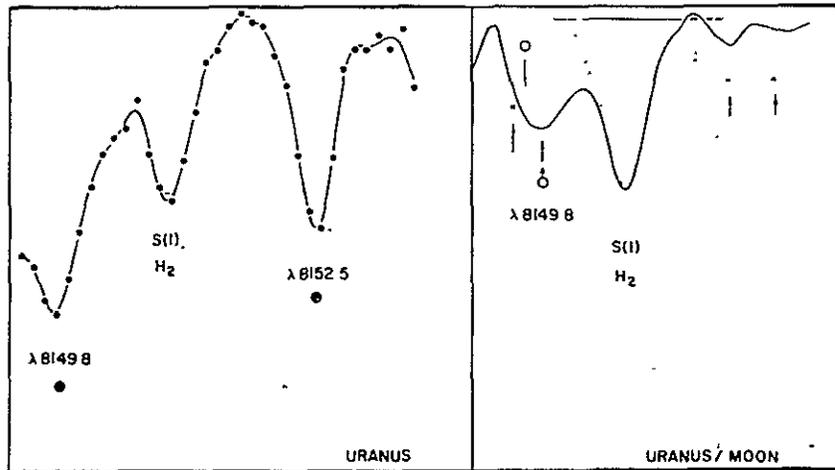


Figure 2-29. Scanner spectra of Uranus' $S_3(1)$ H_2 quadrupole line (left) with ratio to moon (right). Zero intensity in the bottom. Another feature is visible at $\lambda 8149.8$ near a solar line. For each quadrupole line, the equivalent width is significantly greater than the value Lutz (1972) obtained.

This year, I have obtained significantly higher resolution spectra of these lines. These will be published before long and they approximately confirm the lower-resolution spectra.

The values I get for the equivalent widths are somewhat larger than Lutz (1973) gets. The lowest I have gotten is around 120 milli-angstroms with a large uncertainty way back in 1970 but the more recent results, which better resolve these features from the telluric ones, indicate values in the 150 to 160 milli-angstrom range. They imply that the hydrogen abundance along the mean free path is significantly greater at this wavelength than at the 4-0 wavelengths. Or, to state that another way, that the mean free path is larger at this wavelength than at the bluer wavelength, indicating that the particulate scattering, since it is unlikely to have a very sharp wavelength dependence, does not totally dominate the Rayleigh scattering.

Before we discuss the pressure induced absorptions, Table 2-5 is a tabulation by Encrenaz and Owen (1973) of a number of equivalent widths obtained by various investigators for these lines up to the last apparition. They also quote hydrogen abundances which typically fall in the range 450 plus or minus 100 kilometer amagats. These abundances assume Fink and Belton's (1969) curve of growth for hydrogen and neglect such effects as the pressure shifts and new pressure-broadening coefficients. Consequently, they are too large.

As Danielson and Macy have suggested, pressure shifts of the hydrogen quadrupole lines must be taken into account in analyzing the equivalent widths of Uranus' hydrogen quadrupole lines. McKellar (1974) has shown that consequently the derived hydrogen abundance can be 25% to 50% less. His results are plotted in Figure 2-30.

DR. BELTON: Do you have the resolution to verify this effect by measuring the width of the line?

DR. TRAFTON: We can resolve about .03 angstroms with the echelle but the planet's rotation will smear the spectrum owing to the Doppler effect. The slit would have to be set along the central meridian during very good seeing to minimize that effect.

Since one cannot neglect the pressure shifts, the hydrogen abundances quoted in the literature for Uranus are probably too large. However, I measure larger equivalent widths for the 3-0 lines than Lutz (1973) obtained; this may partially offset the effect of taking pressure into account in the analysis.

Table 2-5. Hydrogen Lines on Saturn and Uranus

PLANET	BAND	LINE	EQUIVALENT WIDTH mÅ	ABUNDANCE H ₂ km amagat	T K	REFERENCE
Saturn	(3-0)	S(1)	65 ± 10	190 ± 40	90	Owen (1969)
Saturn	(4-0)	S(0)	11.1 ± 5			Giver and Spinrad (1966)
		S(1)	11.9 ± 3			
Saturn	(3-0)	S(0)	48 ± 4	76 ± 20	77 ± 16	This paper
			- 7			
		S(1)	41 + 7			
			- 3			
Saturn	(4-0)	S(0)	9 + 5	78 ± 20	80 ± 26	This paper
			- 3			
		S(1)	8 + 3			
			- 2			
Uranus	(4-0)	S(0)	30 ± 8	480	118 ± 40	Belton et al. (1971)
		S(1)	41 + 15			from Giver and Spinrad (1966)
			- 9			
Uranus	(4-0)	S(0)	30 ± 8	670	118 ± 40	Lutz (1973)
		S(1)	41 + 15			from Giver and Spinrad (1966)
			- 9			
Uranus	(4-0)	S(0)	62 ± 6	780 ± 200	78 + 15 - 10	Price (1973)
		S(1)	58 ± 4			
Uranus	(4-0)	S(0)	30 ± 3			Trafton (1973)
		S(1)	29 ± 3			
Uranus	(4-0)	S(0)	30 + 4	460 ± 140	82 ± 20	This paper
			- 3			
		S(1)	29 + 6			
			- 3			
Uranus	(3-0)	S(0)	95 ± 25	435 ± 100	111 ± 30	Lutz (1973)
		S(1)	114 ± 15			
Uranus	(3-0)	S(0)	170 ± 18			Trafton (1973)
		S(1)	167 ± 20			

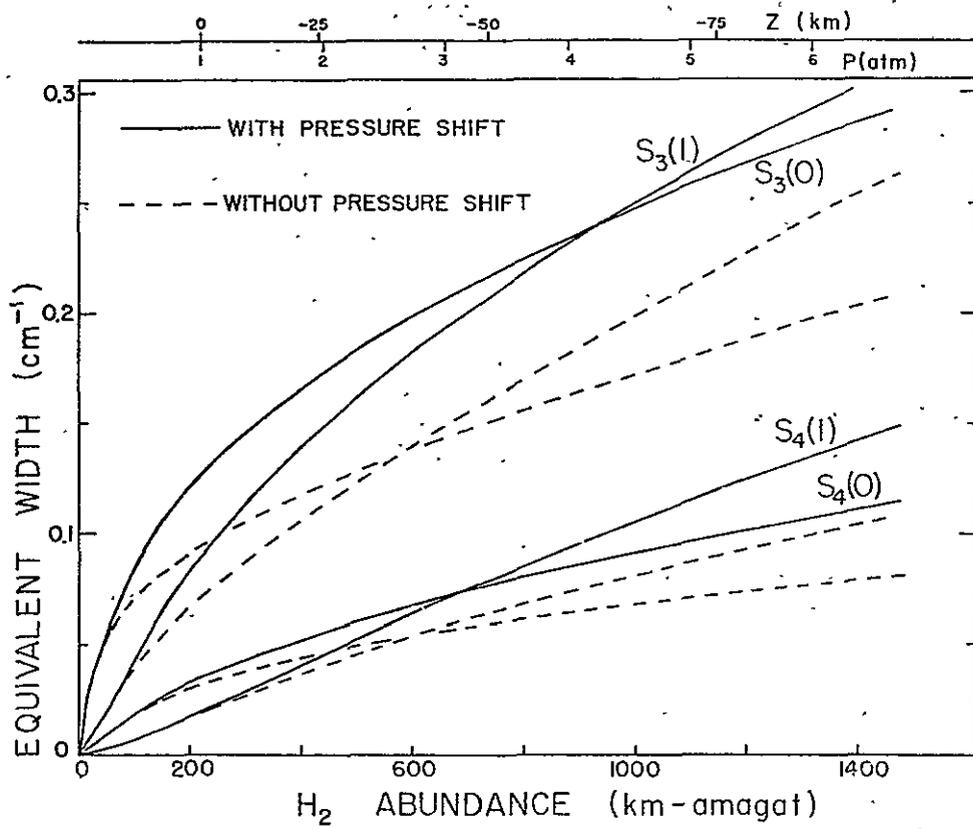


Figure 2-30. The effect of pressure shift on the curve of growth for the H₂ quadrupole lines for Uranus' atmosphere (from McKellar 1974).

B. Pressure-Induced Spectrum

The next series of figures I presented at the 1972 meeting of the Division of Planetary Sciences in Kona, Hawaii on the pressure-induced absorption for Uranus. Figure 2-31 shows the spectrum of Uranus and the Moon in the vicinity of the 3-0 pressure-induced band of hydrogen at λ 8272. The intensity zero for Uranus is indicated by the tic mark. The bottom border is 40% above the Moon's zero. The wavelength ranges from λ 8050 to λ 8360. Superposed are the R, P and Q branches of a telluric water band. The Q branch at λ 8226 is prominent. The resolution element (1.3 angstroms) is small compared with the width of the pressure-induced feature (\sim 75 angstroms) because I wanted to get a resolution high enough to depict the continuum between the water lines so that when I ratioed Uranus to the Moon or the Sun, I would be confident about the resulting shape of the pressure-induced feature.

Figure 2-32 shows the corresponding ratio spectrum of Uranus to the Sun, and also the ratio spectrum of the star Eta Boo to the Sun. This demonstrates the success in cancelling out the telluric water band in this region. Therefore, this should be the correct behavior of Uranus' spectrum. The solar and stellar features have not been cancelled owing to the finite Doppler shift with respect to the Earth. This does not affect the shape of the spectrum, though.

Figure 2-33 shows three independent spectra ratioed to the sun in the region of the pressure-induced absorption. Detailed comparison indicates which features are real. Notice the existence of a number of sharp spectral features on the right. Presumably, these are manifolds on the wing of the 8400 angstrom methane band. These are quite pronounced and regular. This suggests that they might be amenable to analysis to provide information about the Uranian atmosphere.

Figure 2-34 shows a schematic spectrum of the pressure-induced absorption superposed on the best of the three Uranus spectra partially illustrated in Figure 2-33. We note the correspondence of wavelengths of the S(0) features with the λ 8270 one, and the apparent absence of a feature at the location of the S(1) band. This feature may be hidden in the strong methane absorption in the neighborhood.

More recently, Belton and Spinrad (1973) have observed Uranus' pressure-induced spectrum, at an order of magnitude less resolution, but they also have analyzed it more thoroughly. Their ratio spectra for the 3-0 band are shown in Figure 2-35 which includes all of the major planets. The shape they derive is a bit different from the one I get probably because of the influence of the telluric water band on the low-resolution ratio spectra but the absolute reflectivity of the band is probably quite adequate for the analysis they performed.

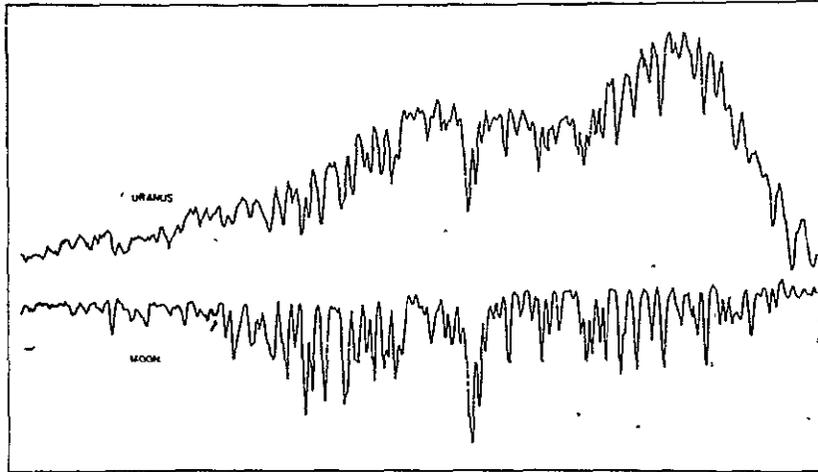


Figure 2-31. Scanner observations of Uranus and the Moon at 1.3 Å resolution for $\lambda 8050$ to $\lambda 8360$ covering the pressure-induced H_2 (3-0) band. Intensity is plotted versus wavelength. The intensity zero for Uranus is given by the tic mark. For the Moon, the bottom of the graph is 40% from the zero to the continuum level. The lunar spectrum shows the presence of a telluric H_2O band with a Q branch at $\lambda 8226$.

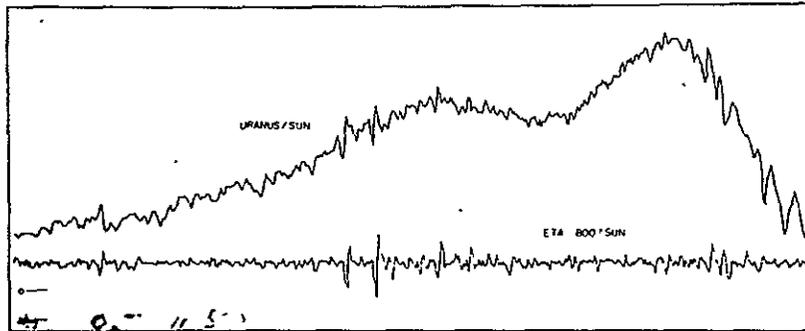


Figure 2-32. Ratio spectra of Uranus/Sun and Eta Boo/Sun showing cancellation of the telluric H_2O band. The solar features are not completely cancelled owing to a Doppler shift.

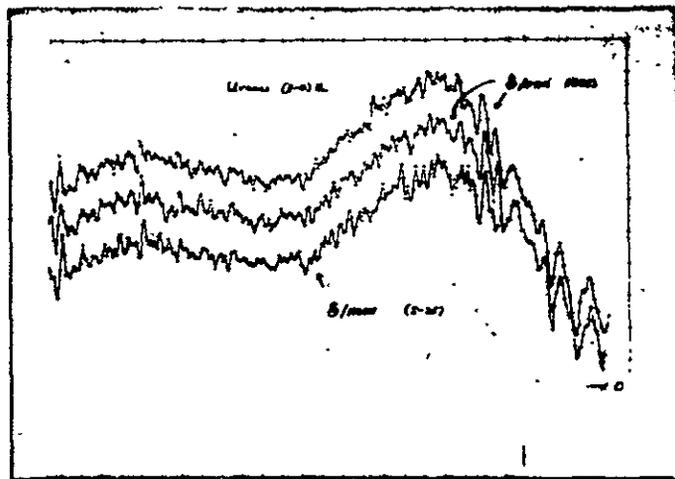


Figure 2-33. Three independent ratio spectra of the (3-0) pressure induced H_2 band and CH_4 absorptions in Uranus' spectrum. I obtained the top two (Uranus/Sun) at 1.3\AA resolution with an RCA GaAs photomultiplier tube using the Coude scanner of the 2.7 m telescope at the McDonald Observatory. I obtained the lower spectrum using a photomultiplier having an S-24 photocathode. Different Doppler shifts are represented. The parallel structure must be real, revealing many weak absorption features in Uranus' spectrum. Sharp features appear on the right third of the spectrum suggestive of CH_4 manifolds or possibly Q branches (presented at the 1972 meeting of the Division of Planetary Science).

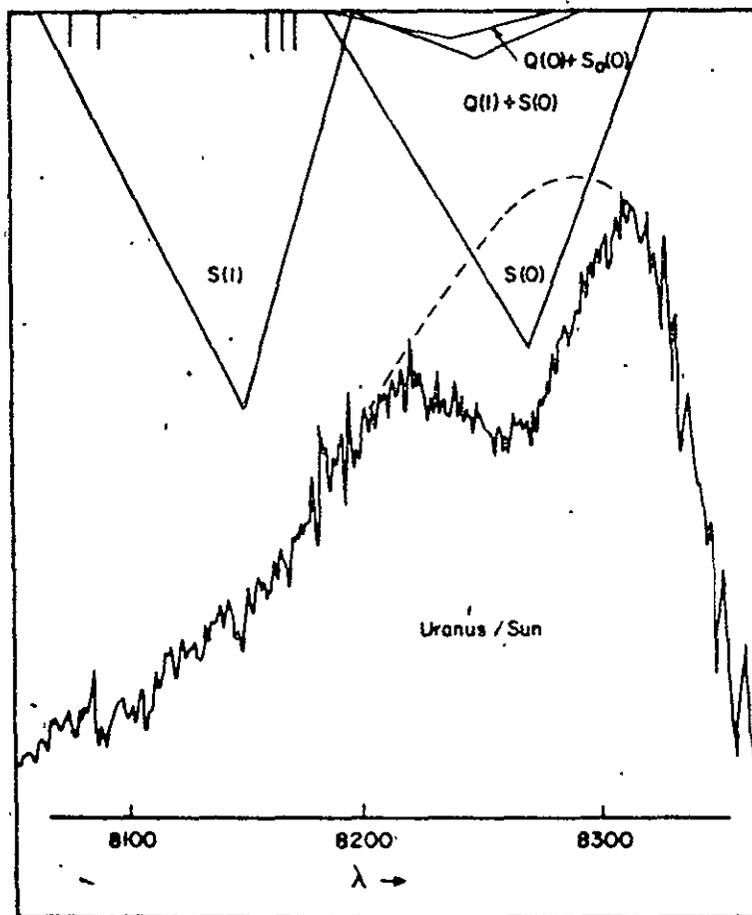


Figure 2-34. Schematic spectrum of the pressure-induced (3-0) H_2 absorption superposed on the best of the three Uranus ratio spectra partially illustrated in figure 2-38. (Presented at the 1972 meeting of the Division of Planetary Sciences.)

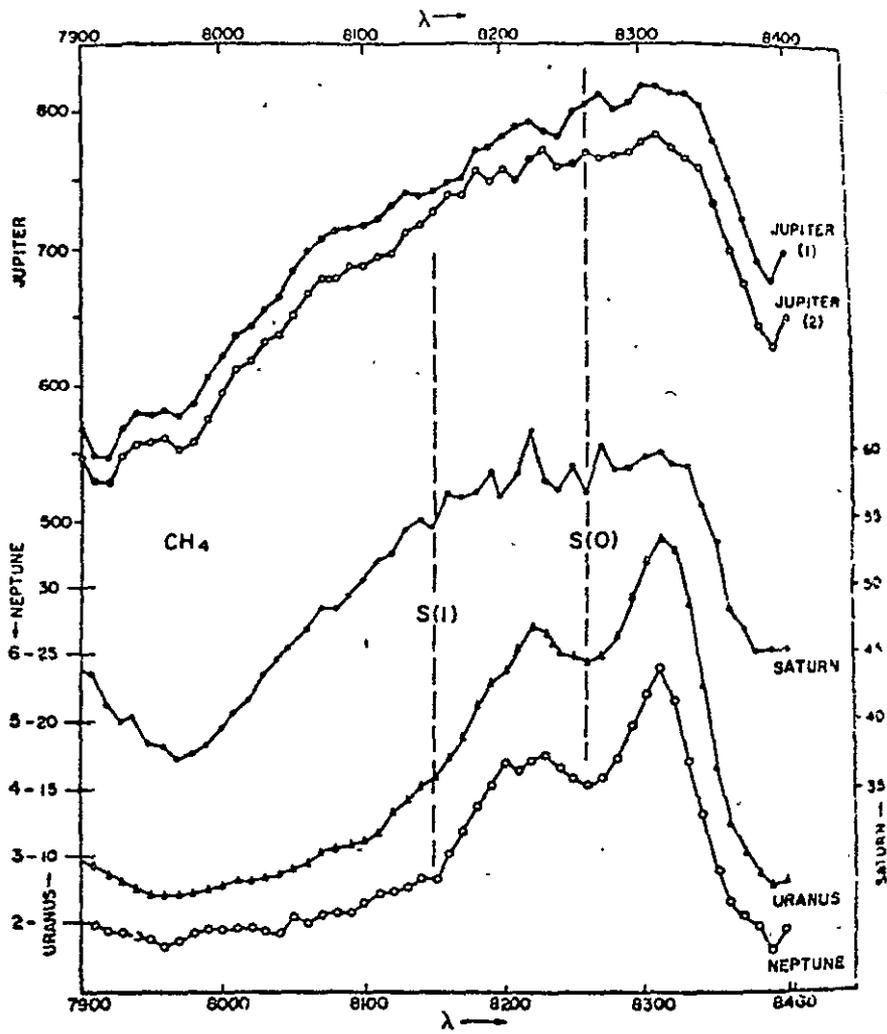


Figure 2-35. Intermediate resolution ($\sim 15 \text{ \AA}$) ratio spectra of the major planets covering the (3-0) H_2 pressure-induced region. (From Belton and Spinrad 1973).

DR. BELTON: Do you have an independent check of the reflectivity?

DR. TRAFTON: Not directly. I am just following what you did.

DR. POLLACK: I am getting worried about all the speculation here --

DR. TRAFTON: It is not quite speculation. I have slightly lower resolution ratio-spectra of Uranus covering the blue to infrared. Normalizing these by Younkin's and Wamsteker's albedos in the blue and green, I agree with Belton and Spinrad's value for the albedo at $\lambda 8270$, or perhaps get a slightly lower value. Since absorption bands in the blue and green are weak, the low-resolution albedos there are probably safe to use.

Figure 2-36 shows Belton and Spinrad's results when they fit a pure hydrogen model to their spectra. They find it is too dark at the wavelength of the pressure-induced feature so a pure-hydrogen atmosphere is inadequate. I have checked this calculation and agree. There has to be some kind of scatterer, either gaseous or particulate, in order to raise the albedo to the observed value. They reject the gaseous case claiming that it gives hydrogen quadrupole equivalent widths which are too small. They suggest that some distribution of particulate scatters and possibly a cloud layer is present. There may, of course, be gases other than hydrogen mixed with aerosols.

On Figure 2-37, they make two points; (1) The 3-0 band dominates the methane absorption while methane dominates the absorption in the neighborhood of the 4-0 overtone. (2) They question, as well as Owen and perhaps others, whether the $\lambda 6420$ feature is, therefore, really part of the 4-0 band of hydrogen.

Figure 2-38 presents Giver and Spinrad's (1966) spectra for the 4-0 overtone feature. Even at this high resolution, the global continuum is not resolved. The absorption falls short of the global continuum, suggesting that a broad, pressure induced, feature may be present after all.

If still higher resolution spectra yield the same result, we could argue that this depression should indeed be due to the pressure-induced feature, not just due to many superimposed methane lines.

In Figure 2-39 we have some laboratory spectra obtained by Owen, et al. (1974), showing that there is a methane absorption at the same wavelength as the 4-0 pressure-induced feature. They argue therefore, that much of this feature must be methane. This further complicates any attempt at analysis.

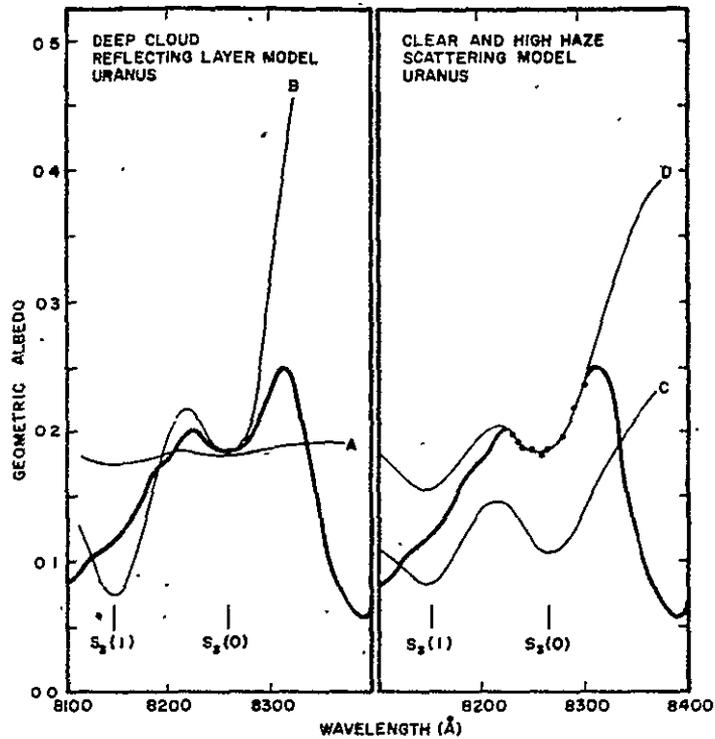


Figure 2-36. Modelling the pressure induced spectrum. Heavy lines and dots are the geometric albedo of Uranus in the vicinity of the $S_3(0)$ pressure-induced transition. Curve A, a dense cloud (Lambertian; surface reflection coefficient of 0.29) overlaid by 100 km-atm of H_2 ; curve B, a model with a conservative cloud deck overlaid by 500 km-atm of H_2 ; curve C, clear, pure H_2 , atmosphere, $3-0$ transitions in H_2 only; curve D, a hazy atmosphere: pure H_2 with a cloud scattering coefficient of $5.5 \times 10^{-9} \text{ cm}^{-1} \text{ amagat}^{-1}$ of H_2 . (Belton and Spinard, 1973.)

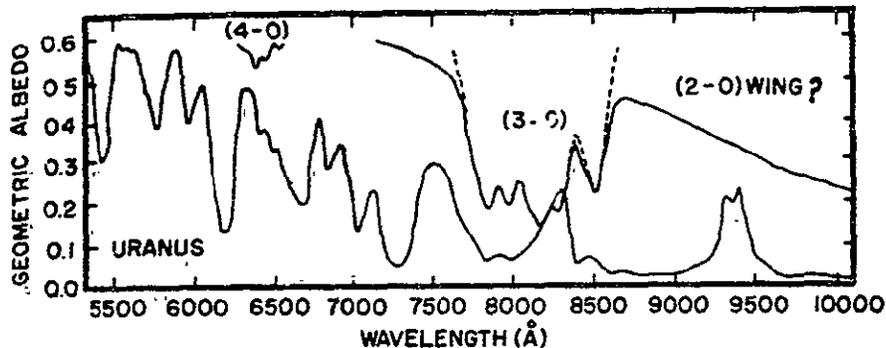


Figure 2-37. Near infrared geometric albedo of Uranus (adapted from Younkin 1970). Albedos have been adjusted to a radius of 25,900 km. Also shown are the predictions of the hazy scattering Model (cloud scattering coefficient of $5.5 \times 10^{-9} \text{ cm}^{-1} \text{ amagat}^{-1}$ of H_2). The broad wing feature extending across the 3-0 complex is due to the 2-0 band, with extrapolation of the MacTaggart and Hunt's line profile. This disappears if the Bosomworth and Gush (1965) "exponentially modified tail" profile is used. Results using the latter profile are shown as dotted lines. The strongest absorption features in the calculated profiles at S(1) transitions. The calculated S(0) transition fall immediately to the red of the S(1) transitions.

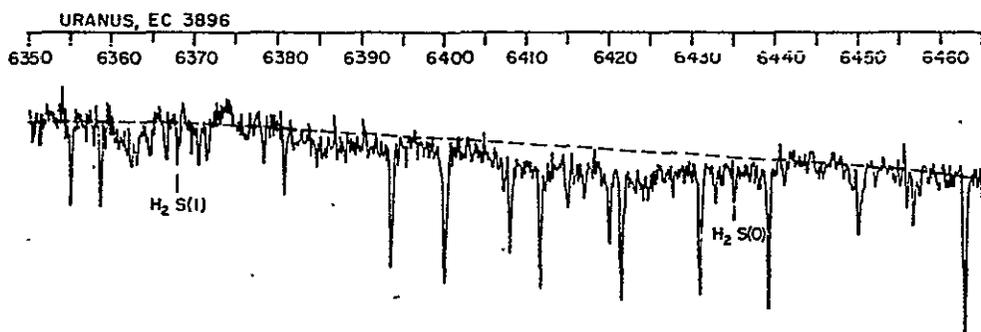


Figure 2-38. Intensity tracing of the spectrogram of Uranus EC 3896 in the region of the broad pressure-induced $H_2 S(0)$ (4,0) dipole line centered at $\lambda 6420$. The $S(0)$ and $S(1)$ (4,0) quadrupole lines are also indicated. (Giver and Spinrad, 1966.)

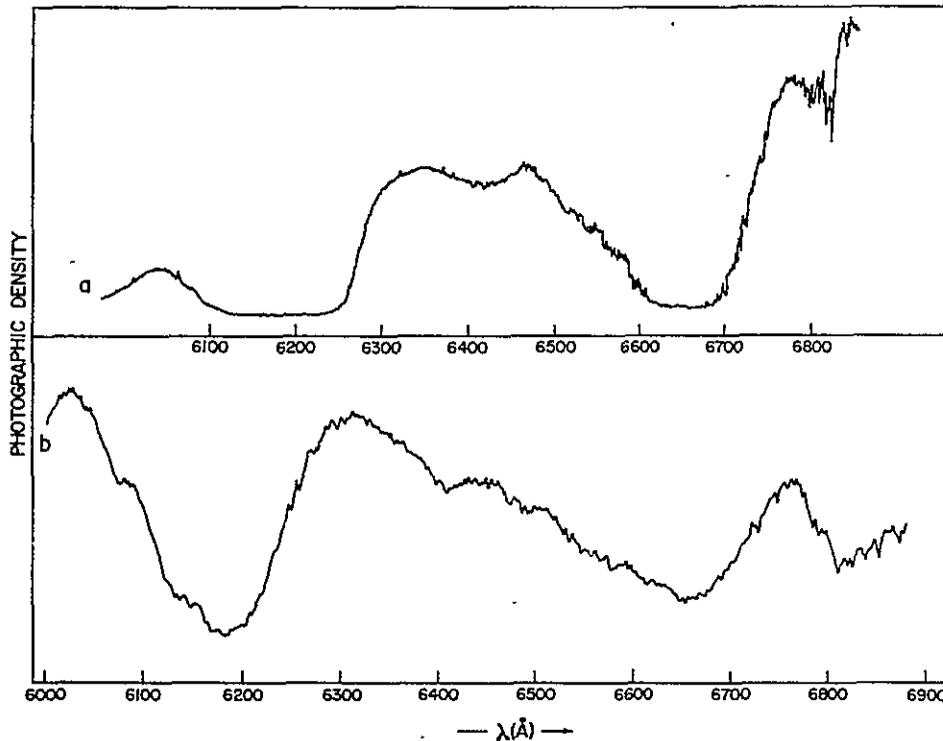


Figure 2-39. (a) Density tracing of the 10.1 km amagat spectrum of methane, without the wavelength comparison lines, taken with the 33-m NRCC White cell. The rotational structure longward of 6800 has been tentatively identified as $5\nu_3$ by Owen (1966) (b) Intensity spectrum of Uranus divided by the lunar spectrum in the same spectral region as curve a. Note the slight difference in scales, and that the ordinate labeling of "photographic density" applies only to curve a. (Owen, *et al.*, 1974.)

In Figure 2-40 we can see the result of some observations I presented in Hawaii of this band. There is no telluric band to worry about here and the solar spectrum has been cancelled out, as you can determine from the spectrum.

Can you see the distortions of the local continuum lying near the predicted position of various components of the 4-0 quadrupole feature, and even one around the predicted position of the S-1 feature? Although the wavelength correspondences are not excellent, these would suggest that at least part of this depression arises from pressure-induced hydrogen; but, again, analysis is not practical because we don't know what part is due to the pressure-induced absorption and what part is due to methane.

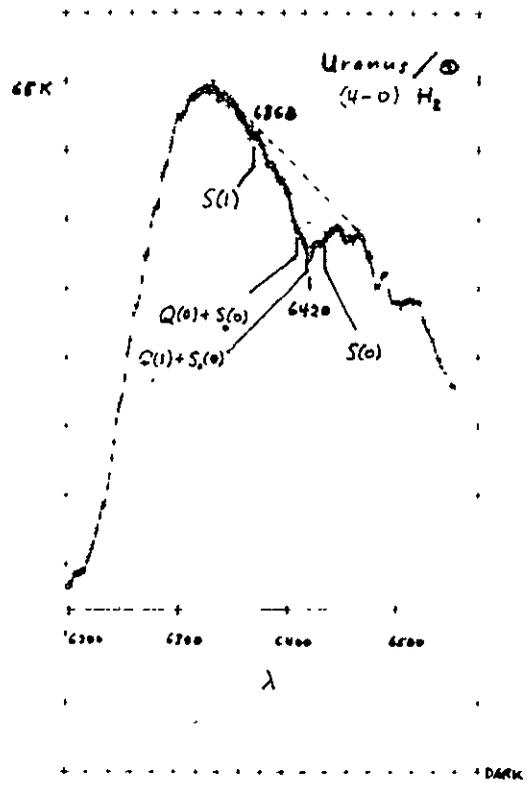


Figure 2-40. Ratio spectrum Uranus/Sun in the spectral region of the (4-0) H₂ pressure induced band. Zero intensity is at the bottom. Part of this feature may indeed arise from H₂ rather than entirely from CH₄ (presented at the 1972 Meeting of the Division of Planetary Sciences).

I will now discuss one final category of pressure-induced absorption. Danielson (1974) has recently proposed that simultaneous transitions in methane-hydrogen collisions contribute significantly to the Uranian spectrum. The wavelengths he predicts for these features are shown in Figure 2-41. The spectrum is hypothesized to be the methane spectrum with superposed ghost methane spectra shifted by the rotational-vibrational hydrogen frequencies. Since the methane transitions are permitted, the pressure-induced simultaneous transition should be roughly comparable in probability to the pure hydrogen pressure-induced transition. Therefore, these various components may arise from the simultaneous transitions. If so, the Uranus spectrum contains more information about the atmospheric structure than we have suspected. Caution should be exercised, however, since Owen has observed some of these weak, supposedly double transitions in a laboratory methane spectrum. Therefore, it is an open question just what role double transitions play in the Uranus spectrum.

C. The Raman Spectrum

As far as I know the first measurements of the Raman effect for Uranus were those by Belton, Wallace and Price (1973), shown in Figure 2-42(a). These include the vicinity of the H and K lines of calcium and are ratioed to comparison spectra (b). They modeled the Uranian spectrum theoretically, using the populations of the two lowest rotational states of hydrogen for the free parameter. The thin lines represent the theoretical spectrum and the strong lines the observed spectrum. Figures 2-42(c) and (d) show the corresponding ratio spectra for the case of all the molecules in the ground state and in the first excited rotational state, respectively. Interpolating, they find that the percentage Raman component is almost as much as for a pure hydrogen atmosphere. They find it to be about 3% for the Raman S(0) line and about 8% for the Raman S(1) component. The best fit to the observations is shown in Figure 2-42(e) which includes the Raman K₁ and H₀ lines, and traces of the H₁ and maybe the K₀ lines, depending on where the continuum is.

Similar measurements by Woodman et al. (1974) claim to set an upper limit of 5% to the Raman component of the spectrum. Although neither of these observations is precise enough to reveal much about the role of scattering or particulate matter in the Uranian atmosphere, I think that we have not by any means gone as far as we can go in getting high quality observations for exploiting the Raman effect. A work has recently been published by Fast et al. (1974) using an auto correlation technique for detecting Raman features. Something may result from better observational data.

DR. WALLACE: But what can you learn from it? We thought we could learn something from this when we did it, but --

DR. TRAFTON: Hopefully, you can learn something about the role of scattering. If there is a lot of particle scattering compared to Rayleigh or Raman scattering, then the Raman spectrum should be weak. On the other hand, if the Raman component is around 13%, then all you learn is that it looks like a pure hydrogen atmosphere. It remains to be seen what we can learn.

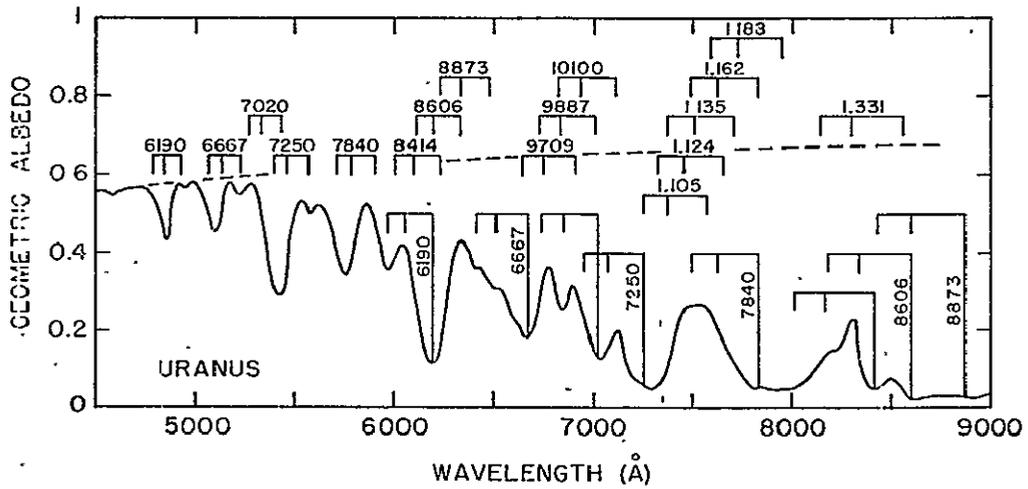


Figure 2-41. Proposed identification of features in Uranus' spectrum in terms of double transitions from $\text{CH}_4\text{-H}_2$ collisions. (From Danielson 1974.)

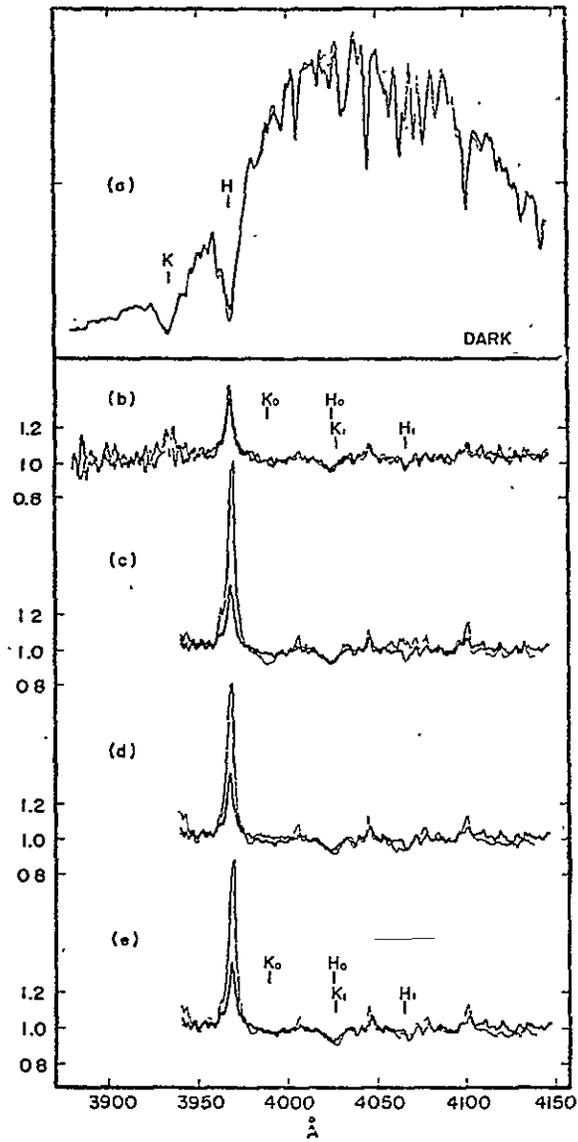


Figure 2-42. Detection of the Raman effect in Uranus' spectrum (From Belton et al., 1973).

DR. POLLACK: What is the reason that it fits so badly, that one spike?

TRAFTON AND OTHERS: The large spike at the H line is due to filling-in (Ring effect) of this line in the lunar comparison spectrum. At least part of this is due to the solid-state equivalent of the Raman effect. It would be much better to use the Sun for the comparison, but that was not feasible at the time. The effect is also present in the spectrum of the day sky.

DR. TRAFTON: But the Raman effect also occurs in the Earth's atmosphere.

DR. WALLACE: It probably occurs in the clouds of Uranus also to some extent, but not with the particular shift characteristic of H₂.

DR. TRAFTON: Another thing this kind of observation can tell you is something about the composition of the atmosphere. For example, is there a lot of nitrogen in the Uranian atmosphere? If we got a Raman spectrum above the Earth's atmosphere from a large space telescope, we might be able to identify interesting constituents of the Uranian and other atmospheres.

IV. HAZE

I would like to conclude with some additional remarks about the particulate scattering. I have already mentioned some in discussing the pressure-induced absorptions. Most of you are aware of Prinn and Lewis' (1973) thermodynamic arguments for a high methane haze layer, based on the freezing of methane at lower temperatures.

The Stratoscope II observations, (Danielson et al. 1972) as shown in Figure 2-43, imply that Uranus does not have an infinitely deep Rayleigh scattering atmosphere. The limb-darkening is too shallow. It is more characteristic of a finite Rayleigh atmosphere overlying the cloud surface.

Then I also have some evidence from the 1 micron methane bands, shown in Figure 2-44, which are ratioed to the lunar spectrum. A band this strong ought to be black in the laboratory but it has asymptotically a non-zero value in the spectrum of Uranus. This is also the case for the λ 8900 band. This filling-in suggests that there is some scattering in the Uranian atmosphere. It doesn't have to arise from particles, however, it could arise from a gas. To check the possible stray light in the spectrograph, we have looked at the center of the λ 7600 oxygen band where these lines come down and flatten at some non-zero value. We measure how high off the zero level these very strong lines are and that turns out to be significantly less than indicated in Figure 2-44.

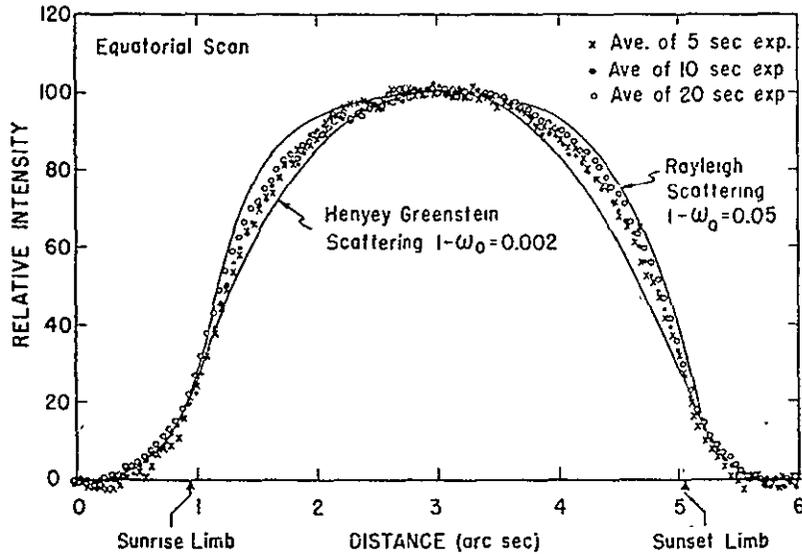
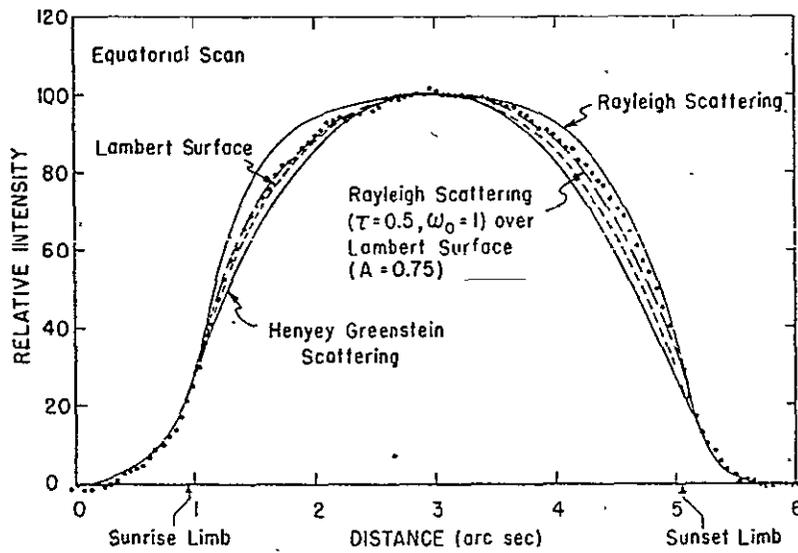


Figure 2-43. Comparison of Rayleigh scattering and Henyey-Greenstein limb-darkening curves (blurred with the instrumental function) with the observed limb-darkening curves based on the averages of the 5-, 10-, and 20-s exposures of Uranus. The difference between the observed curves is a measure of the systematic errors in the photometric corrections. (From Danielson et al., 1972.)



Comparison of the four theoretical limb-darkening curves (blurred with the instrumental function) with the observed limb-darkening curve based on the average of all 17 Uranus images. (From Danielson et al., 1972.)

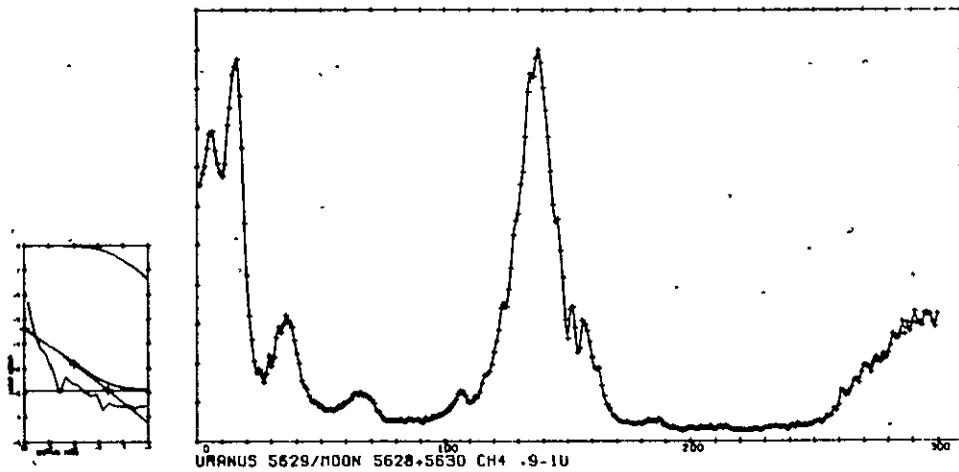


Figure 2-44. Uranus' spectrum from 0.82μ to 1.08μ showing the strength of the CH_4 absorption and a tapering lower envelope which may result from aerosols in Uranus' atmosphere. In the absence of aerosols, the 1μ and 0.9μ bands would be black.

Notice the apparent wavelength dependence of the envelope. If this is really "black" at 1 micron, then we have a fairly sharp wavelength dependence of whatever is causing the scattering. I haven't looked at this quantitatively, but I would suggest that particles don't scatter with wavelength dependencies this large so this behavior would perhaps, be more characteristic of Rayleigh scattering. It is something that should be looked into because it bears on the question of what is causing the scattering, another gas or particles.

DR. POLLACK: How much does the wavelength change over there?

DR. TRAFTON: The graph runs from about 8200 to 10,700 angstroms.

DR. HUNTEN: Methane particles, though, would be expected to have a wobbly albedo in any region where methane gas absorbs.

DR. TRAFTON: Yes, if the particle sizes are on the order of the wavelength of light, this may also be true. My comments are not by any means definitive.

Then there is a limb-brightening observation mentioned by Westphal (1972) and observed by Sinton (1972), but these, as Belton and Price (1973) have shown, do not necessarily imply the existence of a high cloud layer. They could result from a single-scattering albedo which decreases with depth. An absorbing gas could accomplish this.

DR. BELTON: The pressure-induced continuum absorption would increase strongly with depth and if it became very, very strong, it perhaps would be ammonia.

DR. POLLACK: What is the limb brightening observation?

DR. TRAFTON: This is an observation in the center of a strong methane band, like at 8900 angstroms, where one scans spatially across the disc of Uranus in the presence of some amount of seeing, and you see an increase in brightness from the center to the limb. This is what you would expect if it were a high cloud layer. But it is also what you would expect if you had an inhomogeneous atmosphere where the single scattering albedo decreases with increasing depth.

DR. OWEN: But that isn't all you see in these scans, right? If you believe them, you also see some structure in the center and that gets harder to explain.

DR. TRAFTON: Yes, except that may result from seeing and scintillation effects because these scans are a collection of individual scans where the seeing or scintillation varies with time. I am not convinced that the structure is real.

DR. OWEN: There are images, too, that indicate some feature.

DR. TRAFTON: There is one more point I want to make about the particles. Figure 2-45 presents the OAO observations of Uranus (Savage and Caldwell 1974) showing that the albedo in this region is significantly less than you would expect for a semi-infinite Rayleigh scattering atmosphere, indicating some substance with reflectivity less than unity in the atmosphere. This cannot be a gas unless the gas somehow absorbs at these wavelengths. Some aerosol is probably the cause.

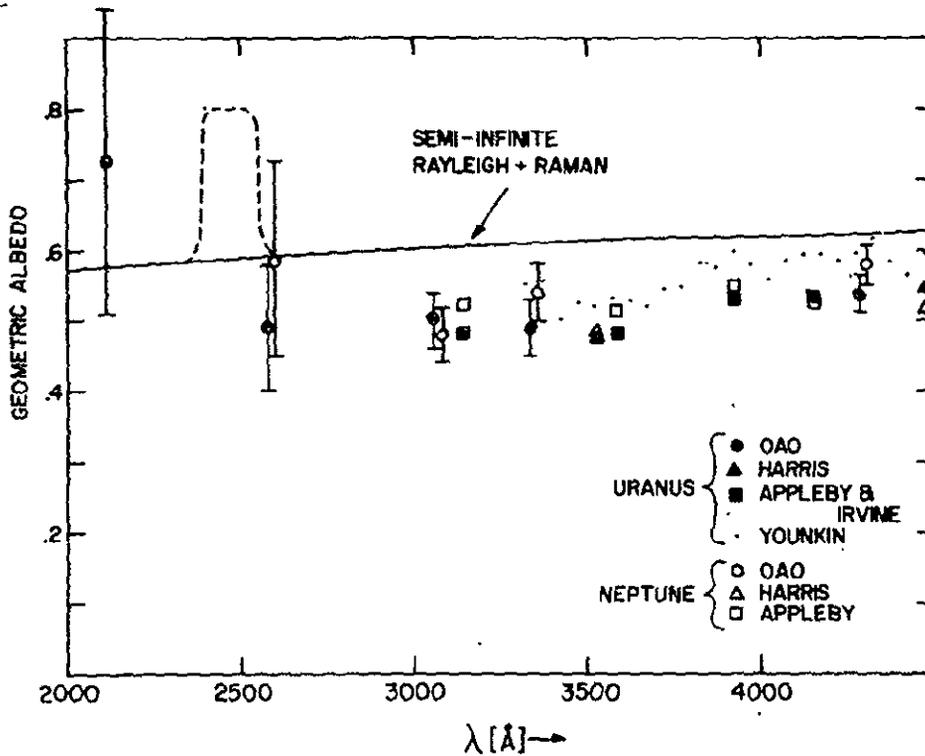


Figure 2-45. OAO-2 geometrical albedo measurements for Uranus and Neptune along with a number of ground-based measurements for these planets. All data have been plotted at the effective wavelengths for a solar-type energy distribution. The error bars on the OAO data points are discussed in the text. The solid curve is an approximate theoretical curve for a pure semi-infinite Rayleigh-Raman scattering atmosphere. The dashed bump in the region 2400-2550 Å illustrates a wavelength interval where the approximation used in obtaining this theoretical curve becomes invalid. In this region a more accurate theoretical albedo would probably lie somewhere between the solid and dashed curve. (Savage and Caldwell, 1974).

V. SUMMARY

DR. TRAFTON: I see the methane abundances as being fairly large, certainly larger than the visible abundance in Saturn's atmosphere and probably larger than almost two kilometer-amagats visible in Titan's atmosphere. Also the 1.06 micron features, are probably not methane so that they indicate the presence of another gas.

Hydrogen is likely to be more abundant than the sum of the remaining gases in this atmosphere. If this is not true, then I don't think it misses the mark by far. There is some question about the hydrogen abundance because of the uncertainty in such effects as the pressure shift. We have no evidence that likely minor constituents such as ammonia, ethane, hydrogen sulfide or ethylene exist. We have not yet detected HD. This is difficult because of the local methane absorption.

On the pressure-induced spectrum, I think there is no doubt that the $\lambda 8270$ feature is the S(0) band of the pressure-induced spectrum. I suspect that part of the $\lambda 6420$ feature is the 4-0 pressure-induced spectrum, but at least part of it must be methane.

Finally, there is evidence of aerosols in the Uranian atmosphere.

DR. POLLACK: What would you put as the upper and lower bounds of the methane-to-hydrogen ratio?

DR. TRAFTON: That is a tough one. I think the abundance for spectroscopically visible methane could be very roughly 30 times what it is in Saturn's atmosphere, and we have about 50 meter amagats of methane visible in Saturn's atmosphere at intermediate latitudes. In Uranus' upper atmosphere, methane is probably frozen out, so there this ratio is variable. As for the hydrogen abundance, we should await an analysis of the hydrogen lines we take into account the pressure shift but a tentative value for a mean lower limit to this ratio would be, very roughly, 0.005.

2.5.2 Abundances in the Uranus Atmosphere

TOBIAS C. OWEN

You have already been looking at a lot of spectra at various resolutions, and I want to show you a few more.

The 5500-7000 \AA region of the spectrum of Uranus divided by the moon, and Jupiter divided by the moon is shown in Figure 2-46; the 4000 to 5500 \AA region is given in Figure 2-47.

I want to call your attention to the feature in the spectrum of Uranus at 5430 \AA approximately; you can see that there is a band at this wavelength in the spectrum of Jupiter as well. This had actually been reported some 40 years ago by Adel and Slipher (1934) but people have not paid much attention to this early paper, perhaps because the observational techniques were much less sophisticated than those available now. Anyway, that band is definitely there. (cf. Bugaenko et al. 1971).

Next to it, there is a little absorption in Jupiter which doesn't seem to be present on Uranus at 5500 \AA . This is an ammonia band that was first identified at high resolution (Owen 1971). It is definitely visible on this low resolution scan.

That gives us some confidence in the spectro-photometry and makes us think that we are seeing this band (5760 \AA) and possibly even this one (5970 \AA) in the Jupiter spectrum.

DR. WALLACE: You are using an awful lot of imagination there.

DR. OWEN: Perhaps! It would look better if the wavelength scale were compressed; you could see it a lot more clearly.

DR. OWEN: This is, of course, 6190 \AA , which you have seen before. And this is the region at 6800 \AA where there is a band which may or may not be $5\nu_3$ but, in any case, it has some clearly defined rotational structure.

The basic difficulty that we have in attempting to interpret these spectra is that we are lacking laboratory data. The 6190 \AA band, as far as I know, does not have a published band strength. Lacking such data, we decided to use Jupiter as our "laboratory standard".

The problem we set ourselves was, if we can see these bands in Jupiter's spectrum and if we adopt the methane abundance determined for Jupiter, can we calculate the methane abundance on Uranus?

I went to Bob Cess with this problem, and he used a band model he had derived with a student (Cess and Ramanathan 1972) for other purposes to interpret these observations. Adopting a value of 450 kilometer amagats for the hydrogen abundance on Uranus, he found a mixing ratio for CH_4 to H_2 on the order of .03 to .02, depending on the amount of helium that is assumed to be present. If you enrich the helium, then you need less methane because you are increasing the effective pressure.

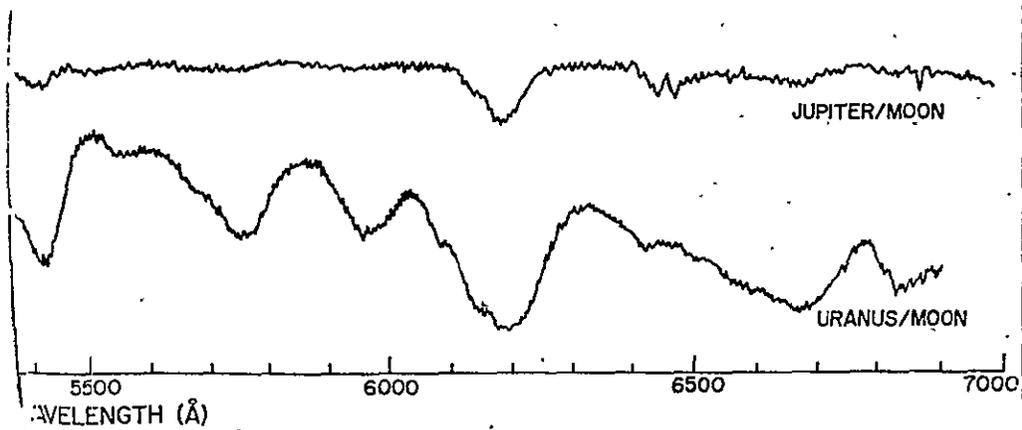


Figure 2-46. Spectra of Jupiter and Uranus in the region 4000 to 7000 Å. Each planetary spectrum has been divided by the lunar spectrum to remove the solar and telluric absorptions. All spectra were recorded in collaboration with J. H. Woodman using the Tull coude scanner at the 2.7m telescope of the McDonald Observatory.

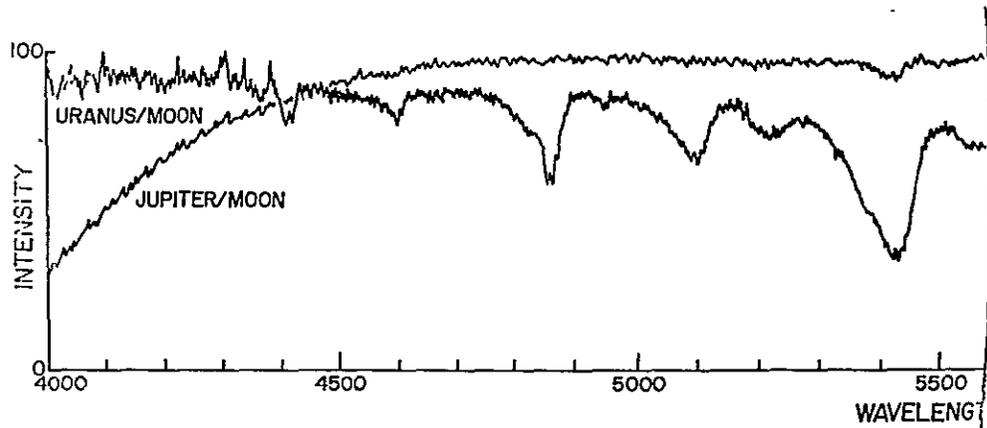


Figure 2-47. Spectra of Jupiter and Uranus in the region 4000 to 7000 Å. Each planetary spectrum has been divided by the lunar spectrum to remove the solar and telluric absorptions. All spectra were recorded in collaboration with J. H. Woodman using the Tull coude scanner at the 2.7m telescope of the McDonald Observatory.

DR. BELTON: That is on Jupiter or Uranus?

DR. OWEN: This is the mixing ratio on Uranus, assuming that you know the mixing ratio on Jupiter.

DR. WALLACE: What type of band model?

DR. OWEN: The model has the following form:

$$A = 2A_0 \ln \left[1 + \frac{u}{2 + \sqrt{u(1 + 1/\beta)}} \right]$$

Where A is the equivalent width of the band, A_0 is the bandwidth parameter and

$$u = \frac{\eta S [\text{CH}_4]}{A_0} \quad \beta = \frac{4\gamma_0 P_{\text{eff}}}{d}$$

I should mention that we are in the process of applying this model to methane bands in spectra of Titan and all of the outer planets; we hope to have some results worth publishing very shortly (Owen and Cess, 1975).

DR. WALLACE: The result is bound to be sensitive to the model that is used.

DR. OWEN: Certainly; this model has been used to interpret carbon dioxide absorptions on Mars and Venus and it seems to work quite well.

DR. POLLACK: Is it a scattering model?

DR. OWEN: No, it is Simple-Reflection, that is what we are talking about.

DR. POLLACK: Now that abundance determination is significantly above previous numbers.

DR. OWEN: That is correct, it is very much higher. And this was encouraging to me because the problem that we have had for the last few years in trying to interpret these bands with laboratory spectra is that we simply cannot get a long enough absorption path in the laboratory to reproduce the intensities of the bands as they are seen in the spectra of Uranus and Neptune (Encrenaz et al. 1974, Owen et al. 1974).

As Larry Trafton has already mentioned, Belton and Hayes (1974) have suggested that the short wavelength bands are strongly enhanced because of Rayleigh scattering in the atmosphere. Therefore, the absorption intensities that you measure here are not really the true estimate of the amount of gas in the column as compared with the abundance that you measure at longer wavelengths.

DR. BELTON: That is backwards:

The Rayleigh scattering would, in fact, lead to enhanced abundances in the red rather than the blue. What gives you the peak of the abundance, higher abundance at 5500Å, is the competition between that increase due to Rayleigh scattering towards the red and the rapid fall off of continuum albedo past 5000Å towards the red.

So the increase comes about because the continuum albedo is a maximum in the blue.

DR. OWEN: How do you set the continuum albedo, then?

DR. BELTON: I used Prinn and Lewis.

DR. OWEN: To restate our result, it corresponds to path lengths like, 12 kilometer amagats of methane, for a one-way path in the atmosphere.

DR. POLLACK: Toby, one more question as to how that number was arrived at: a theoretical model was used to compute what the actual absorption would be in the methane bands?

DR. OWEN: You just take a model for the band and into that model you put the band strength S , the line half-width, γ_0 , the spacing of the lines d , and the effective pressure. There is a square root temperature dependence and you also need to know a bandwidth parameter. Then you determine the band strength by using Jupiter as your calibration, and you use the standard spacing of 10.5 cm^{-1} for methane. Then, if you know what the hydrogen abundance is, you use that to calculate the pressure on Uranus.

In the laboratory we have just looked at some long path spectra and we find that we don't see the weak bands with path lengths of about 10 kilometer amagats. That would, of course, correspond to the two-way traversal so that says that you have got to have more than about five meter amagats in a single traversal, the exact number being a function of the air mass you assume.

Now Figure 2-47 is carrying the spectrum to shorter and shorter wavelengths. And I call your attention to the absorption band at 4410 angstroms. As I recall the curve in Belton and Hayes (1974) for the competition between the continuum albedo and Rayleigh scattering this band occurs at about the same place on the ordinate scale as the 6800Å band. And that means that the effective path length for absorption in the atmosphere of Uranus should be about the same in the two cases, so that this band ought to show up in the laboratory spectra that show the 6800Å band if I understand what you are saying correctly, and if the continuum albedo is right, which we have from Lewis and Prinn.

DR. BELTON: If it is methane.

DR. OWEN: If it is methane. Well, that is sort of where we are. I think it is a basic problem, and I don't see an immediate resolution of it.

I think that if we could just get a laboratory band strength for one of these bands, it doesn't really matter which one, it could be 5430 \AA or the 6190 \AA which is not a difficult measurement to make, we would finally know where we are. We need the absolute calibration, and that is what we just don't have.

DR. BELTON: There is one problem that has scared me. I think in each case you have to decide whether it is an allowed band or whether it is a pressure-induced feature, in which case you can really get screwed up.

DR. OWEN: The 4410 \AA band, or any of these others.

DR. BELTON: Yes.

DR. OWEN: When you have the band strengths to use in a proper curve of growth analysis, then you will know what you are up against.

DR. BELTON: I might make one comment: when comparing these short wavelengths, one uneasy feeling I have had is that on weak absorptions (as seen in the lab), the effect of temperature could be, as you go from room temperature down to Uranus, to concentrate the band strength toward the center of the band. So on Uranus you tend to see sharp, relatively strong features whereas in the lab it would tend to get washed out because of the higher excitation.

DR. OWEN: That is absolutely true, and we see that effect.

DR. BELTON: I would like to get a measurement, but intuitively I would say you could get rid of a factor of maybe two to five or something of this discrepancy just from that one single fact.

DR. OWEN: I think that may well be true; and again, I just feel frustrated not having the laboratory data. The only data we have are photographic data, and they are just wholly unsuitable for this kind of analysis. On the other hand, the band model calculations should be independent of this effect and yet they tend to support the conclusions based on the laboratory data.

It is interesting that even when Kuiper (1952) did his original line of sight observations and when Adel and Slipher (1934) did their work, they did not study the short wavelength bands. And Adel and Slipher made the point that they were unable to see these features in their laboratory spectra so they were forced to extrapolate to obtain their values of methane abundances.

We in fact, see the 4860 \AA band in the laboratory spectra obtained by Dr. D. A. Ramsay.

DR. WHITEHEAD: I think before the subject of spectra is dropped completely, we ought to list the spectra that would be useful to take in the lab so that people who have the equipment can be encouraged to take it under the conditions that would be useful. Maybe a list on the blackboard?

DR. OWEN: I would say that you simply get as many of these methane bands as you can. It is as simple as that; measure the ones you can measure.

DR. HUNTEN: And under preferably at least two different pressure conditions as well. Efforts to do this are underway in at least two places that I know of. It is not an easy task. The people doing it are Boese, here at Ames, and Dick and Fink, at Kitt Peak and LPL.

DR. BELTON: I think also it would be interesting to go right down to the other end of the pressure-induced methane-hydrogen spectra, way down in the translational region and you might as well throw in hydrogen while you are at it.

DR. TRAFTON: Would you expect these bands to lie on the linear part of the curve of growth, these weak ones in the Uranus spectrum?

DR. OWEN: It is very hard to tell. I think the 4860Å, for example, probably does not. Some of the weaker bands may; the 4950Å might be in the linear region.

DR. TRAFTON: So they would be independent of the pressure whereas the stronger bands in the red would be a function of the pressure and this may introduce a dichotomy in the interpretation of the strengths of the blue and red methane features.

DR. HUNTEN: We all agree, we need a variety of lab spectra under several conditions.

Note added June, 1975: Some of these spectra have recently been obtained by B. Lutz, and do seem to change the conclusions reached above and in Owen and Cess (1975). The direction of the change is to keep the methane abundance the same (approximately 6 kilometer amagats, or a mixing ratio of 1% to 3%), but it is not possible to make any statement about the atmospheric pressure.

2.6 CLOUDS

2.6.1 Clouds

Dr. Michael Belton

DR. BELTON: What evidence exists for clouds on Uranus? Is there any direct evidence? I have one picture with a belt on it; maybe a lot of information from visual observers who say they saw markings (see Alexander, 1965). Maybe there is some tenuous evidence of clouds there, but you certainly wouldn't want to stick your neck out too far.

This picture (which I am not free to publish) is from New Mexico State University; it was taken under the direction of Brad Smith and was given to me by R. B. Minton.

DR. POLLACK: Where is the belt on there?

DR. BELTON: You have to stand at about three feet away and cringe your eyes a little bit to get it under the right illumination, but I think you can see it.

Actually, I have just completed writing a paper (for Icarus) about why take pictures of Uranus. In that paper I argue strongly that one must take what the visual observers say in general rather seriously. When they see markings, I believe that they really see markings.

On the other hand, when it comes down to interpretation of the details of those markings, that is where I feel that the visual observers go wrong or get into controversies. The basic question to whether they really see a marking or not is probably: Yes, if they said they did, they did.

DR. POLLACK: The point is, though, at what point in the physical/physiological system does that marking get introduced, whether it gets introduced on the end of the telescope where Uranus is or whether it gets introduced somewhere in the computer processing.

DR. BELTON: Well, I did talk to Dollfus a little bit about that because I didn't know the answer to that question. It seems that a telescope generally gets rid of the clouds, so you have to have very definite markings and you have to have a very good telescope before you can see these kinds of things. Generally, the effect of a slightly-out-of-tune telescope is to eradicate detail.

I don't want to pursue this too far; I just want to put a question mark. Maybe there is direct visual evidence for clouds.

What other direct evidence is there? There is only one that I know of, and that is the one that Larry Trafton has already talked about. The methane bands aren't black where they are supposed to be black. Also the (3-0) pressure-induced hydrogen overtone -- (Figure 2-37) -- is not as black as it would be if you just had a pure hydrogen, Rayleigh scattering type atmosphere.

DR. HUNTEN: I guess I should have asked Larry this; it has more to do with the spectrograph. Obviously, the non-blackness of the methane bands certainly indicates there is some scattering present, but it doesn't say whether or not the scattering is just Rayleigh scattering.

DR. TRAFTON: We would have to look at the wavelength dependence on that. As I pointed out during my talk, if you compare the one micron band with the .9 micron band, it seems like both of them really hit an asymptote above zero.

In the case of .9 micron band, that is significantly elevated with respect to the one micron band, suggesting fairly sharp wavelength dependence. The question is can you exclude particulate scattering as a result of this observation.

DR. BELTON: My own feeling about this is that, and I haven't looked at the center of methane bands, but in the work that Spinrad and I (1973) did on the 3-0 pressure-induced overtone hydrogen, we tried to explain the observations just with adding another molecule of some species and try to fill in the absorption, with Rayleigh scattering due to that molecule. We tried helium and we tried methane. You come up with numbers like if you have 300 times as much helium as hydrogen, then you could fit the band. But in that case you got so much Rayleigh scattering that you don't have any quadrupole lines left. So you cross it out. And the same thing with methane. You can't balance the allowed transitions and pressure-induced transitions very well by just going along that route. So our conclusion, from the 3-0 pressure-induced overtone was that we had to have some particulates.

DR. TRAFTON: By the way, why is that physically? It seems to me that both the gas and particles will be very bright, have high albedos. The only essential difference is that one contributes to the pressure. Do you really need methane abundances three times the hydrogen abundance in order to raise the albedo?

DR. BELTON: In that transition, that is the way the numbers came out.

DR. TRAFTON: It is just the relationship between geometric albedo and single scattering albedo, you just need something with a single scattering albedo high enough to cause the gas abundance to go up that much. It just seems odd to me that the mean free path in the aerosol is always within a factor of two or three of the gases. It seems like too strong a coincidence.

DR. BELTON: I agree with you. It makes me feel very uncomfortable.

DR. POLLACK: Doesn't that really depend upon how well mixed your aerosols are? In other words, if you had a case of a nice sharp cloud layer like you sometimes get with water clouds, then what you say is certainly correct. But if you have the top part of a haze layer that is just barely able to condense, you naturally get that sort of coincidence.

DR. TRAFTON: Still, it does seem like a large coincidence that it is so small.

DR. BELTON: Nevertheless, maybe we should move on here. I don't know what the resolution of that problem is.

DR. JOHNSON: What about that limb-darkening curve the Stratoscope portrayed?

DR. BELTON: I think that it can be perfectly rationally explained by clouds but it also can be rationally explained by pressure-induced continuum absorption.

So we could put limb brightening and methane absorption "for" particulates. On the other hand, there really isn't anything "against" clouds except that you do see an awful lot of H₂ and, of course, its interpretation is questionable; that was the basis of the Belton-Price-McElroy paper. Now those results are very much in question with McKellar's point about the pressure shifts. Which, I think, is right.

There is also the business of Raman scattering, but we also have to put a question mark on that. It was inconclusive from the interpretive point of view. Observationally, it probably is there; that is my feeling because I really feel that Wallace, Price and I have a positive detection. But, on the other hand, when you sit down and try to analyze the Raman feature, it doesn't tell you very much. It tells you there are no very thick clouds.

That is the end of the empirical evidence.

Table 2-6. Evidence of Clouds on Uranus

For	Against
Visual markings (?)	Hydrogen abundance (?)
Center of Strong Bands (CH ₄ , H ₂)	Raman Scattering (?)
Limb Brightening (?)	

To sum it up, (Table 2-6) what does it mean? It means that there probably is a cloud there. It doesn't, however, tell you where the clouds are except that they are not, say, down below the limit of visibility in H₂ as determined by its Rayleigh scattering cross section, so it is probably up above the "several atmosphere" level.

DR. OWEN: When you say clouds, incidentally, what are you really talking about? It is aerosols of some kind but that is not to say that you have necessarily a condensation cloud that is very well defined. You could have a haze in the atmosphere. What is it you are really speaking of?

DR. BELTON: I am thinking of something in the atmosphere that has a cross-section for scattering which is ten orders of magnitude greater than Rayleigh. Anything that has that is what I call a "cloud particle".

DR. HUNTEN: We are not talking of just clouds, we are talking of clouds or haze or aerosols or --

DR. OWEN: It is really haze that is being talked about.

DR. BELTON: There is something which tells us that there is in the observational record some influence of these cloud scatterers. But it doesn't tell us at all anything about where they are. So now we have to go to some kind of theoretical treatment, and the only one that I know of in the literature is that of Lewis and Prinn, who used three pieces of information. One is Trafton's curve for temperature versus pressure in Uranus, and this one is rather a hot one. I think the effective temperature is about 64°. It could be a little bit cooler. But that is what I think he used. The other two pieces of information are the vapor pressure curves of ammonia and methane, and also the solar mixing ratio for these constituents. With this information we can draw condensation curves which intersect Trafton's model atmosphere and would suggest that clouds would exist in this atmosphere; methane clouds around the 300 millibar level on this picture, and ammonia clouds around about 8 bars.

The question that I want to address now is whether those methane clouds really exist or not because the empirical data could be satisfied either by just having an ammonia cloud deep down and a clear atmosphere above, or it could perhaps be satisfied by just having the methane cloud provide the volume scattering that is required by the observations.

So will the methane condense? Well, maybe; maybe not. I went to Mason's work (1957) on physics of clouds in which the first chapter is devoted to the theory of homogeneous nucleation. Providing you don't have any nuclei for condensation, and this is well documented in the case of water, you can have considerable degrees of super-saturation. This is the basis for the operation of cloud chambers, of course.

Let us assume for the moment that there aren't any condensation nuclei for methane. Maybe it doesn't like to condense. After all, its first electric moment is the octopole moment which is very short range. The molecule is like a billiard ball; it doesn't like to "stick" to itself. It is not a very interactive sort of molecule. So maybe it doesn't like to condense.

In Mason's book there is a law, which isn't really a law at all, but a formula: $(T/\sigma)^{3/2} \ln S \sim \text{Constant} \sim 11$. It is due to a man called Powell. The equation says; If you take the temperature T, and divide it by the surface energy of the bulk liquid, he calls σ , and you carry the result to the 3/2

power and multiply the result by the logarithm of the degree of super-saturation, the final product is roughly constant. If you follow the theory as outlined -- and, as Mason says, it is oversimplified -- this constant mainly depends upon the mean mass of the species that you are interested in. Powell did various experiments with water and found out in fact that as you vary this temperature and measure the degree of super-saturation you can get, this number was roughly constant.

So what I did was just accept this result and, with no other reason except that it seemed like a very good thing to do, said, "Methane and water have mean masses that are similar, and in spite of the fact that the short range molecular forces may be very, very different, let's just assume that the constant in Powell's equation applies to methane as well. In Uranus, we know what "T" is, roughly 60° and that is not going to be far off, surely. And σ , the surface energy for methane, as I said before, while the molecular forces are presumably weaker, it is going to be lower than the value for water. I couldn't find a number for liquid methane to go in there but there are various numbers for other hydrocarbons available and they are all roughly the same value: about a third the value for water. Saturated hydrocarbons apparently do not stick very well. You now ask what kind of degree of super-saturation might we expect on Uranus --

DR. STONE: Is this formula based on how much super-saturation you can get without nuclei before it spontaneously starts to condense?

DR. BELTON: It is based on the fact that the surface energy on a droplet with a curved surface is much less than it is in the bulk form where the surface is flat.

DR. STONE: What physical principle is that equation expressing?

DR. BELTON: Exactly what I just said. Things find it easier to escape from a curved surface, or evaporate, if you like, than they do --

DR. HUNTEN: What is the curvature of a two-molecule droplet?

DR. BELTON: Well, there is a whole theory of nuclei aggregation and the statistical equilibrium of bunches of molecules. This is not a simple thing, it is a very complicated thing.

So I put in, $T \approx 60^{\circ}$, σ a third of the value of water, and accepted the value of 11 for the constant, which the theory says is only strongly dependent on the mass. And for S, the supersaturation of methane on Uranus you get about 19.

So what does that mean for Uranus? This would mean, if we now draw the same kind of curve for the onset of homogeneous nucleation, then that curve would appear as shown in Figure 2-48.

DR. LEWIS: What if you increase the methane-hydrogen ratio relative to solar composition by a factor of 20?

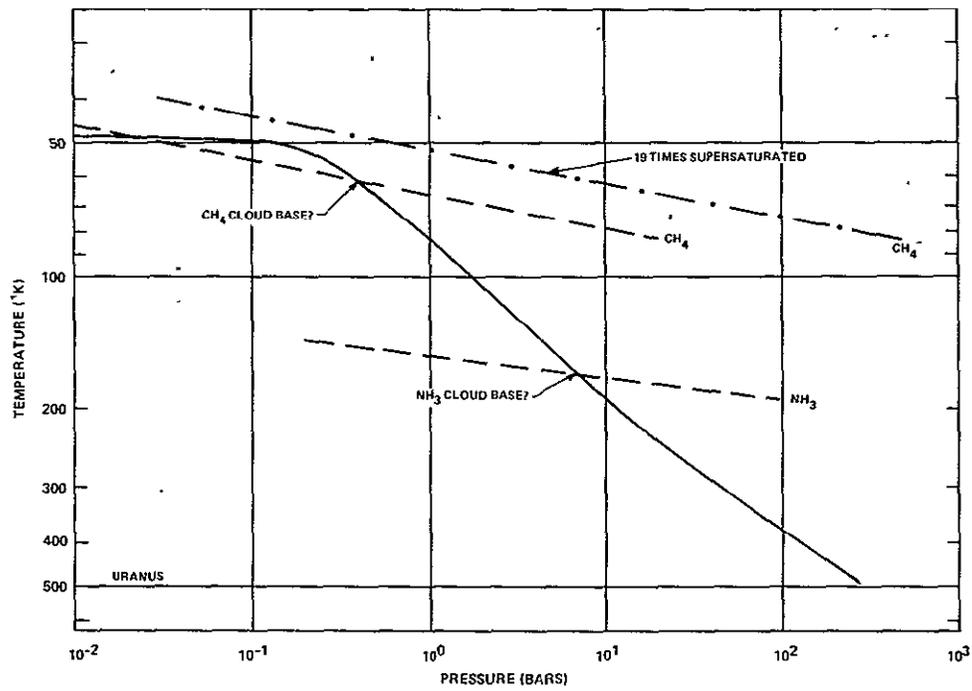


Figure 2-48. Homogeneous nucleation curve.

DR. BELTON: Then the condensation curve would move back to the left and you would still get condensation.

There are other things you can do. You can introduce, say, some other molecule into the system. This would have the effect of moving the curve representing the structure of the atmosphere back and forth on the diagram.

For example, I am not really sure that what I said about the effect of increasing methane by a factor of 20 was completely correct. If we had that much methane, then it could dominate in the translational opacity down at a hundred wave numbers and possibly change the atmospheric structure completely from what Larry Trafton has calculated. It is not clear what would happen. You can't second guess it.

The point I am trying to make is one similar to one that Don Hunten made earlier; you don't just take the saturation curve and see where it crosses and say, "There is a cloud there." I hope everybody agrees with this.

The other point is that there is a theoretical basis for looking at the production, and the growth, of liquid, or solid particles under these conditions, and we should take advantage of it.

DR. POLLACK: You might say on this question of to what degree nuclei in the atmosphere might lower this value, that in the Uranus atmosphere there is a big difference between the ability to nucleate water and your ability to nucleate ice in the sense that you have to be more picky in terms of being able to nucleate ice. In that sense, I would suspect that a solid such as methane would be hard to nucleate. So I would think that perhaps this is a very relevant consideration.

DR. LEWIS: I think I understand the effect, and it has to do with the entropy of condensation. One needs a very particular orientation of the water molecules to make the ice structure.

Methane is a very forgiving solid. Methane can scarcely be made to be free of enormous quantities of lattice defects. It is almost like a partially ordered liquid. Methane molecules care very little how they are oriented to each other.

DR. BELTON: But doesn't that mean also that there aren't any deep potential wells that they kind of fall into when they pull themselves together, just no glue!

DR. LEWIS: Yes, and that is why the vapor pressure is so low. I am referring to the angular, not radial, potentials.

DR. STONE: One other figure that we need to go with this, if you need 19 times supersaturation before this condensation can start. What does that mean in terms of temperature? In other words, how cold does it have to get before it will be that super saturated and condensation will start? Is that a big temperature difference, 40° or so?

DR. BELTON: You can see it on Figure 2-48. It is about 10^0 .

DR. STONE: That is not very big to work with, to say that it won't condense.

DR. OWEN: It seems to me also that you will still have cosmic rays.

DR. LEWIS: That is what I was going to say. Cosmic rays are a beautiful source of ions for condensation nuclei.

DR. OWEN: It is very hard to get to a supersaturation of three on Uranus because of this cosmic ray problem.

DR. BELTON: Well, it depends on the rate, you see, because the nucleated aggregates will naturally tend to break up. Even though the gas is supersaturated, an aggregate will be thermally destroyed at a certain rate.

DR. OWEN: There is some haze there, presumably, from the stuff that is being formed from this.

DR. POLLACK: The cosmic rays act in the sense of forming ions?

DR. BELTON: Correct, as in a Wilson cloud chamber.

DR. WALLACE: As to whether or not you have a cloud, the question I am interested in is how would the mixing ratio of methane to hydrogen drop through that cloud into the higher altitude regions?

DR. POLLACK: Presumably, it just follows the saturation curve.

DR. OWEN: But, to put the question another way, if these mixing ratios are correct, it means that we have to be seeing through the haze, obviously. I mean, it can't be a very thick cloud. But you have an analogous situation on Jupiter with the ammonia, basically.

DR. HUNTEN: There is one other factor that bothers me considerably about the way we make clouds in such an easy fashion on other planets, and that is that a cloud is not an equilibrium phenomenon and we are modeling it with a quasi-equilibrium theory. The only equilibrium state in a cloud is one in which all the particles have fallen out of the region to a warmer level where they evaporate or sublime. You then have a vapor which follows the vapor pressure curve; there is no cloud whatsoever. And, of course, what destroys that situation and gives you a cloud back again is vertical mixing, but there are lots of reasons to imagine that vertical mixing might be much less intense on a planet like Uranus than it is on Jupiter or the Earth.

DR. STONE: "Much less" is something I don't think I can go along with. "Less", yes, but you can balance that off with clouds of smaller particles, and have a lot of leeway in comparing it with, say, the Earth, where we have some pretty large particles.

Mike, what is your best judgment, now, as to clouds; yes or no? Methane clouds.

DR. BELTON: My judgment is that there are aerosols in the atmosphere. I think the observations clearly show it. What I am not certain about right now is where they are. And my best judgment about that is that it is definitely ammonia clouds and I see no reason why it couldn't mix itself up, in spite of what has been said, say, up to the three atmosphere level or something like that, from the eight atmosphere level that John says it could start at.

DR. GULKIS: Why do you say definitely an ammonia cloud? The rest of that picture indicates that it could maybe be sulfide. Maybe the ammonia cloud is missing, too.

DR. BELTON: I agree.

2.6.2 Clouds

Dr. John Lewis

DR. LEWIS: I would say, then that we have come to a resolution of our disagreements, Mike. As long as we don't call the clouds, "clouds", we are there.

I do not have a formal presentation to make on the subject of clouds, I am entirely willing to tell you about what happens when you construct solar composition model atmospheres for Uranus and Neptune; however, I don't think there is any particularly strong reason to go through that tedious exercise once more. I think many of you who are likely to be interested in this have already seen it and those people who aren't, certainly shouldn't be subjected to it.

I am absolutely convinced that the atmospheres of Uranus and Neptune do not have solar composition; but since I don't know what they are, we use solar composition as a point of departure and we can think of modifying the abundances of condensables with respect to that standard.

DR. POLLACK: What absolutely convinces you?

DR. LEWIS: Why am I absolutely convinced that they are not solar composition? The principle of the perversity of nature. Nothing is ever that simple. The fact that everything else in the solar system is variable, complex and Uranus and Neptune do not have solar composition overall.

DR. BELTON: You know, it is very misleading to say that there is or is not solar composition; where do you draw the line?

DR. LEWIS: True! Is it solar composition with respect to what compositional variable? The hydrogen-to-helium ratio? Hydrogen-to-methane ratio? Hydrogen-to-osmium ratio?

These distinctions clearly become more and more important as you get deeper into the atmosphere where the vapor phases of ever more constituents are found. There is an intrinsic limitation to what can be said about the bulk composition of the atmosphere simply from observations made above the top-most cloud or haze layer. Thus, once again, the rationale for entry probes.

DR. POLLACK: One other thing into the pot here about aerosols in the atmosphere of Uranus, and that is, to go to Jupiter for just a second, there is evidence of aerosols really high up in the atmosphere in the case of Jupiter in terms of studies of the passage of sunlight through its atmosphere and taking a look at the occultation of satellites. In that sense, I wonder to what degree that component, whatever the heck it is, might also be present in the case of Uranus?

DR. LEWIS: It is an intriguing question, Jim, but I have no answer. It is a photochemical problem which the present computational techniques are just scarcely able to touch. Darrell Strobel can tell you what happens to methane in the first two steps after it gets photolyzed, but when he tries to push it up to the point of making condensable species, all one can do is guess. I think that is what will happen. But remember, the upper atmosphere temperatures drop in going out from Jupiter to Uranus, and to Neptune, one gets rather precipitous condensation of some of the more simple photolysis products: propane, for example, a propylene will saturate at very low mole fractions and disappear from the gas phase before their photochemistry can start to be interesting. This is one thing, in fact, that we faced in a cursory fashion when we were talking about Titan at this same table last summer, when Carl Sagan was sitting in your chair. In other words, how far does the photochemistry build up before the hydrocarbons condense? Perhaps the best way to talk about this problem is to address what kind of experiments can be done to narrow down the possibilities. It is not going to be that easy to solve theoretically.

2.6.3 Aerosols

Dr. Lloyd Wallace

DR. HUNTEN: I would like to have a statement on the record about UV absorbing aerosols. Do you know, Lloyd? Lloyd talked about their absence on Uranus. Would you care to say a few words or does anybody else have some ideas of the observational situation there?

DR. WALLACE: I don't think they are absent. I think what Don is speaking of is the low ultraviolet albedo of Jupiter, and then Saturn, which has a

similarly low albedo, but may make it a little higher, and then Uranus is distinctly higher and Neptune seems to be a little bit higher still, and this is what Danielson and Axel have referred to as dust in the atmosphere of Jupiter causing this stuff, which is presumably the aerosols that are being discussed. If that is correct, if there are aerosols associated with the methane photochemistry, there is certainly less as you go farther out. But maybe they are not connected with methane photochemistry.

DR. TRAFTON: Figure 2-45 is Tomasko's OAO observations of ultraviolet for Uranus. And they do show fairly smooth, but systematically low value of the ultraviolet albedo, lower, that is, than a semi-infinite Rayleigh plus Raman scattering atmosphere. They attribute this to some constituent of the atmosphere that has reflectivity of less than one. It does seem to rise up around 2200-2300 angstroms.

DR. WALLACE: This is not significant. Look at the error bar.

DR. TRAFTON: That is a big error bar, I will admit.

DR. WALLACE: But I think the Wamsteker Uranus-Neptune ratio says that Neptune is a little bit brighter than Uranus. I suppose if you were going to attempt to learn anything about that stuff, perhaps the best way would be through the infrared. The observation of the 12 micron ethane band and the acetylene on Jupiter was quite a surprise, but I guess everyone should have expected it on a photochemical basis. Perhaps it is another area where one should encourage the infrareders to look around, although that is getting just about impossibly difficult, isn't it?

DR. OWEN: It is very difficult.

2.7 THE STRUCTURE OF THE ATMOSPHERE OF URANUS

Robert E. Danielson

I. INTRODUCTION

The purpose of this paper is to propose a new type of model for the atmosphere of Uranus. This model differs from previous models in two important ways: 1) the CH_4/H_2 ratio below the sublimation level is large ($\geq 10^{-1}$) compared with the solar ratio (7×10^{-4}) and 2) the internal energy is small ($< 10\%$ of the absorbed solar flux). Although neither of these two assumptions are certain, I believe that the available evidence is strongly in their favor. A discussion of the observational evidence is given in Section II.

Because of the small internal energy, the thermal flux decreases rapidly with depth (it varies from 4% at the CH_4 cloud to 100% at the top of the atmosphere in my baseline model - see Figure 2-49). Consequently, the temperature increases more slowly than in constant flux models; if there were no internal source of energy, the temperature would become isothermal in the lower part of the atmosphere. Therefore the model is radiative to large optical depths ($\tau \approx 100$) in spite of the destabilizing influence of the saturated CH_4 vapor. Below this level, the small internal flux is sufficient to produce convection.

A CH_4 droplet cloud forms below the level where the atmosphere becomes convective. At this level, the temperature is about 90°K (near the triple point) and H_2 pressure is about 4 atm. The H_2 abundance above the cloud deck is approximately 400 km-am and the CH_4 abundance (which is very sensitive to the cloud top temperature) is of the order of 5 km-am.

An approximate method, based on the grey approximation, was developed in order to investigate atmospheres with a depth dependent thermal flux. Details of the techniques are presented in Section III. Section IV compares the model with observations. A result of particular interest is the low effective temperature ($T_e \approx 48^\circ\text{K}$) apparently needed to explain the observed thermal emission from Uranus. If so, Uranus radiates only about half as much energy as it receives from the sun. It is suggested that this apparent discrepancy is due to seasonal effects on Uranus which are exacerbated by the large inclination of its axis of rotation. If this is the case, the models presented here are only an approximation to time dependent models which would be needed to describe the planet.

Section V emphasizes that the type of model presented here for Uranus provides a natural explanation of why Neptune appears so similar to Uranus. Provided that the CH_4/H_2 ratio is also large on Neptune and that its internal energy source is small, the atmosphere of Neptune will have properties very similar to Uranus. Section VI contains a discussion of the radio emission problem and suggests some observational and laboratory work which seems particularly promising in advancing our understanding of Uranus.

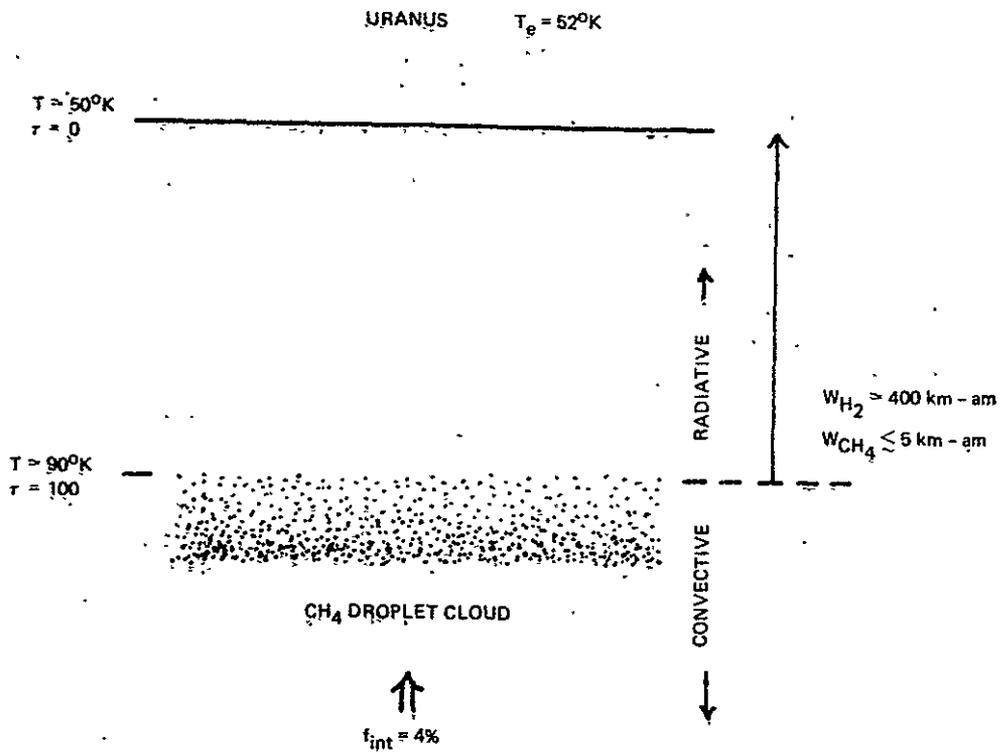


Figure 2-49. A schematic representation of the baseline model of Uranus.

II. OBSERVATIONAL EVIDENCE

A. The H₂ Abundance

Given a model; the H₂ abundance on Uranus can be determined from observations of its quadrupole lines, but the current results are uncertain due to a number of factors. First, the analysis is complicated by the presence of collisional narrowing and pressure shifts (McKellar 1974); the magnitudes of these effects are somewhat uncertain. Second, it is unclear to what extent the (commonly adopted) assumption of Equilibrium H₂ is valid; the rapidity of ortho-para conversion is uncertain. Third, the strength of the 4-0 band has not been measured in the laboratory and its calculated strength (Birnbaum and Poll 1969; Dalgarno, Allison, and Browne 1969) is uncertain by 40% (Birnbaum and Poll 1969). The observed strength of the 3-0 band (Rank, Fink, and Wiggins 1965) agrees with the calculated strength within the error ($\approx 10\%$) of the theoretical calculations (Karl and Poll 1967), but it is more saturated and more difficult to measure than the 4-0 band. Fourth, the observed equivalent widths differ between observers indicating large systematic errors, intrinsic variations, or both.

Within the uncertainty of the 4-0 band strength and the variation of the observed equivalent widths, a model independent lower limit to the line of sight H₂ abundance can be obtained by assuming that the S(0) and S(1) lines are on the linear portion of the curves of growth. (At Uranian temperature, the J = 2 level is not significantly populated.) Most of the measurements of the 4-0 band (Giver and Spinrad 1966; Trafton 1973; Encrénaz and Owen 1973) yield about 30 mÅ ($72 \times 10^{-3} \text{ cm}^{-1}$) for the equivalent width of the S(0) line and about 33 mÅ ($81 \times 10^{-3} \text{ cm}^{-1}$) for the S(1) line. An exception is Price (1973) who obtains values about twice as large. From the strength of the S(0) line ($2.9 \times 10^{-4} \text{ cm}^{-1}/\text{km am}$) and the S(1) line ($2.5 \times 10^{-4} \text{ cm}^{-1}/\text{km am}$), the lower limit to the line of sight abundance is calculated to be about 600 km-am. (Including Price's (1973) observations in the average would increase the lower limit to about 700 km-am.) It appears that the pressure broadening and pressure shift coefficients are sufficiently large that the lines in the 4-0 band are only mildly saturated (McKellar 1974). Thus the true value of the line of sight abundance should not be too much larger than the lower limit. For the purposes of this paper, I am adopting 600 km-am as the lower limit.

A reflecting layer model on a planet with a Lambertian limb darkening law has an air mass factor of 3 for lines on the linear part of the curve of growth. The corresponding lower limit to the abundance above the reflecting layer is 200 km-am. If the scattering layer is diffuse, the equivalent widths tend to be constant over most of the disk (Danielson and Tomasko 1969). In this case, a more appropriate air mass factor is 2; the corresponding lower limit to the abundance above the effective reflecting level is 300 km-am.

A preliminary analysis of the 3-0 quadrupole with a reflecting layer model indicates that the observations of Lutz (1973) imply a H₂ abundance of about 200 km am while those of Trafton (1973) imply about 500 km am. A more detailed analysis will be given in a later paper (Wannier, Light, Cochran, and Danielson 1975, in preparation).

For the purposes of this paper, I am adopting 400 ± 200 km am as the observed value of the H_2 abundance above the effective reflecting level. The large uncertainty reflects not only the preceding discussion, but also the likelihood that the structure of the Uranus atmosphere differs with latitude and with time (see Section IIc).

B. The CH_4/H_2 Ratio

The spectrum of Uranus shows deep absorption features in its spectrum which extend into the visible as far as the blue. (See Figure 2-50.) All of the features longward of 6000\AA seem to be consistent with those expected from overlapping CH_4 bands. From the weakness or absence of bands shortward of 6000\AA in a 10 km am pathlength spectrum of CH_4 at high dispersion, Encrenaz, et al. (1974) concluded that the line of sight abundance of CH_4 on Uranus was much larger than 10 km am and that the CH_4/H_2 ratio was much larger than solar. On the other hand, Belton and Hayes (1975) concluded that the CH_4 abundance was low (1-3 times solar or $CH_4/H_2 \simeq 10^{-3}$) from their analysis of the 6840\AA band.

If the visible features on Uranus were mainly due to simultaneous transitions in colliding H_2 and CH_4 molecules as Danielson (1974) suggested, the conclusions of Belton and Hayes (1974) and Encrenaz, et al. (1974) might have been reconciled. But Dick and Fink (1975) found no evidence for simultaneous transitions in spectra having a CH_4-H_2 abundance product of about 3 km am^2 , probably less than 10% of the product in the atmosphere of Uranus. However, similar spectra of pure CH_4 (about 3 km am) showed several of the strongest visible features including those 4860, 5090, 5430, 5760, and 5970\AA , providing strong evidence that these features are due to very weak CH_4 bands. Recent observations of weak bands at 4860, 5430, and 5760\AA in the spectra of Jupiter, Saturn, and Titan have recently been reported by Owen and Cess (1975). They used a band model to estimate the abundance-total pressure product on Uranus from the abundance of CH_4 on Jupiter (about 50 m-am) and obtained a value of the order of 12 km-am atm. Their analysis, which assumed a constant mixing ratio and a reflecting layer model, implies a CH_4/H_2 ratio greater than 0.01.

However, this large a mixing ratio cannot exist at all depths in the Uranian atmospheres due to the constraints imposed by the CH_4 vapor pressure curve. As the mixing ratio (deep in the atmosphere) increases, the CH_4 sublimation level will occur at lower depths in the atmosphere. Correspondingly, the fraction of the atmosphere for which CH_4 is saturated (or at least limited in pressure by the Clausius-Clapeyron equation) is larger. Indeed, as the CH_4/H_2 mixing ratio becomes very large, CH_4 will be saturated in the entire atmosphere which is observable in visible and infrared wavelengths. Interior models of Uranus calculated by Podolak (1975) indicate that this may, in fact, be the case. The model which best agrees with observations has a vapor ($CH_4 + NH_3 + H_2O$) to rock ratio which is solar, a vapor to H_2 ratio of about 0.5, and a CH_4/H_2 ratio of about 0.2.

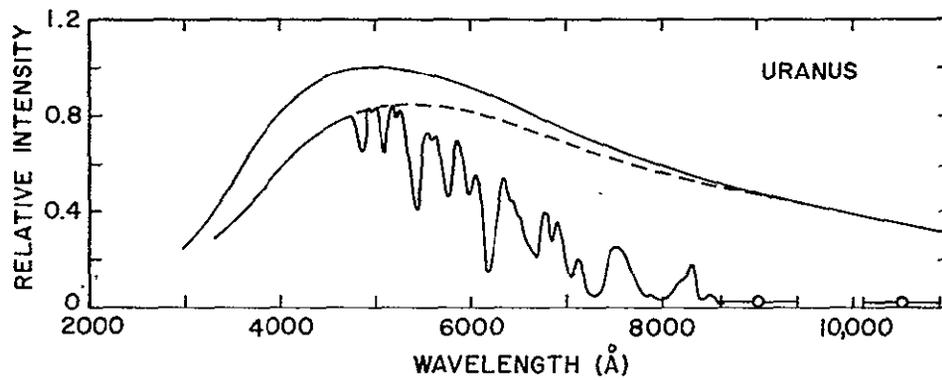


Figure 2-50. The spectrum of Uranus based largely on Younkin's (1970) spectrophotometry: The upper envelope is a smoothed solar spectrum normalized to unity near 5000 Å. The dashed line is a conjecture as to the continuum in the absence of molecular absorption.

Under these circumstances, the line-of-sight abundance of CH_4 is very model sensitive; its value is of the order of 10 km am (see Section IV), an abundance which is consistent with the results of Cess and Owen (1975) but larger than that derived by Belton and Hayes (1975) by about a factor of 10. The observations of Belton and Hayes (1974) would be consistent with higher CH_4 abundance if 1) the true continuum were higher than they assumed, or 2) if the band strength were smaller than assumed. Indeed, their adopted value is a factor of 4 larger than an approximate value of the band strength derived from a laboratory investigation of the 6840 \AA band by Dick and Fink (1975).

For the purposes of this paper, I have adopted a line of sight CH_4 abundance which is of the order of 10 km am.

C. The Energy Balance

Measurements of Younkin (1970) corrected for recent determinations of the diameter of Uranus (Dollfus 1970; Danielson, Tomasko, and Savage 1972) yield a geometric albedo $p^* = 0.25$ for integrated solar radiation. Younkin's (1970) preferred value for the phase integral is $q^* = 1.25$ and the corresponding bolometric bond albedo $A^* = 0.31$. Murphy and Trafton (1974) have adopted $p^* = 0.23$, $q^* = 1.50$, and $A^* = 0.35$.

Although a value of $q = 1.50$ may be appropriate for $\lambda < 5800 \text{\AA}$ where the observed limb darkening approximates that of a Lambert surface (Danielson, Tomasko, and Savage 1972), it is doubtful that this value of q is applicable at the longer wavelengths where much of the sun's insolation occurs and where most of the solar energy deposition occurs. (See Figure 2-49.) To estimate q^* , I have adopted a model of a purely absorbing gas over a conservative Lambert surface, a model which is consistent with that shown in Figure 2-50. By using equations given in Danielson, Caldwell, and Larach (1973), I have calculated q as a function of p (Table 2-7) and find that $q \approx 1$ for the longer wavelengths where p averages a few percent. Thus it appears that Younkin's adopted value of $q^* = 1.25$ is a better value to use than $q^* = 1.50$.

The equilibrium temperature at a distance of 19.18 a.u. for a Bond albedo $A^* = 0.31$ is $T = 58.3^\circ\text{K}$; if $A^* = 0.35$, $T = 57.4^\circ\text{K}$. For the purpose of this paper, I have adopted an equilibrium temperature of 58°K for solar heating.

No measurements have been made of the radiation from Uranus near $\lambda_{\text{max}} = 50\mu$, the wavelength of maximum thermal emission from a 58°K black body. The available measurements, summarized in Table 2-8, are on the short and long wavelength wings of the expected emission spectrum and are therefore uncertain indicators of the effective temperature of Uranus. Nevertheless, it is noteworthy that they are all less than the equilibrium temperature $T_e = 58^\circ\text{K}$. In particular, the 48°K brightness temperature at 33.5μ suggests that $T_e \approx 48^\circ\text{K}$ (see Section IV C) and that Uranus radiates only half as much energy as it receives from the sun. Although the implied internal energy sink is physically unacceptable in the steady state, it may be a plausible result of seasonal effects on Uranus. As indicated in Figure 2-51, the north pole of

URANUS

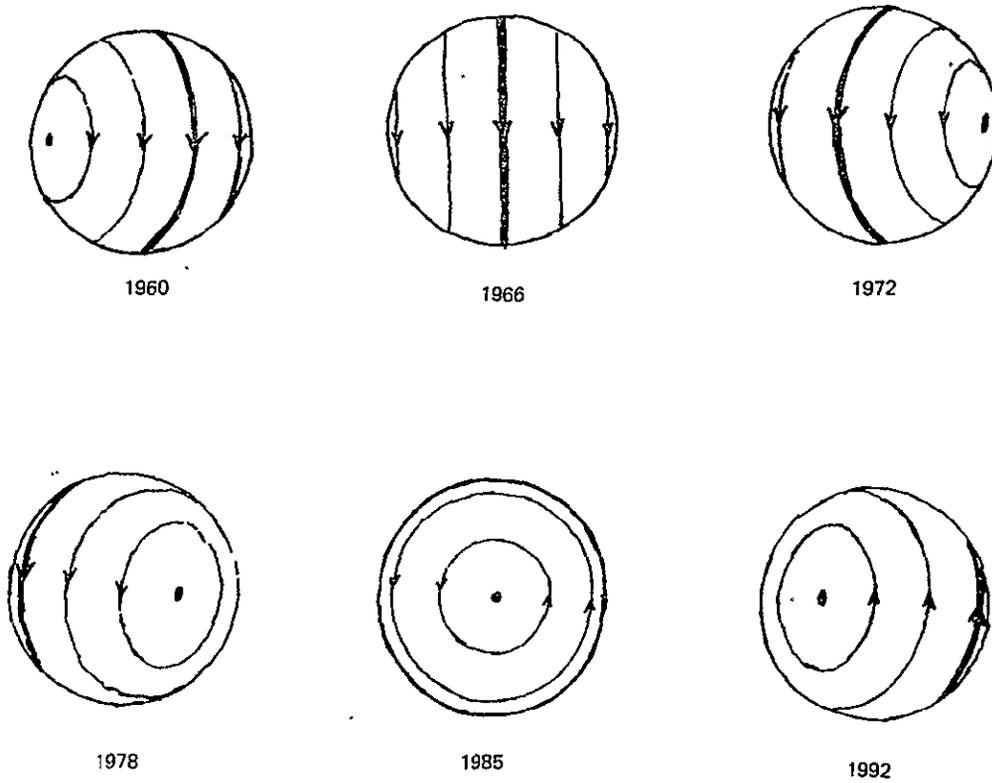


Figure 2-51. Sketches of the changing aspects of Uranus as a function of time, based on Figure 20 in The Planet Uranus by A. F. O'D. Alexander (1965). Faber and Faber, London.

Uranus is now just turning toward us after having been turned away from the sun for more than 40 years. The atmosphere will have cooled substantially in the absence of meridional heat transport. Perhaps at this epoch, the cold hemisphere of Uranus is soaking up solar radiation and thereby radiating less than it absorbs.

In any case, the available data provide no indication of an internal energy source. The presence in Podolak's (1975) preferred model of a rocky core having a mass equal to about $3.5 M_{\text{Earth}}$ implies a minimum flux due to radioactive heating which is only about 1.5% of that due to solar heating. The above percentage is based on a chondritic heating rate of 4×10^{-8} ergs/gm sec (Phinney 1974 - private communication) and a 58°K solar equilibrium temperature.

For the purpose of this paper, I have adopted an internal flux of 4% of the total flux in my baseline model. It may be too large, but it allows for the possibility of a larger core and for other sources of internal energy. The microwave brightness temperature of Uranus increases from about 100°K at $\lambda = 1$ mm to about 200°K at 10 cm (Gulkis and Poynter 1972). The brightness temperature seems to continue to rise for $\lambda > 10$ cm, providing evidence that an internal flux is producing a temperature rise at depths below which solar radiation penetrates. In Podolak's (1975) interior models, the temperature increases to about 3000°K at the rocky core.

D. The Temperature of the Effective Reflecting Level

An upper limit to the NH_3 abundance of 2.5 m-am has been given by Encrenaz, Hardorp, Owen, and Woodman (1974) from the lack of identifiable features in the 6450\AA region. This places an upper limit of about 150°K on the temperature at the effective reflecting level on Uranus.

An upper limit to the mean rotational temperature above the effective reflecting level can also be obtained by assuming that the ortho-para equilibrium in H_2 is rapidly achieved. If it is not, the $j = 1$ level will be populated more than in the equilibrium case leading to a spuriously high temperature. The approximate equality of the equivalent widths of the S(0) and S(1) lines of the 4-0 band suggests an upper limit to the mean rotational temperature of about 85°K .

Belton and Hayes (1975) derived a rotational temperature of 109°K from their analysis of the 6840\AA CH_4 band, much larger than the $60\text{--}70^{\circ}\text{K}$ values obtained by Owen (1966) and Teyfel and Kharitonova (1970) from lower dispersion observations of the same band.

These temperature determinations are subject to systematic errors and are model dependent, but they seem to point to a temperature at the effective reflecting level which is in the range of $80\text{--}120^{\circ}\text{K}$.

III. THE BASELINE MODEL - COMPUTATIONAL TECHNIQUES

A. Radiative-Convective Models With Depth Dependent Fluxes

An approximate technique has been developed to calculate models in which the thermal flux is not constant with depth but varies from the internal flux (4% in the baseline model) to 100% of the total flux at the top of the atmosphere. If one is given the flux $F(\tau)$ as a function of τ , the mean optical depth in the emitted radiation, one can derive the following approximate differential equation for $F(\tau)$ by following the approach outlined by Cess and Khetan (1973):

$$\frac{d^2F}{d\tau^2} - \frac{9}{4} F = -3\sigma \frac{dT^4}{d\tau} \quad (1)$$

where σ is the Stefan-Boltzmann constant.

The boundary condition at $\tau = 0$ is given by:

$$\left(\frac{dF}{d\tau}\right)_0 = \frac{3}{2} (\sigma T_e^4 - 2\sigma T_o^4) \quad (2)$$

where T_e is the effective temperature.

Two approximations are required to derive equations (1) and (2). First the gray approximation is assumed. Second the integral

$$E_2(x) = x \int_x^\infty \frac{e^{-t}}{t^2} dt \quad (3)$$

which appears in the Schwarzschild-Milne Equations (Mihalas 1970, p. 21) is approximated by:

$$E_2(x) \approx \frac{3}{4} \exp\left(-\frac{3}{2} x\right) \quad (4)$$

An analytic solution of equations (1) and (2) can be obtained by assuming $F(\tau)$ is of the form:

$$F(\tau) = F_{int} + \sum_i^n F_i e^{-\tau/\tau_i} \quad (5)$$

where

$$F_0 = F_{\text{int}} + \sum_{i=1}^n F_i$$

F_{int} is the internal flux and τ_i are free parameters describing the depth dependence of the deposition of the solar flux $\sum_i F_i$. The resulting equation is:

$$T^4 = \frac{T_e^4}{2} \left[\left(1 + \sum_{i=1}^n \frac{2f_i}{3\tau_i} \right) + \frac{3}{2} r \left(1 + \sum_{i=1}^n f_i g_i \right) \right] \quad (6)$$

where:

$$f_i = \frac{F_i}{F_0} \quad (7)$$

and

$$g_i(r) = \left[\left(1 - \frac{4}{9\tau_i^2} \right) \left(1 - e^{-r/\tau_i} \right) \left(\frac{\tau_i}{r} \right) \right] - 1 \quad (8)$$

In the limit of $f_i \rightarrow 0$, equation (6) reduces to the familiar Eddington equation for constant flux grey radiative atmospheres.

B. The Mean Opacity

The Rosseland mean optical depth τ of H_2 is related to n , the density (by number) of H_2 by

$$d\tau = -\alpha n^2 dh \quad (9)$$

where α is the Rosseland mean absorption coefficient and h is the depth coordinate. An excellent empirical fit to α which is valid for $He/H_2 = 0.2$ and for $T < 100^\circ K$ is

$$\alpha = 0.065 \left(\frac{T}{100^\circ K} \right)^2 \text{ km}^{-1} \text{ am}^{-2} \quad (10)$$

(Caldwell 1975). However, for the purposes of this paper, I have chosen the computationally simpler, though less accurate expression:

$$a = 0.04 \left(\frac{T}{100^\circ\text{K}} \right) \text{km}^{-1} \text{am}^{-2} = \alpha_0 \left(\frac{T}{T_0} \right) \text{km}^{-1} \text{am}^{-2} \quad (11)$$

where $\alpha_0 = 0.11 \text{ km}^{-1} \text{ am}^{-2}$ and where $T_0 = 273^\circ\text{K}$. Substitution of equation (11) into equation (9) plus the equation of state and the hydrostatic equation yields the following relationship between p , the partial pressure of H_2 , and τ :

$$\tau = 1/2 \beta_0 \bar{H}_0 p^2 \quad (12)$$

where $\beta_0 = 0.11 \text{ km}^{-1} \text{ atm}^{-2}$ and where

$$\bar{H}_0 = \frac{RT_0}{\bar{M}g} \quad (13)$$

is a scale height. Equation (12) assumes that \bar{M} the mean molecular weight is constant. The universal gas constant is R and g is the acceleration due to gravity.

Although some accuracy is lost in deriving equation (12), it provides a relationship between τ and p which is approximately model independent. Its simplicity is very helpful in the present stage of exploratory models of Uranus. Adopting

$$\bar{M} = 2.33 \text{ gm/mole}, \quad g = 830 \text{ cm/sec}^2, \quad \text{we find}$$

$$\log \tau = 0.81 + 2 \log p \text{ (atm)} \quad (14)$$

The adopted value of \bar{M} is that which is appropriate in the upper portion of the atmosphere where the CH_4/H_2 mixing ratio is very small. At the cloud top, where the CH_4/H_2 mixing ratio is about 0.03, $\bar{M} = 2.7$. (See Section IV B.)

C. The Convective Lapse Rate

The presence of a large CH_4/H_2 mixing ratio in the deeper atmosphere (0.10 in the baseline model) markedly alters the convective lapse rate from the dry adiabatic lapse rate. Indeed, the wet adiabatic lapse rate will become parallel to the saturated vapor pressure curve for large abundance ratios. (See Section IV B.)

A straightforward application of the first law of thermodynamics yields the following expression for the wet convective lapse rate which is fairly accurate for temperatures less than 130°K:

$$\left(\frac{d(\log T)}{d(\log p)}\right)_{\text{conv}} = \left(\frac{R}{C_p^*}\right) \frac{1 + \frac{P_c}{p} \left(1 + \frac{B}{T}\right)}{1 + \frac{P_c}{p} \left(\frac{B}{T}\right)^2 \left(\frac{R}{C_p^*}\right)} \quad (15)$$

where p is the partial pressure of H_2 , P_c is the partial pressure of CH_4 , and $B = 1100^\circ K \approx \mathcal{L}/R$ where \mathcal{L} is the latent heat of sublimation or condensation of CH_4 . R is the Universal Gas Constant. The average heat capacity at constant pressure, C_p^* , is related to that of H_2 (C_p) and that of CH_4 ($C_p^c \approx 4R$) by approximately:

$$C_p^* = \left(\frac{p}{p + P_c}\right) C_p + 4 \left(\frac{P_c}{p + P_c}\right) \quad (16)$$

For equilibrium H_2 , C_p/R can be approximated by:

$$\frac{C_p}{R} = 3.0 + 0.5 \sin \left[\frac{\pi}{2} \frac{(T - 140^\circ K)}{100^\circ K} \right] \quad (17)$$

for $40^\circ K \leq T \leq 140^\circ K$.

The saturated vapor pressure of CH_4 can be approximated by:

$$\log P_c (\text{atm}) = 4.657 - \frac{507.3^\circ K}{T} \quad (18)$$

for $T < 90.7^\circ K$ (the triple point) and by:

$$\log P_c (\text{atm}) = 4.008 - \frac{448.4^\circ K}{T} \quad (19)$$

for $90.7^\circ K < T < 191.1^\circ K$ (the critical point).

D. A Bootstrap Approach to $F(\tau)$

The largest weakness in the approach outlined in this paper is our present inability to calculate the depth dependence of $F(\tau)$, the thermal flux, due to lack of precise understanding of the sources of opacity for the

incident solar radiation. Although it seems likely that our knowledge of the key absorption processes will soon advance to the point where some detailed calculations will be possible, a bootstrap approach is required at this time.

The approach that I have taken in these exploratory models is to assume a trial value of $F(\tau)$ and calculate a model. In general, the assumed value of $F(\tau)$ will be inconsistent with the derived model in two ways. First, $F(\tau)$ will not generally approach F_{int} at the CH_4 cloud top which is assumed to be opaque to solar insolation. Second, the mild inversion in the upper atmosphere of Uranus suggested by the observed infrared brightness temperatures at 22.5μ and 33.5μ will generally not be obtained.

The result of several iterations is shown in Figure 2-52 which shows $F(\tau)$ as a function of $\log \tau$. Figure 2-53 shows the corresponding model for $T_e = 52^\circ\text{K}$ and for $f_{\text{int}} = 0.04$. In this baseline model (characterized by $f_1 = f_2 = f_3 = 0.32$ and by $\tau_1 = 0.2$, $\tau_2 = 2$, and $\tau_3 = 20$), about half of the solar flux is deposited above $\tau = 1$. The internal flux is reached near the CH_4 cloud top where $\tau \simeq 100$ by choosing $\tau_3 = 20$. The degree of the thermal inversion is a sensitive function of τ_1 and τ_2 is chosen to make $F(\tau)$ reasonably smooth. Trial calculations indicate that the resulting model is insensitive to the exact form of $F(\tau)$ for $0.1 \leq \tau \leq 10$.

IV. THE BASELINE MODEL-RESULTS

A. The Radiative Solution

Figure 2-53 gives the partial pressure of H_2 , CH_4 , NH_3 , and H_2O as a function of temperature. It is assumed that the He/H_2 ratio = 0.2 by number. Thus, in the upper atmosphere of Uranus, the partial pressure of H_2 equals 80% of the total pressure. The exact value of the H_2/He ratio is not crucial for the exploratory models.

The particular choice of $F(\tau)$ given in Figure 2-52 leads to the mild temperature inversion indicated in Figure 2-53. The rapid decrease of $F(\tau)$ at $\tau = 1$ leads to a much lower radiative gradient than would be the case if $F(\tau)$ were constant. Indeed if $F(\tau)$ approached zero (no internal source) rather than 0.04 for $\tau > 100$, the temperature would approach a zero lapse rate of $T \simeq 100^\circ\text{K}$. However, the radiative opacity at $\tau = 100$ is sufficiently large

that a radiative gradient $\frac{d(\log p)}{d(\log T)} = 2$ results in the deeper atmosphere from the small internal flux of 4%. The magnitude of the limiting radiative gradient is a consequence of the approximate form of the mean absorption law adopted in equation (11).

B. The Convective Solution

The radiative solution described above is valid only if the radiative gradient does not exceed the adiabatic gradient. The magnitude of the latter is a sensitive function of the CH_4/H_2 mixing ratio. (See Equation 15.) As the

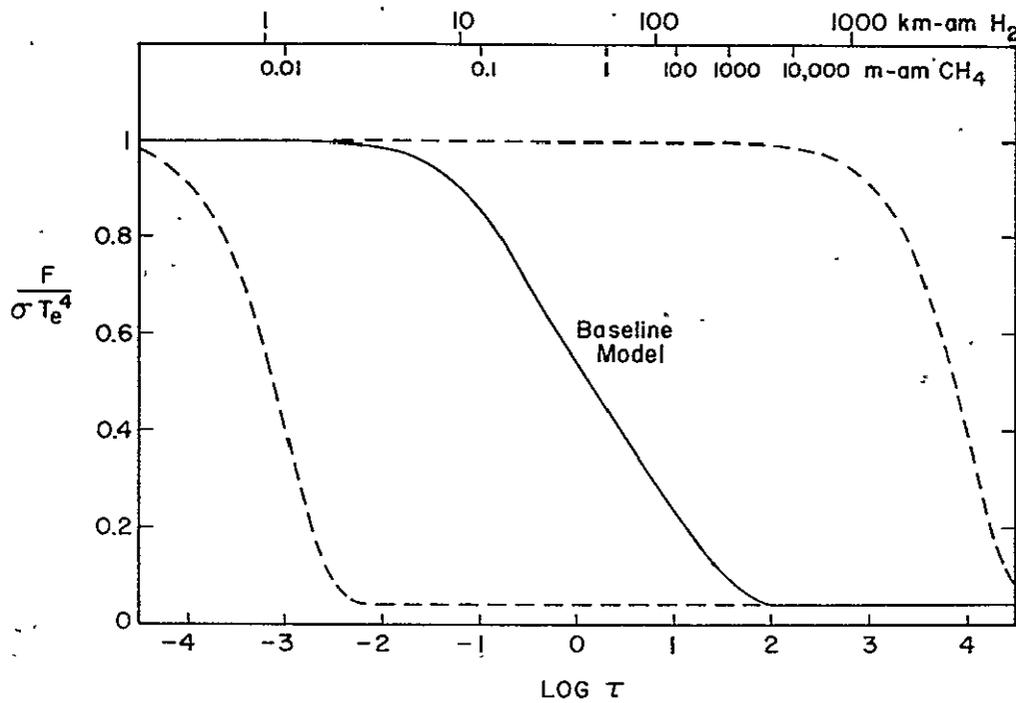


Figure 2-52. The variation of the flux $F(\tau)$ as a function of the mean grey optical depth. τ is shown for the baseline model ($f_1 = f_2 = f_3 = 0.32$; and $\tau_1 = 0.2$, $\tau_2 = 2$, $\tau_3 = 20$). The dotted curves represent extreme variations of $F(\tau)$ which would produce a gigantic thermal inversion in the upper atmosphere ($\tau_1 = 10^{-3}$; $f_1 = 0.96$) or a standard constant flux model ($\tau_1 = 10^4$; $f_1 = 0.96$).

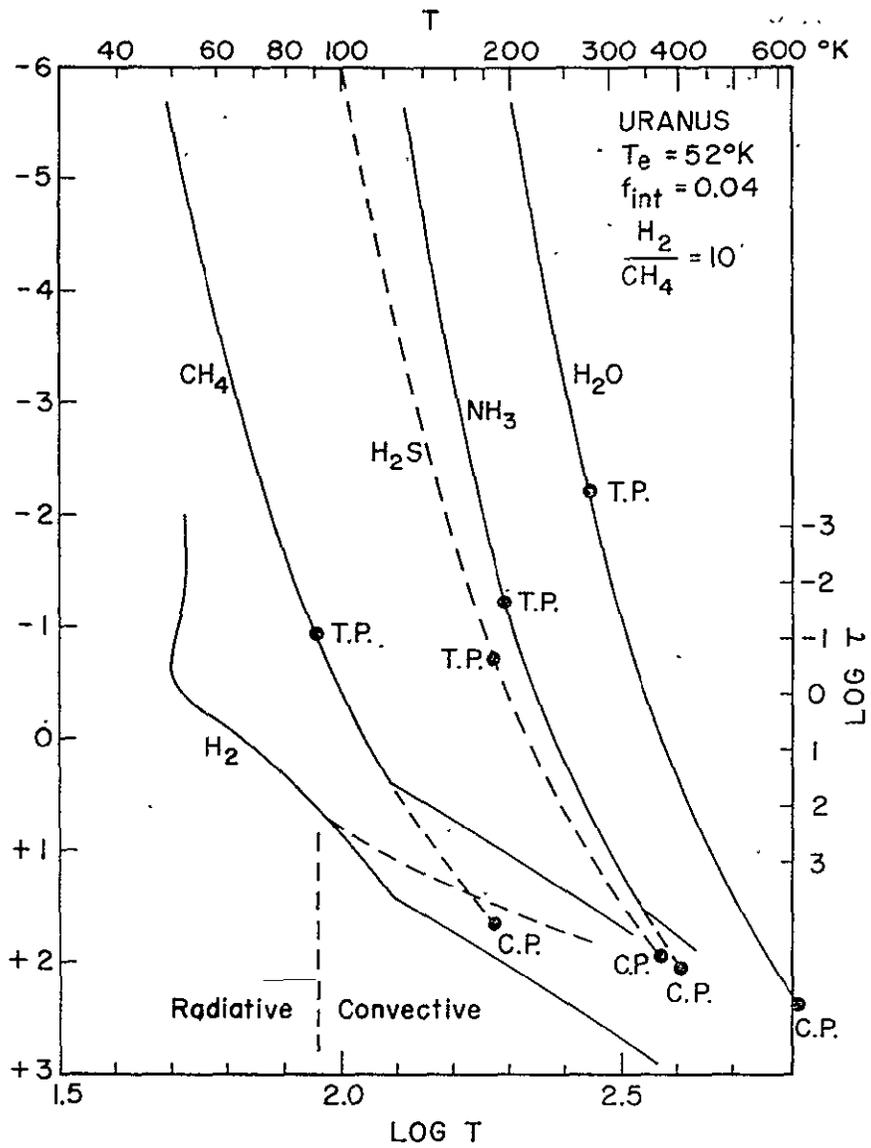


Figure 2-53. The variation of partial pressure of H₂, CH₄, NH₃, and H₂O as a function of temperature in the baseline model. The triple points (T.P.) and critical points (C.P.) are shown for CH₄, H₂S, NH₃, and H₂O.

CH₄/H₂ mixing ratio becomes large (≥ 0.1) the adiabatic gradient is so markedly reduced that it very nearly equals the value of $\frac{d(\log p)}{d(\log T)}$ calculated from the Clausius-Clapeyron equation describing the saturated vapor pressure curve of CH₄.

In Figure 2-53, the model becomes convective at $T \approx 90^\circ\text{K}$ (near the triple point of CH₄) where CH₄/H₂ ≈ 0.03 . At this point, a CH₄ droplet cloud will form. For simplicity, I have assumed that this cloud has a well defined top; it may, in fact, be somewhat hazy. The lower boundary of the cloud (which occurs at $T \approx 125^\circ\text{K}$) is determined by the adopted value of CH₄/H₂ = 0.1 in the interior of the planet. For $T \approx 125^\circ\text{K}$, the dry convective gradient is shown. Since the atmosphere becomes convective at very large optical depths, the upper radiative portion of the atmosphere is virtually unaffected by the onset of convection in the lower atmosphere.

C. The Observable Portion of the Atmosphere of Uranus

The above baseline model leads to a rather definite semi-quantitative picture of the atmosphere of Uranus. Referring to Figures 2-49 and 2-53, the atmosphere is stable to depths where the partial pressure of H₂ is about 4 atm; the corresponding H₂ abundance above the CH₄ cloud is about 400 km am. The CH₄ abundance is a very sensitive function of the atmosphere of Uranus. If CH₄ is saturated above the cloud; its abundance is of the order of 5 km am. If it is not fully saturated, the abundance will be less. In any case, most of the observable CH₄ is located deep in the atmosphere near to the CH₄ cloud where the total pressure is about 4 atmospheres. Under these conditions, very weak bands of CH₄ will be strongly pressure broadened, thereby helping to produce the large features observed at visible wavelengths. The probability of simultaneous transitions in H₂ + CH₄ is also enhanced under these conditions.

A feature of this model is that it predicts that the upper atmosphere of Uranus is very clear of cloud particles. If the atmosphere were not clear the centers of strong bands such as the 6190, 7250 and 8900 Å bands would not be nearly as dark as they are observed to be. Indeed, as shown in Figure 2-54, the albedo in the central portions of these bands is approximately equal to that due to Rayleigh scattering of 100 km am H₂. A small amount of cloud particles in the upper atmosphere would greatly increase the observed brightness of the band centers since the CH₄ abundance in upper atmosphere is so low.

It is interesting to note that the brightness at the center of the 4860 and 5430 Å visible bands is comparable with Rayleigh scattering of 300 km am H₂. This is understandable in terms of these very weak bands being formed only at depths near to the cloud top where the bulk of the observable gaseous CH₄ is located.

Because no continuum photons penetrate to depths having temperatures in excess of 90°K, the predicted NH₃ abundance is of the order of 10⁻⁵ cm-am. This is many orders smaller than the upper limit for NH₃ of 2.5 m-am given by Encrenaz, Hardorp, Owen, and Woodman (1974).

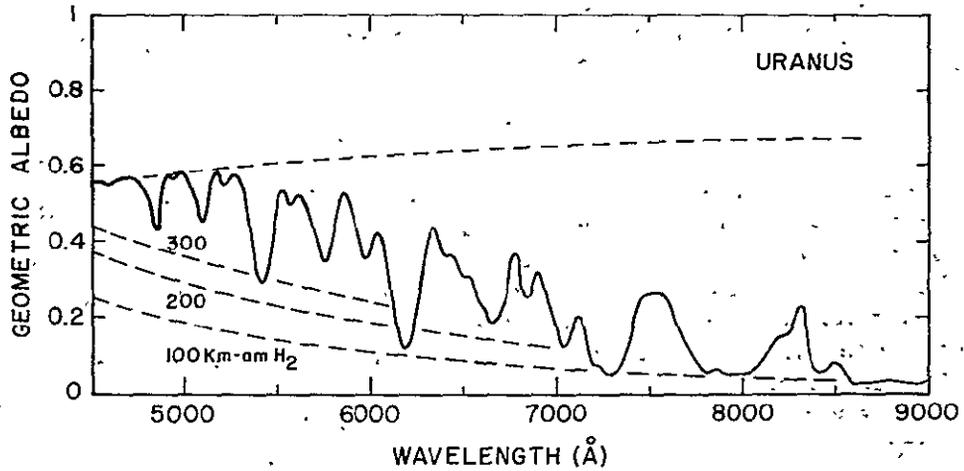


Figure 2-54. The geometric albedo of Uranus based largely on Younkin's (1970) spectrophotometry. The upper dashed curve is a conjecture as to the geometric albedo in the absence of molecular absorption. Also shown is the geometric albedo due only to Rayleigh scattering (Rayleigh phase function) of 100, 200, and 300 km-am of H₂.

Figure 2-55 shows the predicted thermal emission calculated from the baseline model shown in Figure 2-53. The calculations were performed by Dr. John J. Caldwell, whose thermal emission computer program contains a recent version of Dr. L. Trafton's opacity model for a mixture of H₂ and He. Comparison of the predicted thermal spectrum for T_e = 52°K with the observational data (summarized in Table 2-8) indicates that the predicted flux near 33.5μ is nearly a factor of two larger than observed.

It is possible to obtain a better agreement by adjusting F(τ) slightly to maintain the same boundary temperature and by reducing T_e from 52°K to 48°K. As shown in Figure 2-55, the agreement between the predicted and observed spectrum is very satisfactory for this very cold model. Figure 2-56 shows the relevant portion of the partial pressure curve of H₂ superimposed on the baseline model.

V. COMPARISON WITH NEPTUNE

The spectrum of Neptune is very similar to that of Uranus in visible and near infrared wavelengths (Wamsteker 1973). The observed equivalent widths of the H₂ quadrupole lines in the 4-0 band are nearly the same for Uranus and Neptune (Trafton 1974). Considering the fact that Neptune receives only 40% as much solar insolation as Uranus, this striking similarity is probably not accidental. I propose that it is a consequence of Neptune being similar to Uranus in having a small internal energy flux and a large CH₄/H₂ ratio. In this case, one could repeat in detail the type of analysis presented above for Uranus.

I have done this to a very limited degree by adopting an effective temperature T_e = 48°K for Neptune and by adjusting F(τ) so that the resulting model is consistent with the available thermal emission measurements. (See Table 2-9.) The resulting model (partial pressure of H₂ as a function of temperature) is shown in Figure 2-57 and the corresponding thermal emission spectrum is shown in Figure 2-58 along with the available observations. Figure 2-57 shows that a rather large temperature inversion is required on Neptune to simultaneously explain the high brightness temperature near 22.5μ (≈58°K) and the 53°K upper limit near 34μ. It appears that such an inversion provides an alternate explanation for the large internal source postulated by Murphy and Trafton (1974) to explain the high brightness temperature measured near 24μ, as also suggested by Wallace in these proceedings.

Strong independent evidence for a thermal inversion in the atmosphere of Neptune has recently been obtained by Gillett (1975 - private communication) who finds strong emission features near 8μ and 12μ. The observed intensity near 8μ suggests that the temperature in the inversion is greater than 100°K. (The boundary temperature in the model shown in Figure 2-57 is about 80°K.) Analysis of the occultation of BD-17°4388 by Neptune (Rages, Veverka, Wasserman, and Freeman 1974) also indicates the presence of a large thermal inversion in the upper atmosphere of Neptune.

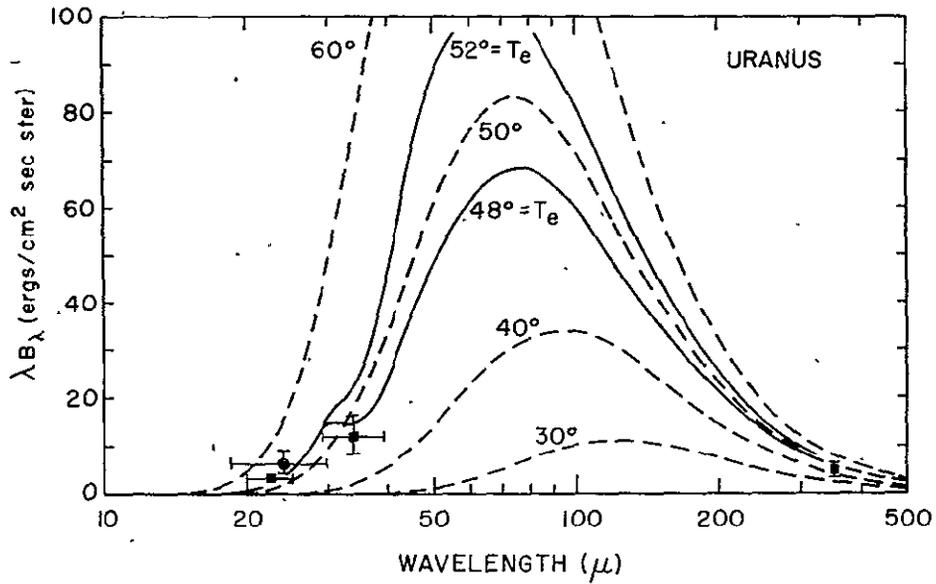


Figure 2-55. The predicted thermal emission spectrum of Uranus as calculated from the baseline model of Figure 2-53 is shown ($T_e = 52^\circ \text{ K}$). Also shown is the predicted thermal emission spectrum from the colder model ($T_e = 48^\circ \text{ K}$) shown in Figure 2-56.

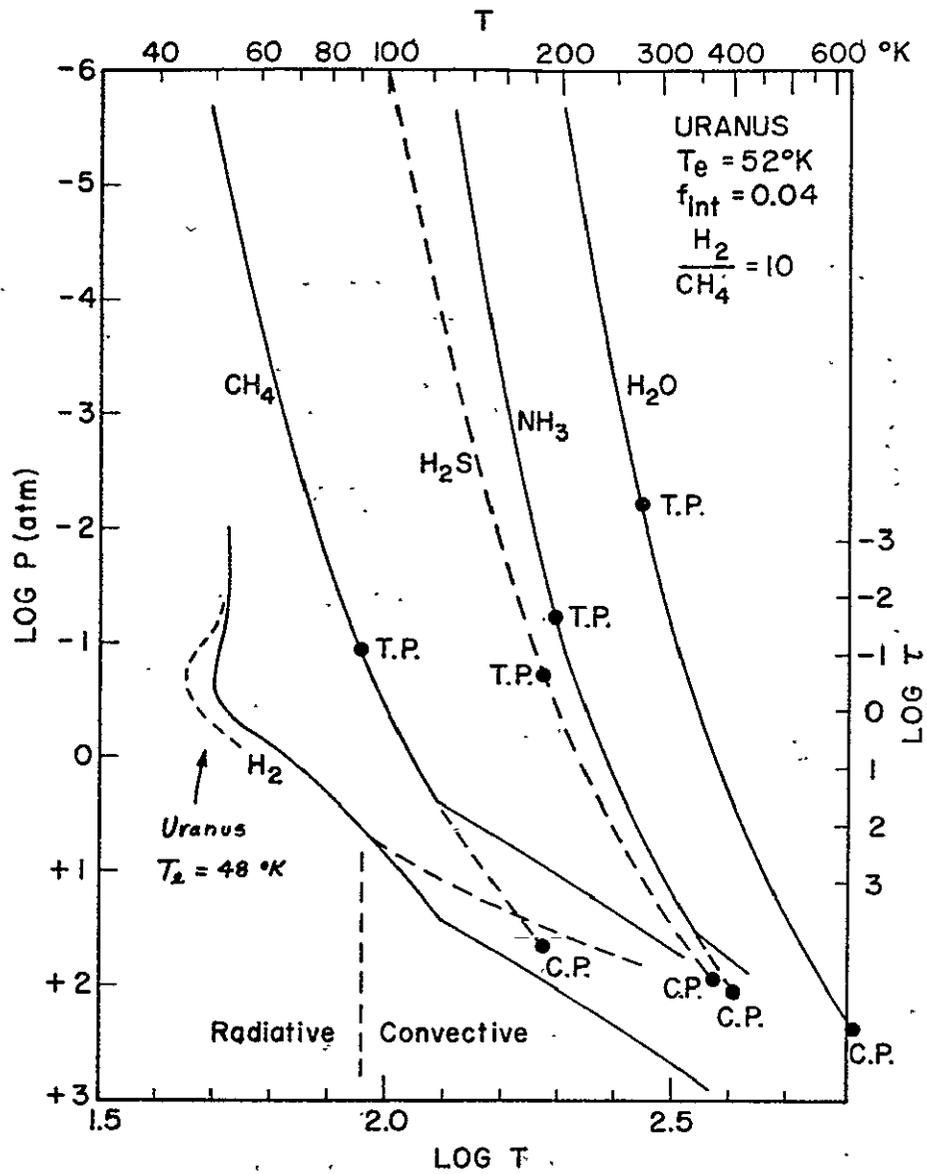


Figure 2-56. The variation of the partial pressure of H₂ for a Uranus model characterized by $T_e = 48^\circ\text{K}$ and by $f_1 = f_2 = f_3 = 0.32$; $\tau_1 = 0.1$, $\tau_2 = 1$, $\tau_3 = 10$.

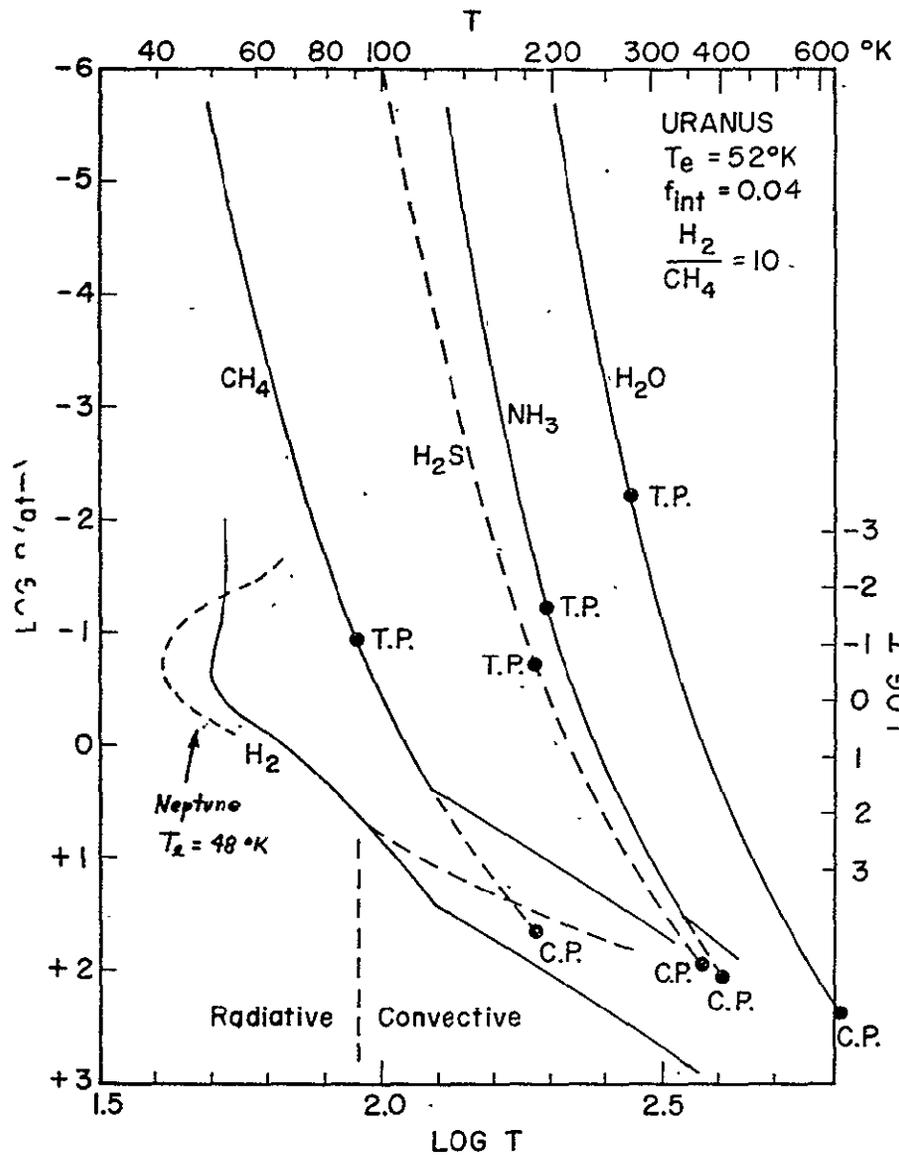


Figure 2-57. The variation of the partial pressure of H_2 for a Neptune model characterized by $T_e = 48^\circ\text{K}$ and by $f_1 = f_2 = f_3 = f_4 = f_5 = 0.192$; and $\tau_1 = 0.01$, $\tau_2 = 0.1$, $\tau_3 = 1$, $\tau_4 = 10$, $\tau_5 = 100$.

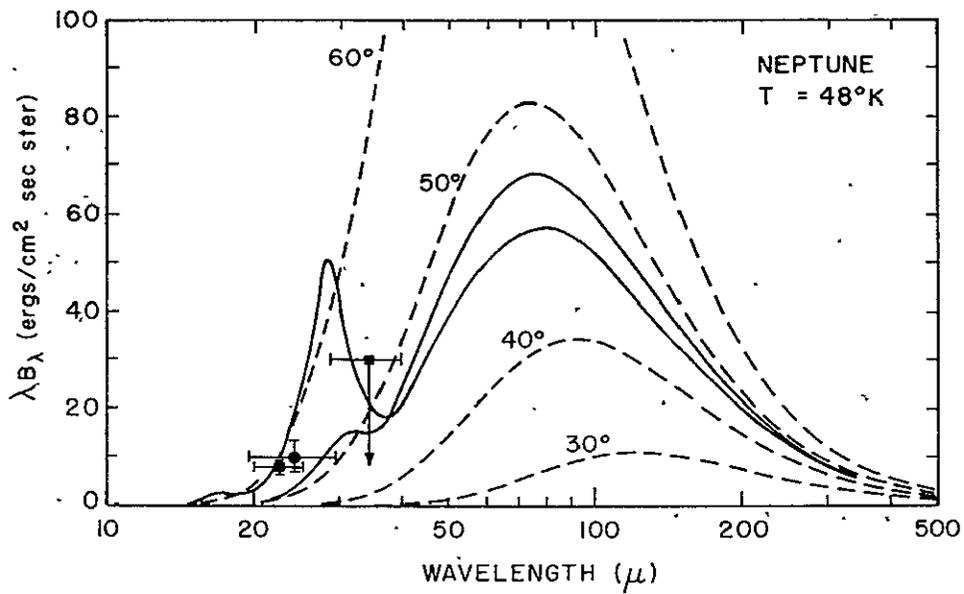


Figure 2-58. The predicted thermal emission spectrum of Neptune calculated from the model shown in Figure 2-57. This figure also repeats the predicted Uranus emission spectrum corresponding to the model of Figure 2-56.

VI. DISCUSSION

This working paper is an attempt to produce a self-consistent model of the atmosphere of Uranus which will serve as a basis for designing the observational and theoretical investigations needed to establish the fundamental characteristics of this most fascinating planet. Several key observations are suggested by the models. One of the most important is to determine the line of sight CH_4 abundance in the Uranus atmosphere. One of the best ways to accomplish this is to use a very weak CH_4 band whose quantum structure and strength is known. The 6840\AA band employed by Belton and Hayes (1975) is generally assumed to be the $5\nu_3$ band of CH_4 , but this identification is not completely certain. In any event, the total band strength is very uncertain; a determination of the true strength of this band is of great importance.

An alternate method of determining the line-of-sight abundance of CH_4 would be possible if some of the visible features in Uranus are due to simultaneous transitions in $\text{CH}_4 + \text{H}_2$. The best candidate for a simultaneous transition in the Uranus spectrum is the 5570\AA feature which coincides very closely with predicted position of the simultaneous transition of the 7250\AA CH_4 band and Q branch of the 1-0 transition in H_2 (Danielson 1974). The strengths of the simultaneous transitions in $\text{CH}_4 + \text{H}_2$ are being determined by Welsh (1975 - private communication).

It is also important to measure the widths of very weak CH_4 lines with sufficient spectral resolution that the total pressure can be obtained. Since the bulk of the CH_4 lines are formed close to the CH_4 cloud in the model presented in this paper, the interpretation of the observations would be straightforward.

Narrow band measurements ($\Delta\lambda \simeq 2\mu$) of the thermal emission from Uranus and Neptune in the 20μ and 33μ windows would be very useful in firming up the thermal structure in the upper atmosphere (above $\tau = 1$). The band passes of the available measurements are sufficiently wide that they cannot be unambiguously compared with theoretical predictions (as in Figures 2-55 and 2-58). A measurement of the brightness temperature of Uranus near 100μ using the NASA Infrared Airborne Observatory is obviously of crucial importance in establishing the effective temperature of the visible disk of Uranus at this epoch.

A weakness of the type of model of Uranus presented in this paper is that it predicts a radio brightness spectrum which is colder than observed. This is illustrated in Figure 2-59 which shows the predicted emission spectrum of a model of Uranus which is very similar to that in Figure 2-53 except that $T_c = 60^\circ\text{K}$. I am indebted to Dr. S. Gulkis for calculating Figure 2-59.

The discrepancy between theoretical and observed radio brightness temperatures of Uranus is characteristic of all models in which the partial pressure of NH_3 is given by the Clausius-Clapeyron equation for NH_3 (Gulkis, These Proceedings). It appears as if NH_3 is nearly quantitatively absent in the Uranus atmosphere to substantial depths ($T > 300^\circ\text{K}$); saturated NH_3 provides too much opacity at radio wavelengths. No model of Uranus can be

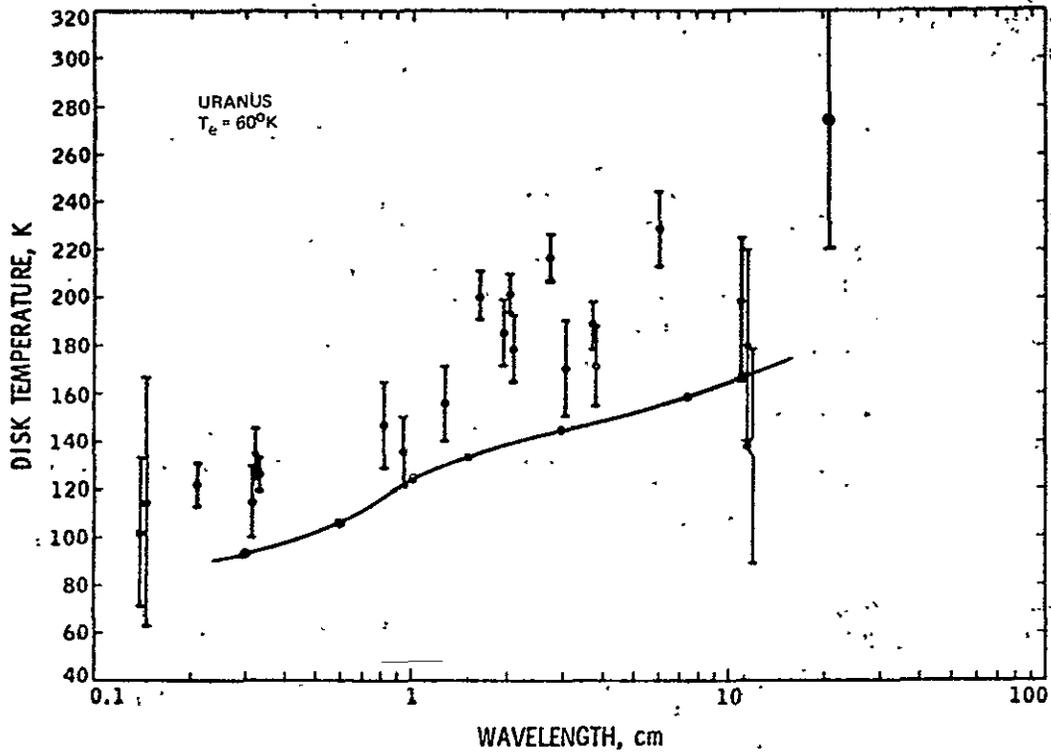
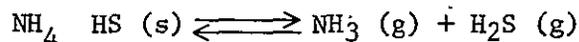


Figure 2-59. The predicted radio brightness spectrum of Uranus based on a model similar to the baseline model of Figure 2-53 but with $T_e = 60^\circ\text{K}$ and with $f_1 = f_2 = f_3 = 0.32$; and $\tau_1 = 0.1$, $\tau_2 = 1$, $\tau_3 = 10$.

considered satisfactory until this puzzling behavior is explained. If the H₂S abundance exceeded the NH₃ abundance in Uranus, the consequent formation of NH₄ HS would reduce the NH₃ abundance by large factors (Lewis 1969). However, this reversal of relative solar and cosmic abundances seems sufficiently implausible to warrant a hard search for a non-cosmogonic explanation.

In that spirit, the following highly conjectural suggestion is made: Figure 2-53 indicates that NH₃ is saturated nearly to its critical point (406°K, 112 atm). The Clausius-Clapeyron equation for H₂S (shown in Figure 2-53) predicts larger vapor pressures for H₂S than for NH₃ for all temperatures less than the critical point of H₂S (373°K, 89 atm). In the event that the NH₃ to H₂ mixing ratio deep in the Uranus atmosphere is somewhat larger than adopted in this paper (0.05), NH₃ would be saturated to its critical point (as is H₂O!). Under these circumstances, it seems conceivable that the complexities of a convecting mixture of NH₃ and H₂S, both near their critical points, may have the net result of reversing the usual ratio of solar abundances (NH₃/H₂S ≈ 5) at depths in the atmosphere above the critical points. In that event, the equilibrium reaction



would deplete NH₃ in the upper atmosphere by several orders of magnitude below that given by the Clausius-Clapeyron equation. In any case, the amount of H₂S in the atmosphere of Uranus above the level where T = 90° is too small to be detected spectroscopically. I believe that further investigation of this conjecture is warranted.

This work is supported by NSF grant GP-39055 and NASA grant NSG-7054.

Table 2-7. The Geometric Albedo (p), The Phase Integral (q), And The Bond Albedo (A) For A Purely Absorbing Gas Over A Lambert Surface

p	q	A
0.67	1.5	1
0.5	1.39	0.69
0.4	1.32	0.53
0.3	1.25	0.38
0.2	1.17	0.23
0.1	1.05	0.11
0.01	0.80	0.01

Table 2-8. Infrared Measurements of Uranus

λ	$\Delta\lambda$	T_B	Reference
22.5 μ	5 μ	53.4 \pm 1.1 $^{\circ}$ K	Rieke and Low (1974)
24	11	54.7 \pm 1.8	Morrison and Cruikshank (1973)
33.5	11	47.8 \pm 1.8	Rieke and Low (1974)
350	\sim 50	43 \pm 8	Harper, Low, Rieke and Armstrong (1972)

Table 2-9. Infrared Measurements of Neptune

λ	$\Delta\lambda$	T_B	Reference
22.5 μ	5 μ	58.0 \pm 1.1 $^{\circ}$ K	Rieke and Low (1974)
24	11	57.2 \pm 1.6	Morrison and Cruikshank (1973)
34	11	< 53	Rieke and Low (1974)

2.8 GENERAL DISCUSSION

All Participants

2.8.1 Occultation

DR. JOHNSON: In March of 1977 Uranus will occult the star SAO 158687 (G.E. Taylor, 1973). The relatively faint visual magnitude of this star (about 3-3.5 stellar magnitudes fainter than Uranus) makes ordinary visual wavelength observations difficult or impossible. However, the strong atmospheric absorptions in Uranus' near infrared spectra will allow very useful photoelectric measurements of the occultation to be made, providing modern red sensitive phototubes and adequate electronics are used. The following pages containing brief communications from R. Carlson (USC) and B.A. Smith (U. of Arizona) give some details of the circumstances of the occultation and preliminary spectrophotometric data for Uranus and SAO 158687, confirming the feasibility of obtaining useful photoelectric observations of this event at wavelengths of about 9000 Å.

DR. HUNTEN: The following resolution is made a part of these Proceedings:

"It is the opinion of this group that the 1977 occultation by Uranus will be an important event, and we direct to it the attention of observers and funding agencies."

Stellar Occultation by Uranus

R. W. Carlson March 6, 1975

Ref: G. E. Taylor, J. British Astron. Assoc. 83, 352 (1973).

Date: 10 March 1977

Time of Midoccultation: 21^{h} UT $\pm 15^{\text{m}}$

Maximum Duration: $\approx 1^{\text{h}}15^{\text{m}}$

Shadow Velocity: 11.7 km/sec

Projection on Disc: Shown in Figure 2-60

Area of Visibility: Portions of Africa, Asia, Australia, and the Antarctic

Altitude: Shown in Figure 2-61 for midoccultation. For example, at Crimea, disappearance will occur near the horizon, while reappearance will occur at an elevation of $\sim 14^{\circ}$. For the Japanese stations disappearance will occur at elevations of $\sim 30^{\circ}$ and solar depression angles of $7-13^{\circ}$, but reappearance will occur after sunrise.

Position of Sun: 128° W of Uranus

Position of Moon: 17° W of Uranus, Age 21^{d}

Star = SAO 158687

Sp = K5 IV (?)

m_v = ≈ 9.5 (Measurements by R. Carlson and R. Brinkmann)

α_{1950} = $14^{\text{h}}35^{\text{m}}27^{\text{s}}$

δ_{1950} = $14^{\circ}44'20''$

Angular Radius: $\approx 0''.00024$ (3 km projected radius at Uranus)

NOTES: With the exception of Pluto, the radius and oblateness of Uranus is the most poorly known of all the planets. The forthcoming occultation provides a unique opportunity to determine these parameters and to investigate the atmosphere of Uranus. Such an opportunity may not occur again for centuries.

Observations with conventional S-1 and S-20 photocathodes would be facilitated by observing in the methane bands at 7250\AA and 7900\AA .

In oblateness and radii determinations it is important to obtain measurements with as much north-south separation as possible. The relative

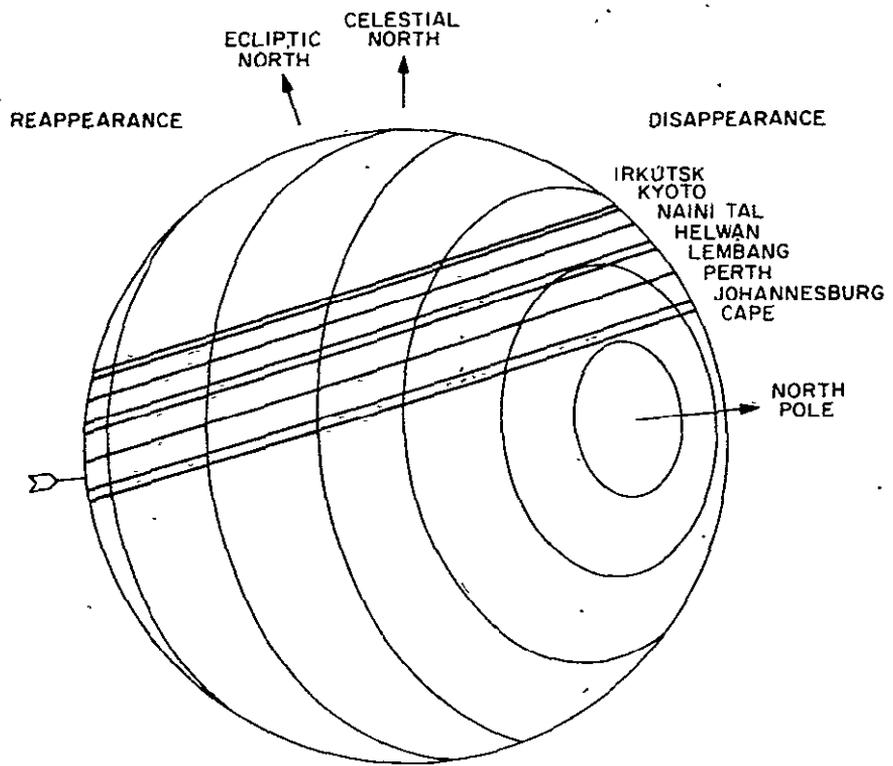


Figure 2-60. Stellar occultation by Uranus -- projection on disc.

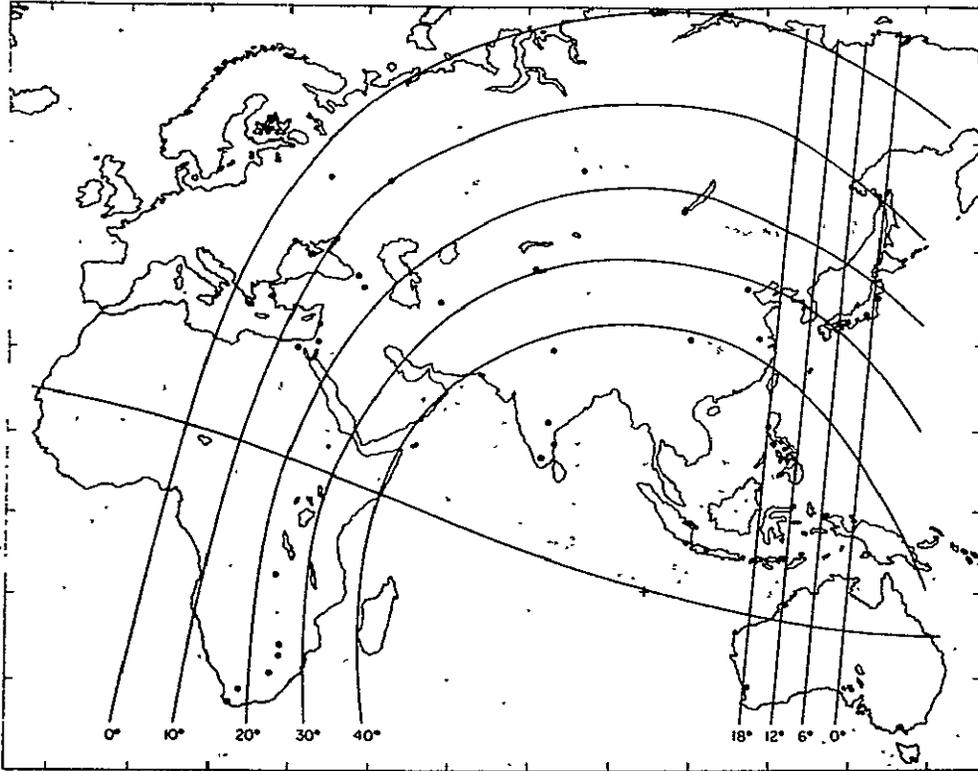


Figure 2-61. Stellar occultation by Uranus -- altitude for midoccultation

timing accuracy should be 1 sec or better. Absolute times are very desirable, particularly if only ingress or egress is observed as for stations near the terminator or in the event of clouds or equipment malfunction.

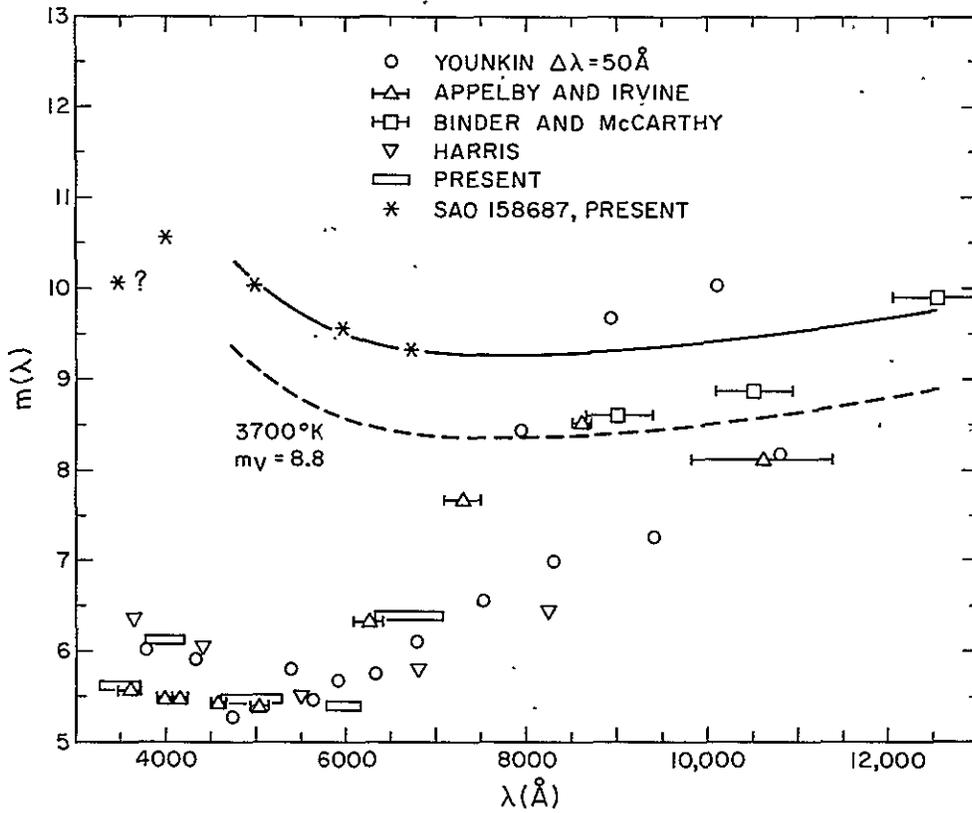


Figure 2-62. Stellar occultation by Uranus -- observation of methane bands at 7250Å and 7900Å

Uranus/SAO 158687 (12 February 1975)

Photoelectric observations of Uranus and SAO 158687 by G. Coyne (Figure 2-63) and spectrophotometry by W. Wisniewski indicate that the two objects will be of comparable brightness at 900 nm. At longer wavelengths the star will become increasingly brighter than Uranus.

With a relative velocity of $11\frac{1}{2}$ km/sec, the star will sweep through 1 scale height in the upper atmosphere of Uranus in about 5 seconds.

B. A. Smith
 The University of Arizona
 Department of Planetary Science
 Lunar and Planetary Laboratory

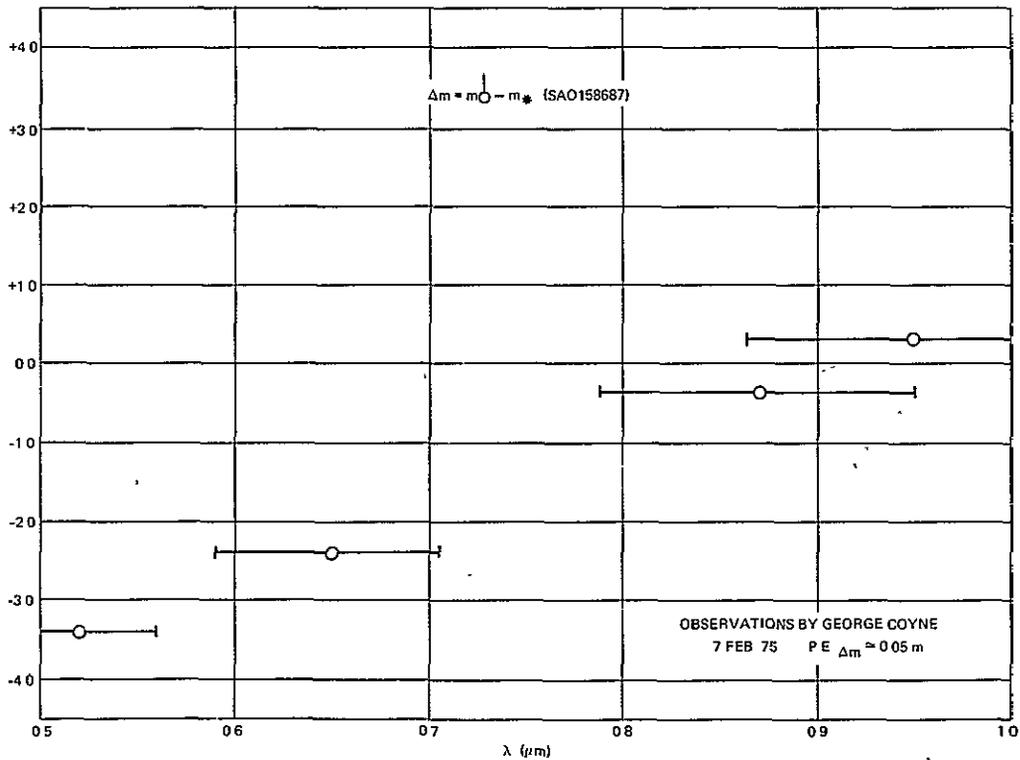


Figure 2-63. Photoelectric observations of Uranus and SAO 158687.

2.8.2 Dynamics of the Atmosphere

DR. STONE: I suppose the question of greatest interest is how it might affect the structure that the probe will see.

DR. POLLACK: No, I think that is one question, but I think just the dynamics in themselves is a very interesting thing. For example, in the case of the Venus probe, the Pioneer Venus probe, there are several experiments that are just dedicated to trying to learn something about the dynamics themselves on Venus, so I think it would be very interesting to get some general feelings on the dynamical regimes you would expect in the case of Uranus and whatever is of interest in that field now.

DR. TRAFTON: In particular, as Uranus points its pole toward the Sun more and more, approaching at 1984, how might the dynamics change?

DR. STONE: Let me say a few qualitative things and then you can ask more specific questions if you want, or look at my forthcoming paper in Icarus.

The first thing to look at, I think, in trying to assess what might happen there are the external boundary conditions. And there are several here that are important. One is the strength of the heating of the atmosphere. That, in principle, can come from two sources, the Sun or any internal source. If you compare these for Uranus with the same quantities for the other planets, you have a much more weakly heated atmosphere. It is further from the Sun, and as best we can tell there is, at most, a weak internal heat source.

Secondly, you can look at the basic solar time constant, which is a measure of how rapidly the atmosphere would relax radiatively. And you find that, because of the low temperatures, you have a very long time scale on the order of 600 or 700 years, depending on the level you are looking at in the atmosphere, but centuries, not decades. And this means that the external heating is going to be less efficient at driving motions or at destabilizing the atmosphere than for the other atmospheres nearer to us that we will be exploring with probes. And this indicates, just qualitatively, without doing any calculations, that it is going to be a much more stable atmosphere, dynamically less active.

When you try to make quantitative statements, I think you can say something. You must have a theory for the effect of the dynamics on the structure. During the last five years there has been quite a bit of work done on this oriented towards the Earth mainly, that by now is pretty well founded both theoretically and observationally. It seems to work pretty well for Mars as well as for the Earth (e.g. Kliore et al., 1973, J.G.R., 78, 4331).

When you go to the outer solar system using the same methods for calculating these things, it becomes more speculative. But if you examine the theoretical basis, the kinds of differences you would expect applying these same techniques, to the outer planets are such that you would have a good

chance of getting upper bounds on the kinds of regimes that will occur. If you apply these methods to Uranus, you come out with something that agrees, bearing in mind the qualitative statement that I made, in predicting a much more stable atmosphere than for the planets closer in.

This doesn't make it an uninteresting atmosphere. It is perhaps the first one we will be able to explore which is more stable than the Earth's. Mars and Jupiter both appear to be dynamically less stable than the Earth's, based on what we know about them now. So that, to me, makes Uranus a fascinating place to look at from the point of view of learning something about the general behavior of atmospheres.

DR. POLLACK: I just want to be sure I understand what you are talking about by "more stable". Say in the case of the Earth, vertical transport of heat by these large scale eddies lead to a more shallow lapse rate than you would get, let's say, with convective instability; are you saying that in the case of Uranus that that vertical heat transport is even a bigger factor?

DR. STONE: Only in a relative sense. I am not saying that the lapse rates are going to be strongly subadiabatic. If you put in these methods of calculating things, it would be hard to get a lapse rate that is more than maybe 2/10 of a degree per kilometer subadiabatic. But in terms of dynamical stability criteria, in terms of dimensionless parameters like the Richardson number, you get huge values, much larger than for the Earth, which implies that the turbulence is very weak, the time scales will be very long relatively speaking, and the spatial scales will be larger relatively. So this would be a completely different type of atmosphere from any other that we have had a chance to explore. I do not include Venus here, because I am really talking only about rotating atmospheres. Venus is quite different and, interesting as it may be, it is not like Uranus simply because of the difference of rotation rates. So Uranus is still unique.

I might mention something else, too, that this very long relaxation time in the atmosphere implies that you will not get strong seasonal effects because of the peculiar orientation of the axis. The revolution period around the Sun is 84 years, while the relaxation time is 600 or 700 years and you just don't have time to respond to that change of the orientation of the axis relative to the Sun.

DR. HUNTEN: That is true for the main atmosphere, troposphere, so to speak. However, the kind of thing Lloyd Wallace was talking about this morning would change quite drastically.

DR. STONE: Yes, when you get high enough that the local absorption dominates, although even there the seasonal effect will be modified by an ocean-type effect. The radiation coming from below will not have much of a seasonal effect, and that will moderate what happens higher up.

DR. POLLACK: What kind of dynamical regime do you expect on Uranus? What about a banded structure like Jupiter?

DR. STONE: I have to make this hedge again, that applying the same sorts of theories to a planet like Uranus is more likely to give you an upper bound on things like static stability and, therefore, it is difficult to make a definite statement.

In the case of Jupiter, interestingly enough, if you use these same methods of calculation and if you believe you are actually calculating what happens, not just an upper bound, you get the result that in high latitudes you would expect a baroclinic eddy-type regime analogous to what you get on Mars and Earth, but not in low latitudes. The Pioneer 10 pictures I have seen do actually suggest that kind of division. So maybe you can take this as being a stronger statement than merely an upper bound; and if you do that for Uranus, it definitely says a weakly turbulent baroclinic instability regime with asymmetric eddies.

By the way, I was interested in that picture that Mike Belton showed with an apparent band. If I saw the band that Mike was referring to, and it is like the bands that the old observers reported, they are not parallel to the equator.

DR. BELTON: The coordinates on the picture are those of the telescope. I think the band is roughly in the right direction, parallel to the equator.

DR. JOHNSON: The angle the old observers claimed to see, though, was 10° to 15° .

DR. STONE: Another interesting thing you can do using these same methods is calculate what sort of scale these eddies would have on Uranus, and it comes out on the order of 1,000 kilometers, which we can't resolve from the Earth, so it is not surprising you don't see much, if that calculation is correct. Or you may occasionally see something like that which may really be just your eye bringing together a lot of small scale stuff that you can't resolve.

DR. GULKIS: What about the gross features, like equatorial jets. Can you predict that on Jupiter?

DR. STONE: We don't even understand it on Jupiter, so --

DR. GULKIS: So, really all of the coarse structure you still don't understand.

DR. STONE: The equatorial jet on Jupiter is a low latitude phenomenon, and all I can say at this moment is that I expect something quite different from an Earth-type regime in low latitudes, and that is what we see. Why it is what we particularly see is speculative.

There are a couple of things about the heating that I should also mention. One is, those results I was just quoting for Uranus are based on the assumption of no internal heat source and, of course, as we saw earlier today, that is very hard to say with any definiteness at this point. We need measurements of the infrared emissions at different wavelengths.

DR. HUNTEN: How big does the heat source need to be to be significant for Uranus?

DR. STONE: I haven't done that calculation. I would have to have a number and then sit down and work it out.

But it certainly is something that is crucial in making these conclusions, what the strength of that heat source is.

The other thing is that with this odd orientation of the axis, when you average over one revolution around the Sun, you get more heat coming in at the poles than at the equator; which would reverse the temperature gradients from the normal, a hot pole and a cold equator; and that will tend to reverse the direction of flow. Instead of it coming from the West, it should be coming from the East and the curvature effects will interact with this flow in a different way than they do on the Earth, or Jupiter, or Mars.

And that should be an interesting thing to study, the latitudinal effects in particular, how things vary latitudinally.

2.9 IMPACT OF PLANETARY ATMOSPHERE CHARACTERISTICS ON PROBE DESIGN

Nick S. Vojvodich, Advanced Space Projects Office, NASA-Ames

MR. NICK S. VOJVODICH: To give you a feel for some of the important probe engineering parameters and related atmospheric characteristics, I have abstracted some material and have a brief overview of the salient points to present here.

Figure 2-64 presents some of the atmospheric properties we would like to know when we are designing a probe. Two of the critical parameters are stability and drag coefficient characteristics. The symbol, M_i , represents the mass fraction of the particular atmospheric component we are interested in and, summing those up, we can calculate the specific heat of the mixture, which turns out to be a primary variable.

I have summarized on this Figure 2-64 the probe performance characteristics, what property in the atmosphere we are interested in -- in terms of what its primary functional relationship -- and the altitude/pressure region of prime interest. We would like to know the specific heat almost through the entire pressure range, from 10^{-3} down to about ten bars because it influences not only the high speed deceleration characteristics but also the subsonic descent and, therefore, the rate at which the instruments can sample.

The structural design, the heating environment, and the heat-shield response are all critical because if these components don't function satisfactorily, we don't have any probe or experiments to work after we have encountered heating.

I indicated a nominal pressure level of about 2×10^{-3} atmospheres, essentially upper atmosphere. So if the density scale height (we can approximate it by an exponential) is given, and the species concentration is established (because the chemistry in the shock layer and, indeed, the response to the heat shield is very strongly influenced by the chemical constituents that exist in the heated shock layer), we have the required atmospheric definition to define the heating and deceleration. But that is adequate only in the high altitude region.

As the probe descends into the lower regions of the atmosphere, where it is traveling at subsonic speeds and where instruments are deployed to sample the constituents, the instrument performance can be strongly influenced by the makeup of the clouds (i.e., if there are any aerosols, or any sulfuric acid like we are facing with the Pioneer Venus probe design). Therefore, first order definition of the likely cloud constituents are required, particularly for the design of instrument interfaces such as windows and inlets.

The descent rate is obviously important for the spatial resolution capability of the probe instrument complement.

PROBE PERFORMANCE	ATMOSPHERIC PROPERTY	REGION OF INTEREST
• AERODYNAMIC STABILITY AND DRAG COEFFICIENT (C_D)	$\gamma = f(M_I)$	$10^{-3} - 10$ ATM
• STRUCTURAL DESIGN, ENTRY G'S	H_S	} 10^{-3}
• HEATING ENVIRONMENT	H_S, M_I	
• HEAT-SHIELD RESPONSE - REQUIREMENTS	M_I	
• DESCENT THERMAL CONTROL	T VS. H	} $10^{-2} - 10$
• DESCENT RATE = (SPATIAL RESOLUTION)	H, $\gamma = f(M_I)$	
• INSTRUMENT PERFORMANCE	M_I	} $10^{-3} - 10$
• COMMUNICATION - SIGNAL ATTENUATION	M_I, T VS. H	
	+ ELECTRON DENSITY IN IONOSPHERE	

H_S = SCALE HEIGHT: $\rho/\rho_{SL} = e^{-H/H_S}$; M_I = SPECIE MASS FRACTION; T = TEMPERATURE

Figure 2-64. Impact of planetary atmosphere characteristics on probe design.

Given successful entry and subsequent adequate performance of the instruments, we still have to be able to communicate from the probe to the spacecraft bus, which will serve as a relay link back to the Earth.

I was talking to Terry Grant regarding communications when we were preparing this presentation and he said that he would like to know the temperature versus altitude and the molecular composition as a function of altitude in addition to the electron density in the ionosphere because, as I understand it, communications margin basically involves a profile inversion. Therefore, the altitude dependence of all these parameters must be known in order to come up with a signal attenuation coefficient. Accordingly, these parameters should be defined throughout the entire pressure spectrum.

DR. OWEN: Could I ask you to be a little more specific about this? In other words, how well do you need to know these things; for example, a hydrogen-helium ratio.

MR. VOJVODICH: I have a couple of charts which will illustrate the dependence of atmospheric structure and composition on model choice. In other words, our current state of understanding defines a pretty broad range.

Figure 2-65 indicates the current atmospheric uncertainties for Saturn and Uranus. On this figure, the model atmospheres are shown for the probe terminal descent region, and it illustrates a wide deviation in both temperature and pressure from the nominal. These extremes were incorporated during our paper studies, and accordingly we were faced with some penalties in trying to design some commonality into this probe for both Saturn and Uranus exploration. Of course, the driving consideration was the cost factor. We felt that if we could incur the basic probe and instrument development costs only once we could do the combination exploration of Saturn, Uranus and (with slight modifications) Jupiter at minimum cost and with a high degree of technical compatibility by using weight margins to insure an adequate factor of safety.

Plot b of Figure 2-65 shows atmospheric pressure from one-tenth of a bar down to 10 bars, and I guess every one is familiar with why this was called a 10-bar probe: because as shown all of the clouds for the planets of interest are estimated to be above the 10-bar altitude. Plot a of the figure shows the cool, nominal, and warm atmospheric models for both Saturn and Uranus. The Jupiter models (NASA SP-8069) exhibit a similar degree of uncertainty in both the structure and composition.

These representations are from the NASA SP monograph documents NASA SP-8103 and 8091, and they present the range in uncertainty. For example, in Uranus, which is the solid line on the figure, at a pressure of one bar, the cool atmosphere is down around 75° Kelvin, and the warm atmosphere is on the order of 140° Kelvin. There is a similar spread for the Saturn model, which is shown as the dashed lines.

Table 2-10 shows the compositions that are associated with these models. And, as you will see, this composition uncertainty turned out to be a real driver in the design of the probe entry thermal protection system.

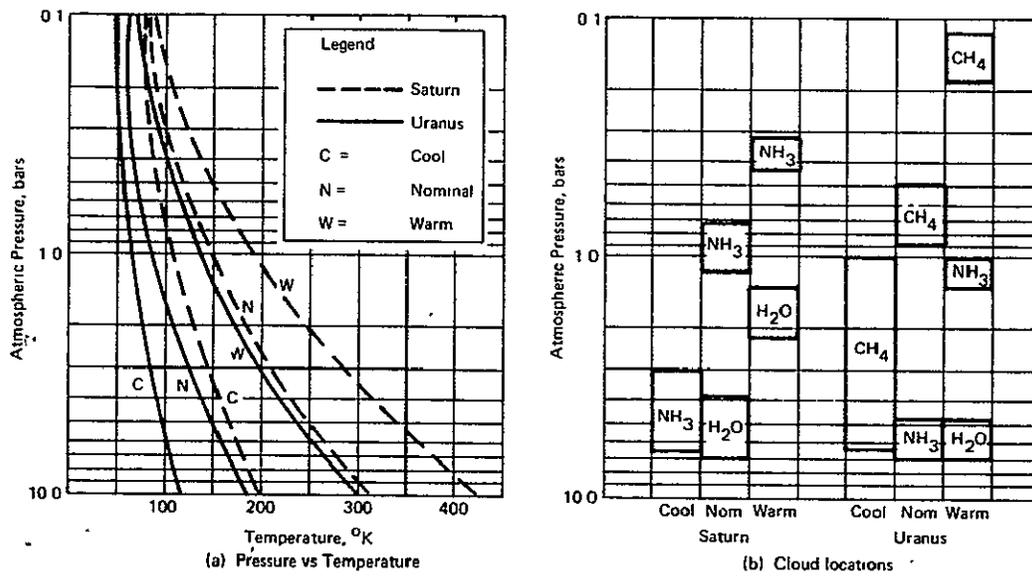


Figure 2-65. Model atmospheres in the probe terminal descent region.

Table 2-10. Model Atmosphere Compositions

CONSTITUENT	PERCENT COMPOSITION BY NUMBER					
	SATURN			URANUS		
	WARM	NOMINAL	COOL	WARM	NOMINAL	COOL
Hydrogen (H ₂)	94.68	88.57	73.00	95.29	85.85	30.56
Helium (He)	5.25	11.21	26.25	3.67	11.00	60.00
Methane (CH ₄)	0.02	0.06	0.22	1.00	3.00	9.00
Ammonia (NH ₃)	Trace	0.02	0.05	Trace	0.02	0.05
Water (H ₂ O)	0.03	0.11	0.37	0.03	0.10	0.30

I'll dispense with Saturn since this workshop is primarily interested in Uranus. Table 2-10 shows the atmospheric composition in terms of hydrogen, helium, methane, ammonia, and water for the warm, the nominal, and the cool model atmospheres. The nominal model is close to the existing solar abundance for hydrogen and helium, which, as I understand it, is currently estimated to be 90/10. The warm model, as shown, is 95% hydrogen and 3% helium.

But the real design problem in these models is the cool atmosphere with 60% helium, 30% hydrogen, which caused some real discomfort when we started looking at the heating rates and the associated heat-shield response. It also had an important implication on the peak decelerations which are shown on in Figure 2-66.

Referring to the curves for Uranus, one can see what happens if you target for an inertial entry angle of, say, 40°. If in the absence of any other information one is designing the probe to encounter a warm atmosphere it would experience about 180 G_e's deceleration. However, if in fact the 60% helium atmosphere were a reality, you would have to design the probe structure to withstand a peak deceleration of 600 G_e's.

So in putting the design together, what one does, of course, is choose the most severe entry angle consistent with the scientific instrument targeting requirements, which is 50°. And if we attempt the cool, it is clear that we will incur structural design weight penalty. Therefore, for the common entry probe, we are designing for 750 G_e's, which is almost twice the deceleration expected for the Pioneer Venus entry probes.

That is one part of the story.

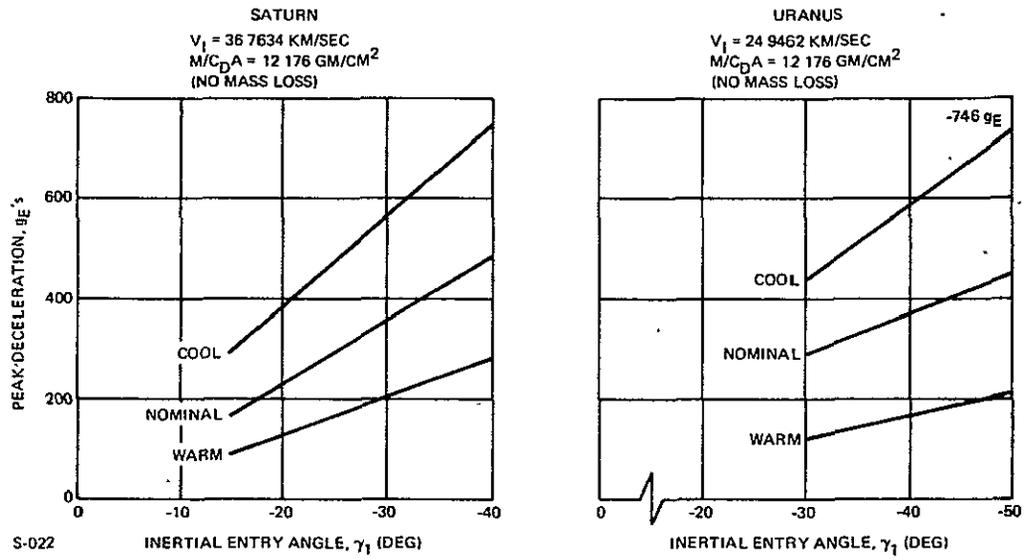


Figure 2-66. Peak decelerations.

DR. HUNTEN: When you play this game, you are taking an extreme bound on the temperature along with an extreme bound on the composition.

MR. VOJVODICH: This is right.

DR. JOHN S. LEWIS: Nick, is it true that far and away the single most important effect is in changing the scale height? You are not changing the optical property of the shock or anything like that; the importance is in changing the scale height?

MR. VOJVODICH: No, as I indicated on one of my earlier charts, the atmospheric composition also plays an important role in determining the probe design, particularly the heat shield. Specifically, Table 2-11 indicates where the importance of chemical constituents in defining the shock layer properties and related heating environments come in. I have shown the heating rate that gets to the surface or that is actually imposed on the vehicle. I have assumed the steepest entry angle along with the worst atmosphere (cool-dense) in terms of heating and the shallowest entry angle with the most benign (warm-expanded) to give me the full variance in terms of what the implications of the model atmospheres are on the resultant heating levels.

Table 2-11. Impact of Atmospheric Composition on Uranus Probe Heating Environment

ATMOSPHERE	ENTRY ANGLE	H _{PH} KM	P _{PH} 10 ⁻³ ATM	PEAK		INTEGRATED		
				T _{SL} °K	P _{SL} ATM	Q _T KW/CM ²	Q _C KW-SEC/CM ²	Q _R
Cool-Dense	-50°	92	3.4	14,930	13.0	63.2	36.0	72.3
Nominal	-40°	171	2.1	5,575	4.2	3.8	33.9	0.10
Warm	-25°	236	1.3	4,872	2.1	2.4	48.6	0.01

Table 2-11 shows the altitude (H_{PH}) at which peak heating would occur. In a cool-dense, it occurs at approximately 92 kilometers and it gets progressively higher -- 171 km for the nominal and 236 km for the warm, with the related external or ambient pressure P_{PH} outside the shock layer decreasing.

DR. OWEN: What is the reference there?

MR. VOJVODICH: From 3.4 x 10⁻³ atmospheres to 1.3 x 10⁻³ atmospheres.

These then are the constraints and the properties that exist outside the shock layer. Now when we consider the shock-heated regions in the vicinity of the probe surface we note some major differences. The gas has been decelerated, of course, with corresponding dramatic increases in both the temperature and pressure with respect to ambient values. The temperature for the warm and nominal mixtures where the primary constituent is hydrogen is on the order of 4,800° and 5,500° Kelvin, and the shock layer pressures are on the order of two to three atmospheres.

Now, for the very high helium content situation, the temperature is up to almost 15,000° Kelvin. The resultant prediction of the radiation heating rate to the probe surface is not easy for such conditions. I have prepared a little monograph which I handed out which gives the dependence of the convective heating rate on atmospheric properties. It is relatively easy to come up with closed-form solution for the convective heating. However, the radiative environment in the presence of ablation product gases is highly coupled and is characterized by a set of complex nonlinear equations which describe the conservation of mass, momentum, and energy. There are two AIAA papers that summarize the methodology required for computer solution as well as present parametric heating results for a selected series of entry cases. I think John Lewis considered the magnitude of the problem when we had discussions here at the Outer Planet Probe Workshop in April. He was interested in the fact that the situation involves a variety of factors that influence the heating, including temperature-dependent properties and absorption and radiation emission, coupled with the type and amount of ablation gas entering the boundary layer.

But the real message here is the critical dependence of the radiative heating rate on the shock layer temperatures; if you were to fit it by a power law, the exponent would be between five and ten.

The radiative rate for the nominal atmosphere is approximately four kw/cm²; it increases to 63 kw/cm² for the cool-dense, and the character of the problem is changed, too. The last three columns of Table 2-11 show, respectively, the peak radiative heat transfer rate and the time-integrated convective and radiative heating.

If one integrates the area under the heating curve, and contrasts the situation illustrated on Table 2-11 between the nominal and warm models, the probe experiences primarily convective heating, 48 kw-sec/cm² and 33 kw-sec/cm², respectively, with negligible radiative heating. However, for the cool-dense, helium-rich model, the probe experiences twice as much total radiation heating as convective. Adding Q_C and Q_R yields about 108 in the cool-dense, and about 34 in nominal, and 49 in the warm. So the atmospheric composition can thus be seen to have a sizable impact on the probe design, particularly how the heat protection problem is solved.

In addition to this atmospheric induced uncertainty in the level of the heating environment there is also a basic system impact question raised; namely, do we choose to go with a conventional charring-type (carbon-phenolic) of ablator which has been characterized extensively in both ground and flight tests or do we select a reflecting-type (silica-silica) which needs development but offers improved shielding from the intense radiation.

Therefore, given the external environment the designer must now consider the problem of solving in depth thermal response of the heat-shield. Part of the solution seeks answers to two questions: (1) How fast does the surface recede as a function of time, and (2) What is the resultant mass loss of the probe heat shield. It is essential to know how the material response is influenced by the chemical constituents that exist in the atmosphere.

Figure 2-67 addresses this question. The upper set of curves illustrates the rate at which a carbon phenolic vaporizes as a function of surface temperature, with atmospheric model and pressure as parameters. As the temperature is increased (by applying more and more heat transfer rate), there is a substantial increase in the normalized rate of ablation. For comparison purposes the two right-hand curves show what would be encountered if the probe were entering the Earth's atmosphere, air, as a function of temperature.

Curves for entry into Saturn are shown on the figure, but let's focus on the Uranus warm and Uranus cool-dense, which are also shown. This figure shows that the temperature dependence of these two is noticeably higher than for the air environment. Of course, if the ablation rate is driven up toward the sublimation regime, the computed results exhibit an overlapping.

The lower part of the figure contains similar curves for a reflecting heat-shield material (silica), which is currently under development. The same sort of temperature-dependent performance is observed. For reference, the dashed curve on the lower figure shows graphite in the Uranus warm atmosphere; this is why the silica reflecting ablators, although having the intrinsic capability of rejecting more of the imposed radiation, are not as efficient as the charring graphitic materials from the standpoint of mass loss, experiencing approximately a decade, a factor of ten or more in recession.

I have discussed thus far some of the essential design ingredients that are required to size the heat-shield system including insulation, and after going through all the steps, we have to come up with final heat-shield weights which are shown on Figure 2-68. In the case of the Saturn/Uranus probe 30% of the weight is allotted to the heat shield. That is why we are spending a lot of time addressing this particular technology aspect of the probe entry problem.

Figure 2-68 also shows the implication of the atmosphere on the design and why we are so interested in making sure this Uranus cool-dense model atmosphere situation gets resolved.

In the earlier studies we tried to design commonality for Saturn and Uranus and after comparing everything found that a shallow entry into the Saturn atmosphere, designed the heat shield for both systems. Then we went back and reran our calculations and did a lot more analysis because we started getting extremely high radiative rates for the Uranus cool-dense model and, lo and behold, it turned out that a shallow Saturn entry no longer was the designing case because we had to add almost one-tenth of an inch of material to this design to get to Uranus.

What I have plotted on Figure 2-68 is actual thickness of heat shield, which, of course, if it is a constant thickness can be transformed to weight. These calculations are for carbon phenolic and a backup structure with an assumed design criterion of 2,000°F maximum at the bond line and, of course, the insulation has to be put here to keep the instrument complement well below that temperature. Therefore, from a design standpoint this uncertainty in the Uranus atmosphere translates directly to a weight penalty on the order of

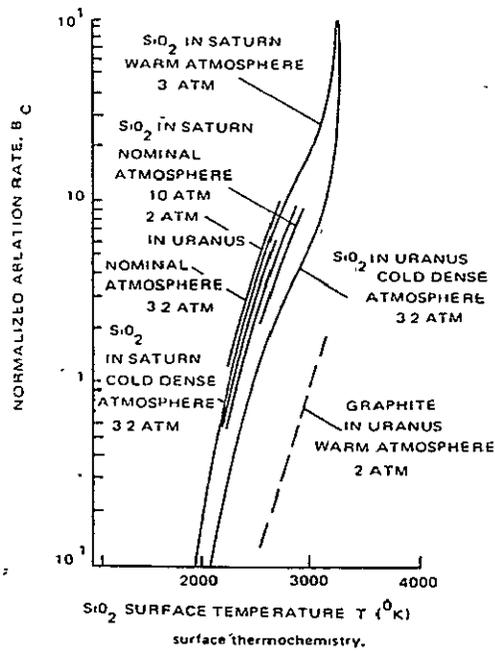
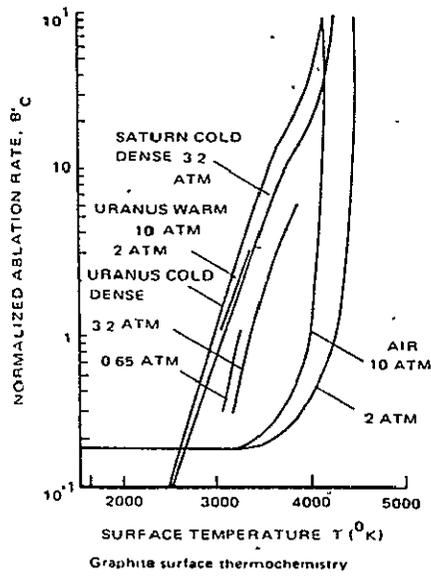


Figure 2-67. Influence of atmospheric composition on ablation rate of candidate probe heat shields.

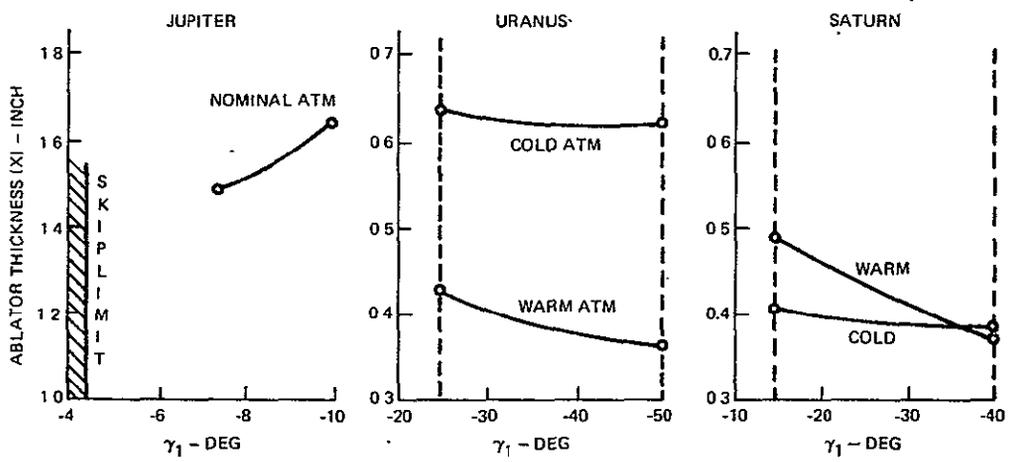
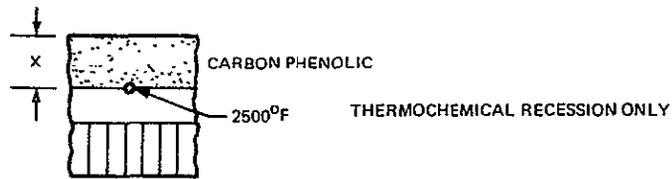


Figure 2-68. Heat shield requirements.

20 to 25 pounds. For comparison purposes Jupiter heat-shield requirements are also shown.

If we consider the baseline probe (250 lbs) with a 25-pound payload, the resulting ratio of total system weight to the weight of scientific instruments is approximately 10 to 1. Accordingly, if we could get some confidence that this Uranus cool-dense model is really somebody's wild dream and not a high probability situation, then we can focus our attention down in the region of a half inch or so of heat shield. I guess what we want to ask of the planetary scientists is: What is the likelihood of actually encountering this helium-rich atmosphere

Translating this to engineering hardware, another way to look at the situation is: instead of a 20-pound heat shield-penalty, the probe might be able to carry and provide the necessary engineering support functions for perhaps another instrument; that is, if we could design with certainty to the nominal or warm models, we could have the capability of putting on another three to four pounds of instruments, which, when you are talking about 25-pound payload, is certainly worthwhile.

DR. LEWIS: Let's take the case in which we fix the entry angle and fix the scale height of the atmosphere. What would be the effect of replacing the helium in that atmosphere with methane so that you get the same scale height but have a totally different radiative environment?

MR. VOJVODICH: In contrast to diatomic hydrogen monatomic helium has a limited number of degrees of freedom for accommodating energy and therefore for a given flight speed, a helium-rich mixture will have a higher shock temperature than a hydrogen-rich gas (see Table 2-11). Methane would have additional degrees of internal freedom for absorbing the energy. Therefore, I would estimate that the net effect of a methane/H₂ mixture would be to experience shock layer temperatures in the warm-to-nominal range.

DR. LEWIS: So the only threat, essentially, would be the deceleration loads, the peak G_e's during entry.

MR. VOJVODICH: This is right. That would take care of the heat problem. The cool-dense does give you a little higher shock-layer pressure, coupled with this steep entry angle. It is a multifaceted constraint game we are playing here.

DR. HUNTEN: You have chosen some extreme atmospheric entry angles.

MR. VOJVODICH: As I indicated earlier, our desire was to bound the problem.

DR. OWEN: Is the entry angle something you can control?

MR. VOJVODICH: In addition to satisfying the basic scientific interests relative to targeting constraints (day or night side, latitude, longitude), attention must also be focused on communication link constraints. In the case

of Jupiter, if we had our choice, we would back off and target close to the skip-out angle. That is the limitation on the other side if you come in too shallow (i.e., it would be analogous to a pebble glancing off a pond).

If the ephemeris is very accurately known, the probe can be targeted down around 5° or 6° for entry on Jupiter, where the skipout boundary is approximately four degrees and the three sigma variance, the accuracy at which you can achieve the target, is a half a degree. But there are, as I mentioned, other scientific trade-offs to be considered in the selection of the entry position.

DR. HUNTEN: I don't think you will find many scientific reasons for a steep entry as long as the communications can be satisfied and the target reached.

MR. VOJVODICH: Terry Grant, do you want to comment?

TERRY GRANT: You mentioned the three sigma variation on the entry angle for Jupiter as being very small. As I understand it, because of the ephemeris uncertainty for Uranus, a three sigma variation entry angle for Uranus (the order of 10°) is quite large, and I think that is where some of those numbers came from.

DR. HUNTEN: It might be cheaper to put some on-board optical navigation equipment on the bus rather than accept a penalty like this for Uranus. This is a trade that must be evaluated.

I am rather startled, in fact, to hear that people are all of a sudden talking about much shallower entries on Jupiter because now they know where Jupiter is because of Pioneer 10, but we have known for five years that Pioneer 10 would give that information. Why wasn't it factored into people's thinking? And I hope a similar prospect will be remembered for other planets.

MR. VOJVODICH: In summary, I have attempted to provide some insight as to what the designer of entry probes requires from the planetary physicists in terms of atmospheric definition. The primary characteristics are the composition and atmospheric structure (temperature and density as a function of altitude). Most important, the extremes from the nominal must be reduced to a reasonable level or the resultant design will incur an unnecessary structural and thermal protection penalty. I feel workshops such as this are invaluable in reviewing the available data, addressing the key issues, and arriving at, and documenting a consensus opinion as to the correct state of planetary atmosphere understanding.

Chapter 3

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