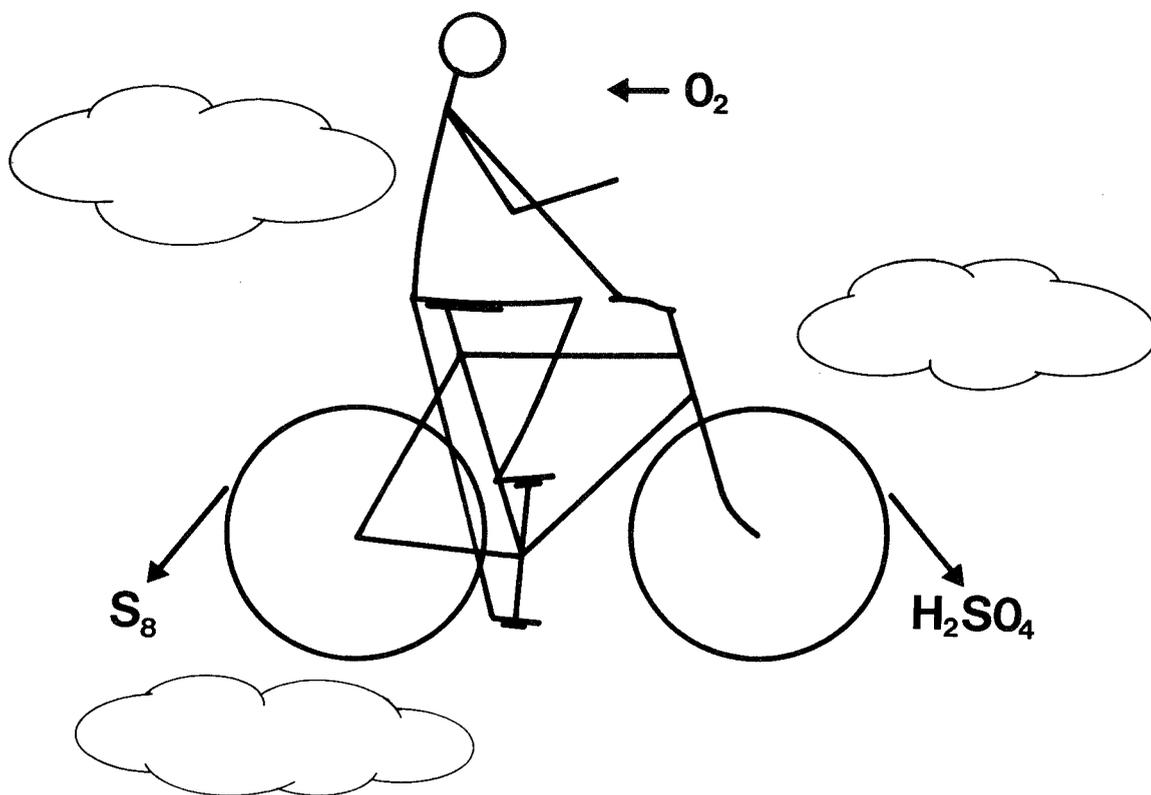# THE ATMOSPHERE OF VENUS

A conference held at

GODDARD INSTITUTE

FOR SPACE STUDIES

New York, New York

October 15-17, 1974

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION

The stick figure is a representation of the chemical bi-cycle, proposed by Ronald G. Prinn, which produces H_2SO_4 when there is an adequate supply of O_2, and S_8 when O_2 is deficient. The visible clouds of Venus are apparently predominantly sulfuric acid, and sulfur is one candidate for the cause of the dark markings in ultraviolet photographs.—J. E. Hansen

THE ATMOSPHERE OF VENUS

The proceedings of a conference held at the
Goddard Institute for Space Studies,
New York, New York, on October 15 to 17, 1974

Edited by

JAMES E. HANSEN
Goddard Institute for Space Studies

PREFACE

The Conference on the Atmosphere of Venus was held at the Goddard Institute for Space Studies, New York, on 15-17 October, 1974. The timing was intended to be late enough to include the principal Mariner 10 Venus flyby (February 1974) findings and early enough to possibly influence details of the planned Pioneer Venus (1978) experiments.

The format allowed considerable time for discussion, with each session having an invited review paper and a few shorter invited papers. The contributors were selected with the help of an organizing committee and the session chairmen. I particularly relied on the advice of Don Hunten and Ichtiaque Rasool.

Contributors were strongly encouraged to submit their papers for publication in a special issue of the Journal of the Atmospheric Sciences, which will be the June 1975 issue. Thirty papers, including the conference review by Don Hunten and a few papers not formally presented at the conference, will appear in that issue. The ordering I suggested for the papers in the Journal is intended to follow traditional emphasis in the Journal, rather than the order of presentation at the conference and in these proceedings. This should not cause any inconvenience, because the division into sub-sections on clouds, dynamics and atmospheric structure, aeronomy and atmospheric evolution is maintained.

Several papers planned by members of the Mariner 10 TV team were not ready in time for the special issue of the Journal. It is expected that those papers will be submitted to the Journal about June 1, and presumably published in late 1975 or early 1976.

In the present conference proceedings we include the review papers, the short papers not published elsewhere, the abstracts of the published papers and essentially all of the discussion. The reason for publishing these proceedings is based on the assumption that their informality will encourage readers to cover more than the parts concerned with their own specialty. Also it may be easier to grasp the main points of contention in this format, because the restrictions on the presentations encouraged the participants to get to the point.

I would like to thank those participants who helped in correcting their edited talk or their comments from the floor, and I apologize to those who did not check their contribution if the editing affected the intended meaning. It turned out to be possible to associate a name with each comment from the floor; the affiliations of all participants are listed at the end of the proceedings. However the irrepressible Dr. Jones, who is known to have affiliation with a number of institutions, preferred not to reveal his address.

I would also like to thank Lisa Nazarenko, Carl Codan and David Rosen for helping to organize the conference and David Ghesquiere for drawing many of the figures. And I am particularly grateful to Lisa, who put together these proceedings including all the typing and splicing in of figures.

J. E. Hansen

CONTENTS

WELCOMING REMARKS

Robert Jastrow, Goddard Institute for Space Studies

I am delighted to see a number of old friends here who we haven't seen for some time. Since Al Cameron and Ichtiaque Rasool departed we have not hosted a conference in the planetary sciences. This is the first time in some years that anyone has taken the initiative to make these arrangements to bring a number of people back to New York.

This particular meeting is, I believe, the first meeting devoted to the planet Venus since the Kitt Peak Conference in 1968 which Chamberlain, Rasool and I organized. And it comes at a fitting time, at the midpoint between the consolidation of earlier results and the preparation for the Pioneer flights in 1978.

There's one other element in opening a conference on Venus that I would like to mention. When we came to New York from Washington in 1961, the first work that Ichtiaque and I and a few other people were involved in consisted of reading Carl Sagan's thesis as a preliminary to our program of work in radiative transfer. We found that exercise very constructive. We cut our teeth on those theoretical problems at that time, also with the help of Richard Goody and Goody's book. So there's an element of nostalgia and particular satisfaction in seeing the fine program that has been put together, which brings matters full circle for us after about 13 years in New York and in this line of work.

I look forward to chatting with all of you and I welcome those of you who have come from a great distance and crossed several time zones. I hope that you will find the proceedings profitable and the evenings enjoyable.

CLOUD PHYSICS AND INTERACTION WITH DYNAMICS

Peter Gierasch, Cornell University

This is supposed to be a review of cloud physics work on the Venus atmosphere, and the first thing to say is that there is not very much to review so I can't really do a review. What I am going to do is chiefly just raise some questions. I want to begin by putting down some basic data that we have from interpretation of observations.

At a certain reference level in the atmosphere, where the pressure is about 50 millibars and the temperature is about 250°K, there are cloud particles. And I don't mind if these numbers are slightly wrong. They may not be consistent, but for my purpose small inconsistencies like a factor of two or three in this number don't matter.

There is a cloud and the particles have a very sharp size distribution, centered about 1 μm. The number density of particles is about 40 cm^{-3} if I assume that this level is optical depth 1 and that the particles are well-mixed in the atmosphere, which may not be quite true, so this may be off by an order of magnitude.

The atmospheric density at this level is about $10^{-4}g\ cm^{-3}$ which is about a tenth the density of the terrestrial atmosphere at the ground. The density of material in cloud particles is on the order of, I think, $3 \times 10^{-10}g\ cm^{-3}$ so the cloud particles form a very, very small fraction of the atmosphere at that level.

The density of water vapor in equilibrium over the cloud droplets is about ten times the amount of material that's in the cloud droplets, $3 \times 10^{-9}g\ cm^{-3}$. The particles are made of sulfuric acid of a concentration approximately 75 percent. This cloud is very high up in the Venus atmosphere. The surface pressure is about 100 atmospheres.

The cloud probably extends, with a diminishing concentration, two scale heights or so above this reference level, and it probably extends, according to Andy Young, in a well-mixed way about three or four scale heights below this reference level which makes its bottom at a level between 1 and 5 atmospheres.

Okay, that's more-or-less the basic data that exists.

The chemistry and composition of the cloud are important questions, but I'm not going to talk about them. I'm going to assume someone has given me a cloud with a certain amount of material in it and ask why it has broken itself down into chunks the size they are and where they are.

So the two particular questions that I want to keep in mind while I talk about processes and rates are, (1), why is the height of the cloud top what it is; and (2), why are the drops 1 μm in radius?

I can't answer the questions. I'm going to run down a naive list of rates of different processes which might determine the answers to the questions.

The rate equations for three processes that are important to the growth of droplets in a cloud are shown in Figure 1. Coalescence has a

COALESCENCE $\quad \dfrac{1}{t} \sim \dfrac{\rho_\ell g r^4}{\mu}\left(1+\dfrac{\lambda}{r}\right)n$

COAGULATION $\quad \dfrac{1}{t} \sim \dfrac{kT}{\mu}\left(1+\dfrac{\lambda}{r}\right)n$

CONDENSATION $\quad \dfrac{1}{t} \sim \dfrac{\mu r}{\rho_a}\dfrac{1}{1+\dfrac{\lambda}{r}}n$
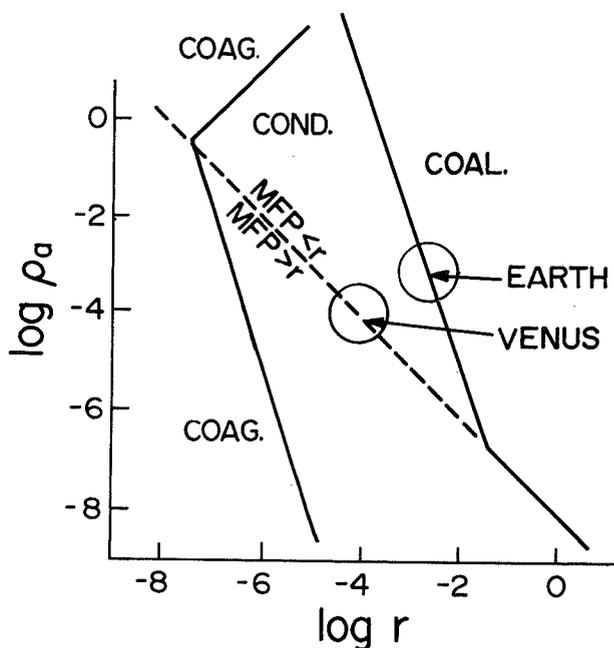
Fig. 1. Rate equations for three processes.

Fig. 2. Regimes in which different cloud physics processes dominate, as a function of atmospheric density and particle radius.

rate that is on the order of the density of the cloud droplets, times gravity, times the radius to the 4th power, divided by viscosity, times 1 plus Cunningham factor, times number density. Coalescence for my purposes is going to be growth of droplets which happens because of terminal velocities in the gravity field as they collide with one another, both proportional to the area and the speed.

Coagulation has a rate which depends on the thermal energy divided by a viscosity, and I've put in a Cunningham correction here to take care of the mean free path, and the amount of material.

Condensation is another process important to the growth of droplets in a cloud, and it occurs with a rate which is determined by a diffusion coefficient, and the size times 1 over another Cunningham factor, times the amount of material. And when I write this down, I've assumed that roughly half the material is in a vapor phase and half is in the droplets.

DR. JONES: What's coagulation?

DR. GIERASCH: Coagulation is particles colliding together because of thermal motion.

DR. JONES: Could you give more on the Cunningham factor?

DR. GIERASCH: I've put in a viscosity which will work when the mean-free-path is very short. And everywhere that this viscosity enters in these three equations, I've just plugged in this factor which is called the Cunningham factor. λ is the mean-free-path, r is the radius of the droplet. It corrects in the coagulation case for mean-free-path effects, and in the other cases it's not known how well this factor works. It's
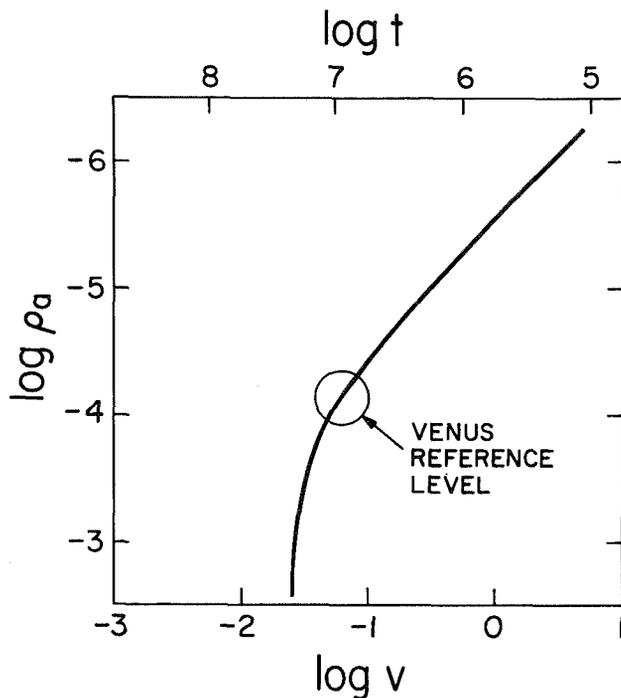
Fig. 3. Particle fall speed as a function of altitude.

Fig. 4. Time constants for reference level in the Venus atmosphere.

not crucial for what I'm going to say.

For all three of these processes, the rates are proportional to the amount of material that's in the cloud. The other two strongly varying parameters that enter are the atmospheric density and the radius of the droplets. Figure 2 shows a regime diagram, log of particle radius in centimeters on the horizontal axis, log of atmospheric density on the vertical axis. I have blocked out the regimes where each of these rates are largest. The dash line is where the mean-free-path equals the radius. My reference Venus level is indicated.

Cloud physics on the earth is complicated because we fall right on the edge between condensation and coalescence.

For the Venus situation, condensation is by far the most rapid of these rates. Its time constant is about 10 seconds. This is the time constant for a droplet to equilibrate if it's out of vapor pressure equilibrium with its surroundings.

Coalescence time scales at this point in this regime are doubly small. They are extremely slow, first because terminal velocities are slow, and secondly because Reynold's numbers around droplets are so low that droplets tend not to collide anyway under such slow terminal velocities.

Coagulation does happen at this Venus reference point but with a time scale of about 10 years for doubling droplets. So it's very, very slow.

So we should talk first about condensation since it's the most rapid thing that goes on. The first obvious remark to make is that the droplets must be in vapor pressure equilibrium with the surrounding gas. In the Venus cloud, since there's much more water vapor than there is material in

the droplets, that means that the droplets have come to equilibrium with the water vapor, rather than the water vapor being controlled by the droplets.

The second point is that there is a droplet growth process which involves condensation and surface tension, and in particular two droplets which are not quite the same size. The larger one will grow at the expense of the smaller one because of surface tension effects. That happens in terrestrial clouds and is an important growth process. But that cannot happen in these Venus clouds for the following reasons: The clouds are acid, and although there is surface tension, if the smaller droplet begins to get squeezed down it's the water vapor which has moved out, because the water vapor pressure is far larger than any of the sulfur-containing vapor pressures. So the concentration in the smaller droplet increases, and this decreases the equilibrium vapor pressure over it, which more than compensates for the surface tension effect. So these droplets in the Venus atmosphere are stable against this kind of process involving condensation transfer of vapor, and, it seems to me, we can set aside condensation effects as a cause of changing particle sizes in the Venus atmosphere.

Figure 3 shows the next rate I want to mention. Plotted there is the particle fall rate -- the logarithm of the particle fall speed as a function of height. For the reference level in the Venus clouds the fall-out time constant, the time to fall one scale height, is about 10^7 seconds. That's a time constant that we'll talk more about later.

The other important thing is that we are at the level where the mean-free-path approximates the particle radius. Above the reference level particle fall speeds increase rapidly with height. Below that level particle fall speeds are approximately constant with height. And the particular numbers illustrated are for 1 μm radius particles.

In Figure 4 I have a summary of all these time constants. There is one new number on this slide, the radiative time constant near the cloud top, which is about 10 days. Another number that's important is the dynamical turnover time for the whole atmosphere, and for that reason I'm anxiously awaiting Peter Stone's review. For that number I've put down 10^7 seconds, which comes from some of his earlier work.

So we have: fallout time, 10^7 seconds; coagulation time, 3×10^8 seconds; radiative time constant, 10^6 seconds; and deep dynamical over-turning time on the order of 10^7 seconds.

The remarks that I think are important are: First, at some level just above this reference level, the fallout time constant, which decreases with height, becomes equal to the radiative time constant in the atmosphere.

Secondly, at some level not too far below this reference level, the coagulation time constant, which becomes shorter as you go down because the particle number density becomes greater, becomes equal to the fallout time. That is obviously going to be an important balance.

The third remark is about this dynamical time, which is very embarrassing. It's on the same order as the fallout time, but it's a very uncertain number. That's why I'm anxiously awaiting Peter Stone's review tomorrow.

Now, these are the processes that involve dynamics and not chemistry that I've been able to identify as potentially important. I want to finish up by making two conjectures about the possible answers to my two questions. I don't necessarily believe them, but the conjectures at least

illustrate the kinds of interactions, the kinds of servomechanisms that might be working between these time constants to produce a cloud top where it is and a particle size what it is.

First, about the height of the cloud top. Prinn has recently done the following exercise: Assume that eddy mixing holds the cloud up against particle fallout. Then since we know particle fallout speeds, we can estimate what the eddy mixing should be to hold them up. The value is about 10^5 cm^{2}s^{-1}, which corresponds to a time of about 10 days to fall a scale height.

But the real question is what causes that eddy mixing. Goody, Ingersoll and I have together and separately worked on different aspects of this. One possibility that Ingersoll and I have suggested is that near the top of a cloud such as this one there are radiative instabilities which operate in the following way:

Suppose cloud particles absorb sunlight and their density falls off with increasing height. Then a wisp of cloud that rises a bit is warmed more strongly than the ambient air around it, it therefore will rise further. The cutoff to this process would be at a height roughly where the radiative time constant equals the fallout time. This seems to be roughly the way things are. Remember the fallout time is a factor of 10 smaller, about two scale heights above the reference level.

So that's my first conjecture, that the fundamental physical rate balances that determine the height of the cloud top are radiative time constants versus fallout times.

My second conjecture involves particle sizes. I cannot understand how, by any dynamical processes in the Venus cloud, particles grow in size unless they are cycled up and down in the cloud and have a lifetime which is quite long, compared to the dynamical time constant for circulations up and down in the cloud.

There may be chemical processes, nucleation processes, that really determine the number of particles in the Venus cloud. But if not, if the number density and the radius is determined by dynamics, then the only way you can get a particle to grow very big is to cycle it down to the bottom of the cloud, where coagulation can be important. The eventual balance would be coagulation time versus fallout time for that given particle radius.

Now, it is true that if particles move down in the Venus cloud and reach a level where the coagulation time is the same order as the fallout time, there could be a balance. But in order to observe particles up at the top, there must be cycling back up. Also, you would probably need to have the cycling in order to produce a sharp size distribution.

So I offer that as a conjecture for a dynamical explanation for the size of the Venus cloud particles. The physical balance would be the coagulation rate at the cloud base against the fallout rate at the cloud base.

And finally, the questions that are raised by all of these speculations are: One, and most important, what really is the nature of the large-scale dynamics? Is the overturning time shorter than the particle fallout time?

Two, what is the nature of the small-scale dynamics? What really does determine vertical mixing near the cloud top? Is it this radiative instability, or is it shear instabilities, or is it just the large-scale circulation?

Three, what are the radiative heating and cooling rates that drive motions near the cloud top? We need to know the ultraviolet absorption, we need to know the visible absorption, and we need to know the infrared cooling rate. The kind of cooling maps that Ingersoll and Orton [Icarus 21, 121, 1974] have produced from Murray's old observations are very good things to have.

Four, what is the number density and radius of particles as a function of height and time through the cloud?

I hope some of the questions will be answered at the conference.

DR. CESS: We will now have a second review on cloud physics. After it we will be open for questions to both of the speakers.

CLOUD PHYSICS

Andrew Young, Texas A&M University

I'd like to write some basic information on the blackboard to remind you what the atmosphere is. I want to first put down the chemical composition. (see Table 1.)

The atmosphere is well over 90 percent CO_2. It's close to 100 percent but we don't know precisely how close. The only minor ingredient that appears to be uniformly mixed is CO, which has a mixing ratio of about 5.1×10^{-5}, and this figure is fairly accurately determined.

Then there are a number of constituents which have variable mixing ratios. Some of them are known to be variable, and some of them are very likely to be variable. We have H_2O, for example, which in the vapor phase runs typically between 10^{-5} and 10^{-6}. Ed Barker says that it sometimes gets a little outside these limits, but it varies by at least this order of magnitude.

HCl is about 4×10^{-7}, and it is probably variable. (These are vapor phase mixing ratios, and they are number mixing ratios, not mass mixing ratios.) And the HCl line seems to be formed deeper in the atmosphere at a higher pressure than the CO.

And finally we know about HF, which in the vapor phase is of the order of 10^{-8}.

There are a number of constituents that people have looked for and not found. No oxygen has been detected that I know of, and that means it has

COMPONENT	MIXING RATIO	REMARKS
CO_2	0.9+	Major constituent
CO	5.1×10^{-5}	Uniformly mixed
H_2O	10^{-5} to 10^{-6}	Variable
H_2SO_4	2.3×10^{-6}	Liquid
HCl	4×10^{-7}	<u>Not</u> uniformly mixed
HF	10^{-8}	
O_2	$< 5 \times 10^{-6}$	Upper limits on unobserved gases
NH_3, H_2S, COS	$< 10^{-7}$	"
SO_2	$< 10^{-8}$	"
O_3	$< 3 \times 10^{-9}$	"

Table 1. *Cloud-top Composition of the Venus Atmosphere.*

to be less than about 0.1 of the CO. And this is very peculiar because we expect the CO to be produced by photodissociation of CO_2, which means that there should just be a factor of 2 between the CO and the O_2, and instead the factor is more like 10. So oxygen, molecular oxygen, is at least five times under-abundant. That means we have to think of some place for the oxygen to end up.

$$n_{5500\ \text{Å}} = 1.44 \pm 0.015$$

$$a_{eff} = 1.05 \pm 0.10\ \mu m$$

$$\text{size variance} = 0.07 \pm 0.02$$

$$p = 50 \pm 25\ mb$$

$$T = 250 \pm 10°K$$

Table 2. Cloud-top Aerosol Properties

In addition to fluorine and chlorine, which represent cosmically fairly abundant elements, we have to worry about compounds of sulfur, and sulfur is quite a bit more abundant than these two halogens. People have looked for H_2S, and they've looked for SO_2, and COS, and the upper limits on these are on the order of 10^{-7} and 10^{-8}. I might add that the upper limit on ozone is something like 3×10^{-9}, so it's another "forget-it".

It's puzzling that the sulfur compounds are so under-abundant. This was a mystery for a long time. But we now think we know where the sulfur has gone. As you probably know, this is explained by saying that sulfur is turned into sulfuric acid, and sulfuric acid explains a lot of things about Venus. It explains the extreme dryness in the upper atmosphere. The water-vapor mixing ratio is typically less than 1 percent relative humidity at 50 millibars and 250°K.

The number density of the aerosols (Table 2) corresponds to an H_2SO_4 mixing ratio -- which is in the liquid state, not the vapor state -- comparable to that of water, on the order of 2 or 3×10^{-6}. The H_2SO_4 droplets also have at least as much water in them, apparently, as they have H_2SO_4, in terms of the number of molecules.

DR. JONES: Would you repeat that number?

DR. YOUNG: This number comes from saying that a 1 μm aerosol is, to a first approximation, uniformly mixed in the atmosphere, and it has to reach optical depth unity at 50 millibars. When I did the arithmetic I got 30 per cubic centimeter. I'm just trying to give you order of magnitude numbers in order to have some feel for what we're looking at. So my numbers might be off by a factor of two but don't worry about little factors.

DR. JONES: I think I'd argue that the H_2SO_4 is a little larger than that.

DR. YOUNG: Well, I'll let you argue it, okay? This is the number I got when I did the arithmetic.

The number density of droplets agrees very well with the mixing ratio in Table 1, and to give you some idea of what this corresponds to, the mean separation between droplets is something like 1.8 millimeters. So they are not very far apart.

The fall velocities that I calculated indicate that it takes something like 200 days for particles to fall a scale height, and that's roughly in

accord with Peter Gierasch's 10^7 seconds. And that means we have a problem, as Peter pointed out, in explaining the weather phenomena that we see on Venus where something happens in a four-day cycle, because 200 days is very long compared to four days.

And another point that Peter made which I want to emphasize is the fact that the H_2SO_4 droplets are non-volatile. Once you form an H_2SO_4 droplet, you can't get rid of it; you can't make it disappear in any way. On the earth, you can have weather phenomena that take place in a short period of time, because if you take a cloud and you raise the temperature a few degrees, all the water particles evaporate. If you take a cloud on Venus that has sulfuric acid in it, and you heat it up a few degrees, a little bit of water cooks out and the particle size changes by a few percent, but the number density of particles is just the same. You haven't done anything drastic to the cloud.

Now, there's been some argument about what the H_2SO_4 concentration is in the droplets. If you look at the water-vapor mixing ratios in Table 1, and ask what concentration of sulfuric acid is required to be in equilibrium with those mixing ratios, the answer is something like 85 percent. There's a range, of course, so let's say from 80 to 90 percent.

On the other hand, you have refractive index data on these droplets. In fact, that's how sulfuric acid was identified, by the aerosol's peculiar refractive index of about 1.44 in the visible. And according to the latest word from Dudley Williams, the composition of sulfuric acid that agrees best with the refractive index, at 250°K, is about 70 percent H_2SO_4 by weight.

I should point out that H_2SO_4 and water form a very complicated system. They form a monohydrate which is 84.5 percent H_2SO_4 by weight, and they also form a dihydrate which is down around 73 percent H_2SO_4. And this is the composition range that we're interested in. Within this range, the infrared spectrum of sulfuric acid does not change in a very drastic way. When you get to higher hydrations, that is lower concentrations, around 70 percent acid or less, then you start seeing strong water features in the spectrum. When you get to higher concentrations of sulfuric acid, above 85 percent acid, then you start seeing features due to molecular H_2SO_4. None of the infrared spectra of Venus in the 10 μm region that I've seen shows the strong H_2SO_4 features in that region. That means that this is an upper limit on the concentration.

Now, remember the point that Peter Gierasch made, that droplets have to be in vapor pressure equilibrium with their surroundings. That means when you get very dry conditions, you would expect to have acid concentrations over 85 percent, and so you've got a problem.

I'd like to discuss one way in which you might get rid of that problem, or at least a possible complication that ought to be thought about, that's due to the HF. Whenever you have a strong sulfuric acid solution, H_2SO_4 and HF react very strongly and they form a little bit of water and a horrible thing which is HSO_3F, fluosulfonic acid, a very corrosive material and also an extremely stable molecule.

If you add a little HF to H_2SO_4 acid droplets, you do two things to them. One, you lower the refractive index. The refractive indices of hydrofluoric acid solutions are actually below those of water. A strong hydrofluoric acid solution has a refractive index of 1.31, as opposed to 1.33 for water. So this tends to bring the refractive index of the droplets down. That means that the concentration of sulfuric acid in the droplets must be above 70 percent to give the best agreement with the observed refractive index.
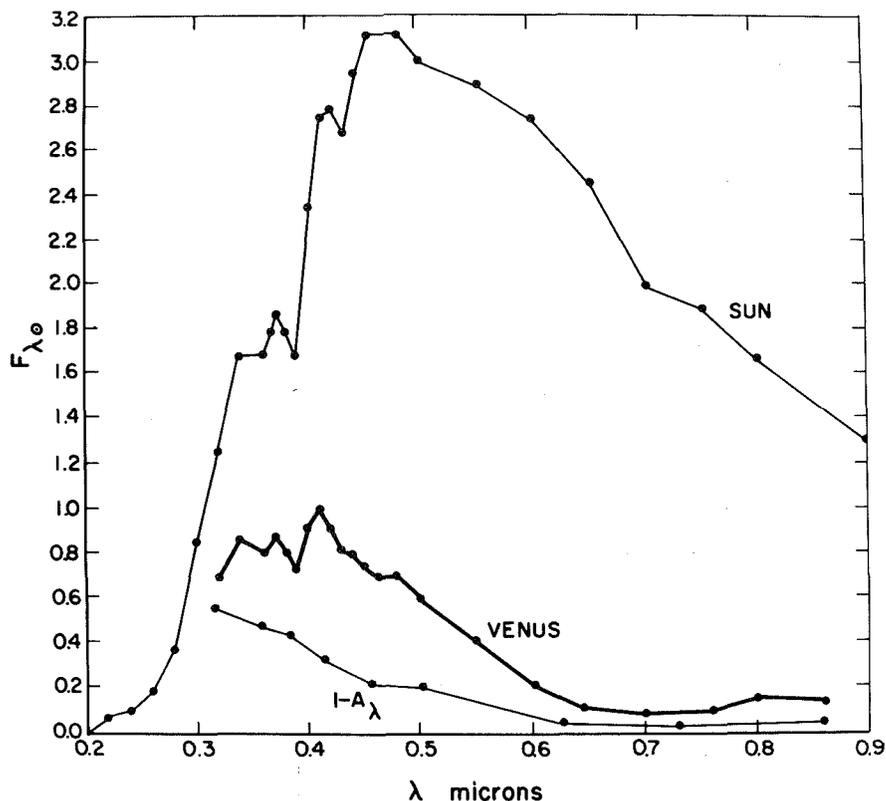
Figure 1. Solar spectral energy distribution, the fraction (1 - A_λ) absorbed by Venus, and the spectral distribution of the energy absorbed by Venus. [After A. T. Young, Icarus 24, 1, 1975].

The other thing that HF does is gobble up water. HF hydrogen bonds the water very strongly and also forms a monohydrate just as sulfuric acid does. In fact, it's such a powerful drying agent that there is no chemical substance known that will extract water from hydrofluoric acid solution. So adding the HF lowers the vapor pressure, and that means that to agree with the vapor pressure you want a lower sulfuric acid concentration in the droplets to get things back into equilibrium.

So HF helps in two ways: It enables you to have more than 70 percent H_2SO_4 to agree with the refractive index, and it enables you to have less than 85 percent H_2SO_4 to agree with the water vapor. The question is whether this happens with a reasonable amount of HF or not.

I think the answer is yes. Fluorine and chlorine are similar in cosmic abundance, and yet in the gas phase there's 40 times more HCl than HF. If you imagine that in the original atmosphere there were equal amounts of HCl and HF, and that most of the HF has gone into the droplets, what does this mean? It means you dissolve something like 4×10^{-7} parts of HF in a few times 10^{-6} of H_2SO_4, so you've got a few percent of HF in the H_2SO_4. That seems to be just about what it takes to bring all these figures into agreement.

Now I don't want to leave you with the impression that we've solved the problem because, unfortunately, so far as I can tell, the physical chemists haven't studied this system in any great detail, particularly at the pressures and temperatures that we encounter on Venus. So there's a great need for laboratory data on this system of water, sulfuric acid,

and HF. And it's a tricky system to study in the laboratory because every-
thing is so [expletive deleted] corrosive. But it needs to be studied
quite extensively.

There's a second problem we need to worry about, and that's the
mysterious ultraviolet absorber that gives the planet its yellowish color
and is somehow responsible for the features we see in ultraviolet photo-
graphs. This absorber is not yet identified, or at least people don't
agree on what they think it might be.

Figure 1 shows that the ultraviolet absorber plays a very important
part in the behavior of the clouds. The top curve is the energy distribu-
tion of the sun in the visible, near infrared and near ultraviolet. The
bottom curve is 1 minus the albedo of Venus, the fraction of the sunlight
that is absorbed. And the product of the two (in the middle) shows the
spectral distribution of the energy that is absorbed from sunlight. The
bulk of the energy is centered right around 0.4 μm. In other words, this
ultraviolet absorber is responsible for most of the heat input to the
clouds. That's something that might not have occurred to you, but it's
important. It means the ultraviolet absorption variations play some
important role in the weather pattern, but I don't know what kind of a role
they play.

DR. BELTON: What is the number? Is it 50 percent?

DR. YOUNG: It is on the order of 50 percent of the total heat input.

Now, another thing that needs to be borne in mind is that the droplets'
radius of 1 μm means that they are "big" compared to the wavelength of
light. That means they are neutral scatterers, apart from the ultraviolet
absorption, and the optical depth of the cloud doesn't depend strongly on
wavelength in this whole visible and ultraviolet region. So where tau
equals 1 at one wavelength in Figure 1 is also nearly where tau equals 1
at any other wavelength in the figure.

That means that everything that we see in the visible and near infra-
red and near ultraviolet is in the same part of the atmosphere. It isn't
true, as people once thought, that you see one level in the atmosphere at
one wavelength and a different level in the atmosphere at a different wave-
length. Everything we see is happening in the same place. That means that
the conditions under which the ultraviolet absorber is living is this 50
millibars and 250°K.

Now, there are problems in understanding this ultraviolet absorber.
We have attempted to attack these from an observational point of view by
looking at temperatures in the clouds and at the amount of CO_2 absorption
over light features and dark features in the ultraviolet. But before I
get into those, I want to talk about another phenomenon that we see in the
ultraviolet, the four-day apparent rotation of the atmosphere.

People are sometimes under the impression that everybody is unanimous
in saying, yes, the atmosphere runs around every four days, which would
require winds on the order of 100 m/s. However, there have been several
attempts to measure these winds directly by means of the Doppler effect in
reflected sunlight, and there is a considerable scatter in the results.
Of the results that people claim reasonable accuracy for, or maybe un-
reasonable accuracy for, we have Slipher's results back around 1900 [Lowell
Obs. Bull., no. 3, 1903] where he got something like 10 ± 10 m/s. Frankly
I don't believe Slipher's number, but that's the number he got and he
claimed it to be accurate.

We have Richardson's measurement in 1958 [P. A. S. P. 70, 251, 1958], where he got a retrograde rotation of 32 ± 33 m/s. Just on the face of it, that is almost three standard deviations away from the 100 m/s that we need to explain the four-day rotation.

And then we have some French observations in the 1960's [B. Guinot and M. Feissel, J. Obs. 51, 13, 1968] which were made after the French discovery of the four-day rotation and which exactly confirm the four-day rotation. They got 103 ± 10 m/s.

Now, there's a fly in all this ointment. Figure 2 shows the fly, and it's this: The sun is rotating with an equatorial velocity of 2 km/s. The lower limb on Venus is more strongly illuminated by the lower limb of the sun, which is approaching it, then the upper limb of the sun. The angular subtense of the sun at Venus is three-quarters of a degree, and even though that's a small fraction of a radian, we are trying to measure rotational speeds on Venus, Doppler shifts, that are a small fraction of the solar Doppler shift. You can see what's happening -- one side of the sun is coming toward Venus and that makes the corresponding side of Venus look like it's going toward the sun. That produces a spurious, apparent rotation in the retrograde direction; and the amount of that spurious rotation, if you believe where people have observed on a planet is really where they say they've observed, is typically on the order of 30 or 40 m/s.

Fig. 2. (a) Sun and Venus seen from the north ecliptic pole. (b) Detailed illumination geometry. At the point P, an angular distance θ from the red-shifted terminator T_R, the angle of incidence from the red-shifted solar limb is i_R, and that from the violet-shifted limb is i_v. [After A. T. Young, Icarus 24, 1, 1975].

Now, that knocks Richardson's number down from 32 to essentially zero. In fact, it actually knocks it positive by a few meters per second, which makes it even tougher to reconcile with 100 m/s retrograde.

It knocks the French number down to something like 65 m/s. And, because they claim a probable error of 10 m/s, 65 ± 10 m/s doesn't agree with a four day rotation. It agrees with a six or seven day rotation.

So there's a problem. Wes Traub has made some measurements, and at least some of his measurements seem to agree with the 100 m/s. I hope he will talk about those later on in the meeting. But the point is we have some observational evidence here which is just not compatible with a four-day rotation of the planet's atmosphere.

I might add there are other observations that are not compatible with it. Figure 3 shows some observations of the amount of CO_2 over various parts of Venus in September and October of 1972. Over a considerable interval, there is a remarkable gradient between the limb and the terminator.

This limb to terminator gradient is persistent and occurs on days when the CO_2 is increasing as well as days on which the CO_2 is decreasing.

Now, this presents a problem, because if you look at Venus -- you're looking at the morning terminator; things are supposedly going retrograde -- what you see at the terminator today you ought to see over at the limb tomorrow. That means in particular when the amount of CO_2 is decreasing, you ought to see less CO_2 at the terminator than you do at the limb.

But, unfortunately, on September 26 when the CO_2 was decreasing quite markedly — we had good observations the day before, good observations the next day, and the CO_2 was going down like mad — we still have less CO_2 at the limb than at the terminator. So that doesn't match up with a four-day rotation. And you can't say, "Well, the particles have fallen out or something," because the fallout time is on the order

Fig. 3. CO_2 amount as a function of longitude on Venus. The phase angle was about 75°. [After A. T. Young, et al., Acta Astron. 24, 55, 1974]

of a couple of hundred days. You can't get rid of those particles. You can't change the amount of gas you're seeing above the clouds that rapidly. They can't evaporate, you can't get rid of them, so why don't these ultraviolet features get wiped out by differential motions if there are really winds blowing 100 m/s? 100 m/s on the earth is a jet stream. You have a lot of turbulence associated with it. Why don't the features all get mixed together and the UV features go away? Why isn't the planet homogenized? That's a problem.

Well, you might think that you could produce this kind of effect if you changed the temperature of the gas, because if you warm the gas up, you drive some of the water out of the acid droplets, and the droplets get smaller and denser. In fact, it turns out that most of the effect is due to the change in composition, because the sulfuric acid solution is very sensitive to composition in this range. So you might see in deeper if you warm the gas up. And that means if you look at the variations from day to day, on the days when you see more CO_2 you ought to see higher temperatures, right?

Well, we've measured temperature, and for the first time, or nearly the first time, we are seeing what we think is a real variation in temperature. (Before, Louise [Gray Young] and Ron [Schorn] have claimed that they saw indistinguishably the same temperature all the time. That is, the internal scatter in the temperature determination was comparable to any real variation in temperature.) For this particular run, the data are quite good. We have something like 50 spectra, and the internal scatter is 3 or 4 degrees. That's the standard deviation that comes out of Louise's temperature fit. But the external scatter, the standard deviation of this population, is something like 10 degrees. So we are seeing, we believe, a real 8 or 9 degree temperature variation.
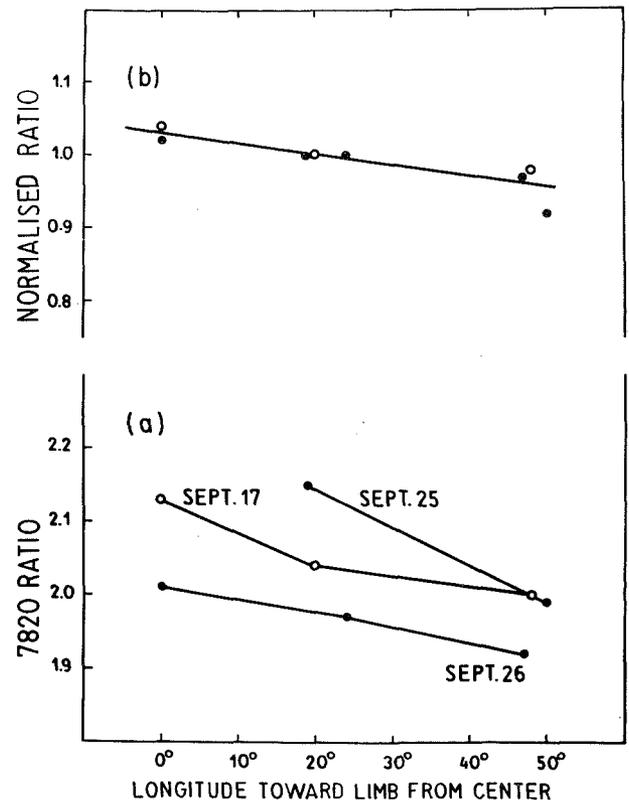
If you heat the gas up 8 or 9 degrees you change the vapor pressure enough to change the water content by about 3 percent. That makes the particles smaller and denser. That should decrease the optical depth by a few percent, which should be very noticeable, and you should have a nice correlation between the temperature and the amount of gas.

Figure 4 shows what we actually found. You see that there is essentially no correlation between the amount of CO_2 absorption and the temperature variation. There is a little bit of negative correlation showing here, whereas we expected a positive one. In fact, this tiny bit of negative correlation is due to the fact that we observed more low-J lines than high-J lines. If you raise the temperature you're shifting equivalent width out to the larger-J lines that we didn't measure. That accounts very nicely for this very small negative correlation. Even if you don't take that effect into account, the correlation here is only 20 percent and it's very feebly significant, statistically.
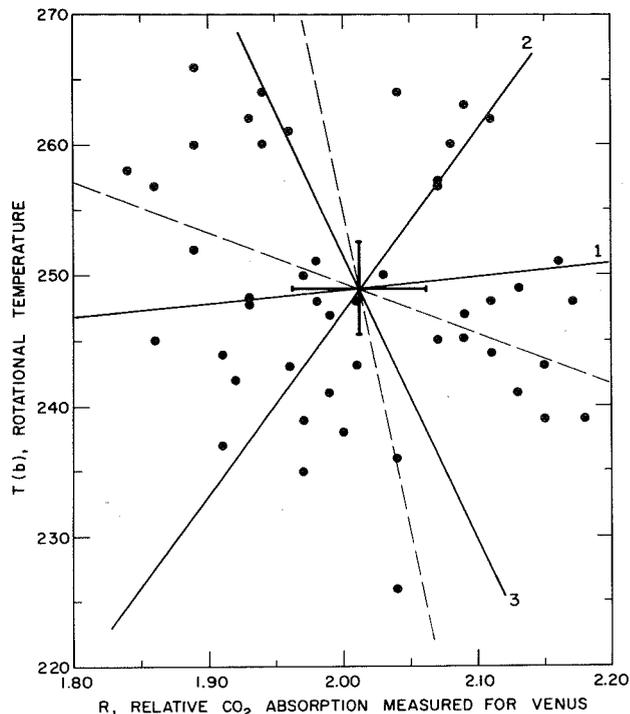
Fig. 4. Amount of CO_2 absorption on Venus and the corresponding rotational temperature.

DR. JONES: Is that a reflecting layer model or a scattering model that these temperatures come from?

DR. YOUNG: These temperatures come from assuming that there's some curve of growth. You don't care what the model is that gives rise to it. You determine the curve of growth slope from the CO_2 band, empirically from the measurements. And we've shown that this takes out not only the effect of whatever the line formation mechanism is, but it also takes out little errors in drawing a continuum. Because all you are doing is comparing a high-J line with a low-J line of the same equivalent width. You are asking what temperature populates those two levels equally. You don't care what the details of line formation are. You just want those two lower levels to be populated the same. That tells you what temperature you have to put into the Boltzmann distribution, okay? That's the physics of it.

Well, we don't get any good correlation between temperatures and amount of CO_2, even though there are quite significant variations in both. That is, this is not just a plot of experimental error. The bulk of the variance that you see in both directions is real, and it's totally uncorrelated.

All right, maybe you think the ultraviolet markings ought to match up with something. Anyway, *I* thought the ultraviolet markings ought to match up with something, so let's look at Figure 5. It shows the shade, estimated on an arbitrary scale from 1 to 5. At the bottom is light markings;
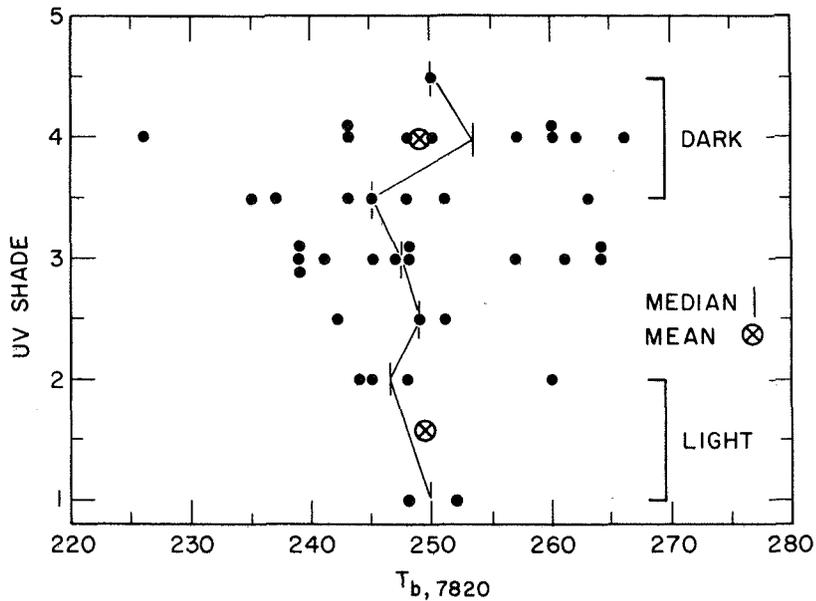
Fig. 5. Rotational temperatures for areas of different UV darkness.

at the top is dark markings. And you can see that the mean temperature for the light and dark groups are the same, within a degree.

Now, there is a tiny effect, which is why I show Figure 5, since all our other correlation diagrams show zilch. The one effect that appears to be here, *if* it's here, is that the temperature spread in the light markings is a little bit less than the temperature spread in the dark markings. I'm not talking about the range, because we've got more data on dark markings, so you'd expect the range to be bigger. The actual standard deviation of the dark-marking points is about twice the standard deviation of the light-marking points. In fact, the lower standard deviation is close to our 3.5 degree internal error. So light markings all seem to have, within a few degrees, the same temperature, whereas dark markings come hot, cold and in-between.

We have also looked for differences in the amount of CO_2 absorption between light and dark markings. The difference in CO_2 absorption, taking out the day-to-day variations and just looking at the variations over the planet on a single day, is 1 ± 3 percent in total CO_2 absorption. In other words, the dark markings in the ultraviolet are totally independent of anything we can measure spectroscopically. On the average they have the same temperature, whether they are light or dark. They have the same amount of gas, whether they are light or dark. It's just as though somebody took a paintbrush and painted pale yellow stripes on the planet and left everything else the same.

Well, why do we have no correlation between UV markings and temperatures and pressures? Why don't any of these things match up?

I have a feeling that wherever tau equals 1 is essentially the level which radiates away to space the heat that's absorbed by this cloud, whatever it is. So we're always looking at the same temperature, regardless of where that occurs in the atmosphere. The radiative equilibrium is essentially determined by the cloud, which is extremely opaque in the thermal infrared. Wherever tau-equals-1 happens to come is where the

temperature gets radiatively adjusted to about 250°K on the average, although there is about a 10 degree real scatter from place to place.

DR. JONES: Is the problem the fact that there is no spatial variation of CO_2 equivalent width?

DR. YOUNG: Yes, it's a big problem. You see, the CO_2 amount apparently varies with time, but it is essentially constant over the planet, except for the limb to terminator gradient, on any given day, whereas the temperatures bounce around all over the planet and don't show a lot of variation from one day to the next. So the amount varies with time and the temperature varies with the position on the planet, to a first approximation. It's very confusing. I don't understand what's going on. But this is what the observations say.

I also want to remind you about the general phase variation of CO_2 equivalent widths. There is apparently a maximum around 60 degrees phase, and then it falls off toward larger phase angles. But in our 1972 data, as the four-day oscillations in CO_2 absorption went up and down, there was a general downward drift contrary to the slope of the long-term average phase curve.

That brings up the point that you must not think that four days is the only time scale that applies to weather on Venus. There are longer time variations. You see a lot more scatter in the phase curve, close to a factor of 2, than you see during that particular three-week run in 1972 when the range was only about 20 percent. So there are long-term effects in the weather.

This is also seen when you look at the ultraviolet photographs. For a while you'll see ultraviolet features like mad and then for a while they kind of poop out, and there's not much to be seen. Then they come back again. That occurs on a time scale of a few weeks. And a few weeks is the time it takes for these particles to fall a couple of tenths of a scale height, which is the kind of changes that we see in the long-term weather. So maybe this long-term weather cycle has something to do with coagulation or fallout or something like that.

DR. BELTON: Since Professors Dollfus and Fymat are here, I don't feel any particular need to defend the French, but I think that the original paper that Guinot wrote on motions actually did precede the establishment of the four-day circulation.

DR. YOUNG: Okay. Their final paper followed it.

DR. BELTON: A minor point. We don't want to have an international incident.

DR. YOUNG: It just makes me worry a little bit because, you see, Slipher got zero when the word was that Venus always kept one face toward the sun. And then the French get four days when the word was four days, and the only person who hasn't had an axe to grind in this game is Richardson. And his data are hard to shoot down because he also checked his technique by measuring a rotation rate of the sun and a rotation rate of Mars, and the systematic error that he found in each of those two cases was on the order of 5 m/s. So it's hard to find enough systematic error in Richardson's measurements which were made, I might add, at five times the dispersion of the French measurements. He used the Snow telescope at Mt. Wilson and the dispersion was 0.8 Å/mm.

DR. BELTON: It seems to me the one argument you miss, when you talk about whether the mass motion is real or not, is the question about the temperature of the dark side stratosphere. That's a question that has to be answered, and it seems to me you have no answer to it.

DR. YOUNG: That's right.

DR. BELTON: The second comment I would like to make is: I think this is a hell of a cloud. If you work out the scattering mean-free-path from the numbers that Peter Gierasch gave, you get something like 10 kilometers, or perhaps two to three scale heights. I don't think that's a cloud.

The other comment is to Peter. He was saying something to the effect that the cloud particles can't grow unless they're recycling, up and down. But I see layers in those clouds. The Mariner 10 limb photographs show cloud layers at 5 mb. Traub and Carlton's spectroscopic results also imply cloud layers.

DR. GIERASCH: Does that mean they're growing?

DR. BELTON: I don't know whether it means they are growing or not. But recycling certainly can't be going on to the degree you implied by your hand motions.

Peter, you also said something to the effect that the droplets don't have any effect on the H_2O vapor.

DR. GIERASCH: There's more water in vapor form than there is in the droplets.

DR. BELTON: Does this mean the clouds are not drying out the water vapor?

DR. YOUNG: Right. Incidentally, if the cloud bottoms out at the point where the Venera 8 shows a kink in the attenuation of light, where the temperature is about 400 or 450°K and the pressure about 5 atmospheres, that's just where H_2SO_4 will evaporate against the water-vapor pressure if the water-vapor mixing ratio in the lower atmosphere is something like 10^{-4}. So that means that deep in the atmosphere there's quite a lot of water in the vapor phase.

DR. JONES: What altitude are you referring to?

DR. YOUNG: The kink in the Venera 8 transmission curve is at 32-35 km.

DR. JONES: Sulfuric acid can evaporate well above that.

DR. YOUNG: But within a factor of 2, you would have it evaporating at about that pressure level.

DR. ROSSOW: How is that mixing ratio for water derived? Is it derived assuming a constant mixing ratio above the clouds?

DR. YOUNG: Yes, and that's probably why it's off by a factor of 2 because the scale height of the water is probably half the scale height of the atmosphere; but you've got a factor of 10 variation with time. So within the huge temporal variations that occur, you can forget a factor of 2.

DR. ROSSOW: Your comment about the HF chemistry is interesting, and the HF chemistry is probably paralleled by the HCl chemistry as well.

DR. YOUNG: Yes. HF is a lot more reactive. Our chemist tells me that HF reacts with sulfuric acid very quickly and HCl just sort of dissolves in it to a slight extent and doesn't react, at least at low temperatures.

DR. ROSSOW: There are all sorts of compounds you have to look at.

DR. YOUNG: Oh, there's a huge array of compounds. Sulfur and oxygen and halogens just form dozens of compounds among them. It's horrendous to do the chemistry right; it's horrible. In fact, there's one other complication in this. The H_2O variations may be due to the HF variations; if you put more HF in the clouds, that gobbles up the water. When you take the HF out of the clouds you have the water able to evaporate again. So the HF might be negatively correlated with the water. Day-to-day variations in HF are something that ought to be looked for, but it's tough to measure.

DR. JONES: Andy, you did not mention the Venera 8 Doppler wind profile measurements. Does that mean that you don't like them for some reason or you forgot to mention them?

DR. YOUNG: Well, I forgot to mention them, but I also wonder if they are really reliable or not. The thing that makes me a little worried is that in reducing their data they forced the probe to be at rest when it was on the surface, and that means if there was any drift in their oscillator with time, that gives a wind speed that automatically increases with height. I don't think that's what's causing their 100 m/s apparent shift, but the measurement is not unimpeachable.

DR. JONES: Does not the profile actually go down to close to zero at about 20 or 25 kilometers? So it's not a linear drift, if drift is a problem.

Peter, would you explain why the dynamical mixing leads to a rather uniform particle size?

DR. GIERASCH: If particles are brought down to the base of the cloud where coagulation is effective, it is most effective for smaller particles whose number density is larger, that's all. So some of the small ones are lost but nothing happens to the big ones. If that goes on long enough, the sizes will sharpen up.

DR. BARKER: I want to point out that our observations of CO_2 line strengths agreed with Andy's during his period of observation in September 1972, when he saw a four-day period with the same type of variation. We also agreed in two subsequent sets of observations in October and December 1973 which ruled out a four-day oscillation at those times.

DR. YOUNG: That's another manifestation of the long-term changes in the weather.

DR. WALKER: Are you saying there is not a higher mixing ratio of water in the lower atmosphere than in the upper atmosphere?

DR. YOUNG: It's a little higher, but it's probably only 10^{-4} in the lower atmosphere.

DR. WALKER: What do the microwave observations say about that?

DR. YOUNG: There's no problem, because the microwave people set upper limits, and their upper limits are like 2×10^{-3}.

DR. HUNTEN: I would like to be sure about the assertion that there is

10 times more water vapor than water in the cloud particles. This is obtained by comparing two very uncertain numbers, and your ratio is only a factor of 10. Let's not go away from here happy that such is really the case for the Venus stratosphere.

DR. YOUNG: Would you believe that it is at least comparable?

DR. GIERASCH: I think I said, as far as the kinds of things I was trying to do, all that matters is that within a factor of 10 they are about the same. I assumed they were about the same. I'm sure you can't get the exact numbers, because you don't know the particle scale height.

DR. YOUNG: And you don't know the water vapor scale height.

DR. GIERASCH: No, but you do know from the 10-second condensation time constant that the water vapor is in equilibrium.

DR. YOUNG: It has to be in equilibrium, that's right.

I might add one other thing. If you look at how the composition of the droplets changes as they move up and down over a scale height, you find that the changes are teeny-tiny. The changes over a scale height are like .002 in refractive index, and the radius changes something like 3.5 percent. So the narrowness of the size distribution and the well-determined refractive index coming out of polarization data are perfectly reasonable. The natural vertical inhomogeneity of the atmosphere is much smaller than the width of the inferred distributions. So that's not what we're seeing.

DR. JONES: Dr. Young made a strong point about the low error bar on the French spectroscopic factor. If you look at the original data very carefully, you will find that a much higher scatter is consistent with what they are measuring.

DR. YOUNG: I agree that those measurements are very shaky, because this 10 m/s probable error for their final result corresponds, on the photographic plates that they use, to a 30th of a μm. That means that on the average they measured line positions to a 30th of a μm. I find it hard to believe that you can achieve that kind of systematic accuracy.

DR. DOLLFUS: One of them was made with the --

DR. YOUNG: With the Fabry-Perot crossed with the spectrograph. So what essentially happens is that the Fabry-Perot provides the reference lines, if you will, against which to measure the position of the solar lines reflected from Venus.

Different people have done this in different ways. Slipher used an iron arc for comparison. Everybody knows that that's subject to horrible systematic errors. Richardson did much better. He used telluric absorptions around 6300 Å, and compared terrestrial absorption lines with the solar absorption lines reflected from Venus. But in the French work, we're back to a laboratory standard, where the Fabry-Perot étalon sets the wavelength reference. And as Don Hunten pointed out, as the temperature changes and the instrument drifts, the reference position for those lines changes quite a bit. So you have to take out this big drift in the instrumental zero point. They did this by fitting parabolic curves or linear fits through their data, but there's a big effect that has to be taken out. Because Richardson used five times higher spectrographic dispersion and compared things that are directly comparable, namely, different kinds of absorption lines on the same spectrum, rather than trying to match up the Fabry-Perot rings with the absorption lines, I have a lot more confidence in Richardson's measurements.

DR. BELTON: I just want to point out, Andy, the 30th of a µm just cannot be relevant. What they do is measure the position of the crossing of a fringe along the length of the line rather than measuring in the direction of the dispersion.

DR. YOUNG: It represents a 30th of a µm error in measuring that position, on the average.

DR. BELTON: That's an irrelevant number.

DR. POLLACK: Peter, I think that there's one other time constant that's worth taking account of. That, in effect, would be the photochemical time constant required to form material in the first place. And from laboratory work that has been done, mostly directed towards sulfuric acid in the earth's atmosphere, it seems as if the limiting step is conversion of SO_2 to SO_3. Time constants that are common in the literature are something like a year. So that's a tremendously long time constant, and it may even make greater complications in the case of these clouds.

DR. GIERASCH: Is that consistent with what you've published, Ron?

DR. PRINN: No.

DR. GIERASCH: It all depends on what reactions you are using.

DR. POLLACK: My comment is based on two things. First, the laboratory measurements to determine rate constants, and it is a very difficult experiment, typically yield time constants of one year. Second, there is some evidence that the sulfuric acid in the earth's stratosphere consists of volcanic injection, and that there is about a year time delay between injection and the sulfate maximum.

DR. HUNTEN: The Venus chemistry doesn't look much like that of the earth, so I don't see why that should be relevant at all.

DR. JONES: Where does your number 10^{-4} for the water vapor in the lower atmosphere come from?

DR. YOUNG: That comes from the vapor pressure of H_2O above sulfuric acid as a function of temperature. The constant boiling mixture is 98.3 percent sulfuric acid. If you now draw on the pressure-temperature diagram lines of constant mixing ratio of H_2O in the Venus atmosphere -- in other words, the P-T profile scaled down by whatever the fractional mixing ratio is -- you get lines that cross the vapor-pressure curve. When you look at the temperature level at which the Russians claim the bottom of the cloud occurs, that comes right around a 10^{-4} water vapor mixing ratio.

That's my basis for saying if you believe the bottom of the cloud is where the Russians say it is, and if you believe the clouds are made out of sulfuric acid, if you ignore the effects of HF which ought to have boiled out, and HSO_3F which also ought to have boiled out, and just consider the sulfuric acid and water system, then that intersection tells you that there's about 10^{-4} mixing ratio of water vapor in the lower atmosphere.

DR. JONES: What happens further down toward the surface?

DR. YOUNG: The clouds would evaporate. This intersection is the point at which the sulfuric acid and the water have equal vapor pressures. As you go hotter than this, then the sulfuric acid evaporates and the particles disappear.

DR. JONES: Isn't this inconsistent with the Venera measurements of about 10^{-3} water vapor mixing ratio?

DR. YOUNG: They get different numbers on different experiments.
They get numbers that go all over the place. So I don't know which to
believe.

DR. GIERASCH: I have a comment with regard to Jim Pollack's question.
It is a crucial question for the dynamics of this cloud whether the photo-
chemistry generates sulfuric acid only as a result of a relatively slow
leakage of particles out the bottom of the clouds before they get destroyed
or whether the photochemistry has the same time constant as everything else
that's going on, which would be the case, for example, if particles were
formed at the top, and then fell right down through and were evaporated and
the vapor came back up. In that case, the time constant of the photo-
chemistry has got to be the same as the dynamical time. And that's impor-
tant. It would be much easier to understand things if the particles
repeatedly cycled in the cloud and thus had a lifetime much longer than
the dynamical time constant.

DR. YOUNG: There are two ways of producing a constant mixing ratio
for the sulfuric acid. One is to have the atmosphere sufficiently turbu-
lent that the mixing has everything homogenized. In that case a short
photochemistry time scale is needed to regenerate the stuff because it gets
cooked apart: When the H_2SO_4 evaporates, it breaks up into water and SO_3.

The other way of making a constant mixing ratio is to form this stuff
way up at the top and then let it percolate down, because at the 50 milli-
bar level the mean-free-path of CO_2 is something like a quarter of a micron,
which is not very much smaller, as Peter pointed out, than the radius of
the droplets. So above this reference level this stuff is in free molecu-
lar flow, and that automatically gives you a constant mixing ratio, if you
just shake the stuff in at the top.

DR. GIERASCH: But I don't like the eddy thing. Prinn has done rele-
vant calculations, with eddy diffusivity, chemistry on the top, and des-
truction on the bottom. The problem is in getting particles of 1 μm size,
unless somehow the chemical rates will do it for you up at the top. The
chemistry is very nice for that kind of model. Gas goes up and the par-
ticles go down in this diffusion. But it's hard to explain the particle
sizes, it seems to me.

DR. PRINN: Let me add one more thing about lifetimes. SO_2 must have
a very short lifetime or we would observe it. It would have concentrations
comparable to what you want in the sulfuric acid.

DR. YOUNG: SO_2 has a mixing ratio upper limit of 10^{-8}. It's the
critical one, with the low upper limit.

DR. GIERASCH: Ron, you said something about the dynamical time
constant.

DR. PRINN: I'm saying that the oxidation time of SO_2 must be a lot
smaller than the "dynamical" time constant for replenishment of SO_2 by
bringing up COS from the bottom and oxidizing it to SO_2. I think we are
talking about different time constants. I'm talking about time constants
for molecules at the top. You're talking about time constants for complete
production of the cloud column. That's a very different beast than my time
constant.

DR. YOUNG: If you are comparing these two numbers, there are at least
several hundred, let's say a thousand, times as many sulfur atoms in sul-
furic acid as there are in SO_2. And that means how do you get the SO_2 up
sufficiently slowly and oxidize it sufficiently fast that only about a
tenth of a percent of it remains as SO_2. You've got to really use it up

fast or it would get up to where you'd see it.

DR. STONE: Peter raised the question of the dynamical time scales. It is very difficult, I think, to assign a dynamical time scale for the upper atmosphere. Perhaps you can put some reasonable upper bounds on it. You quoted a time scale of 10^7 seconds, which is really a sort of over- turning time for the deep atmosphere. That is one time scale that could appear in the upper atmosphere. But for the upper atmosphere itself there are very different values of the parameters, so that value may not at all be the right time scale.

DR. GIERASCH: I hope it's shorter.

DR. STONE: Let me say what I think the reasonable bounds are. The simplest estimate would be the four-day rotation, 10^5 seconds, which is a lot shorter than the overturning time for the deep atmosphere. But that may be too simple-minded. You really want the time scale associated with the overturning in the upper atmosphere, and it's not clear that that's the same as the time scale implied by the zonal velocity. And I agree with Andy that there are probably variations in the velocities. So, for the larger-scale motions in the upper atmosphere, I think you could put a range on the time scales from 10^5 to 10^7 seconds.

As far as the small-scale motions are concerned, for the very stable lapse rates that probably exist in the upper atmosphere the most probable kind of instability, it seems to me, is John Hart's finger instability, which he described for the overturning that might occur on Venus [J. Atmos. Sci. 29, 687, 1972]. It's best to take the time scale for that -- it's around five to ten days -- as characteristic of the small scale motions. So take your choice, anywhere from 10^5 to 10^7 seconds seems plausible at this point.

DR. SAGAN: Your comparisons of the oxygen carbon monoxide mixing ratio seem to imply a molecular oxygen sink on Venus. Where do you think it is?

DR. YOUNG: Well, maybe it's the sulfuric acid. Maybe the oxygen gets used up to oxidize SO_2 and makes sulfuric acid in the upper atmos- phere. Then the problem is what happens to the stuff when it goes down into the deeper atmosphere where it's hot. The H_2SO_4 breaks up into H_2O and SO_3, when it boils down there. And the SO_3, of course, should react with other stuff in the lower atmosphere.

On the other hand, some of this is going into HSO_3F, as I pointed out, and this is a very stable molecule. It doesn't decompose in the laboratory up to 900°C. So that may be where some of the sulfur and the oxygen reside even down near the surface of Venus. But there is such a whole spectrum of sulfur and oxygen and chlorine and fluorine molecules that the chemistry in this system is very, very complicated, even without putting in the photo- chemistry in which you don't know all the reaction rates. So, what a mess.

DR. SAGAN: What about the crust as an oxygen sink?

DR. YOUNG: It could be, yes.

DR. PRINN: The obvious thing that happens to SO_3 is it oxidizes carbon monoxide in the lower atmosphere.

DR. YOUNG: And makes COS, or SO_2, yes.

DR. PRINN: You don't have to have a sink.

DR. YOUNG: Right. The oxygen could just be in different forms at

different levels in the atmosphere, depending on the pressure, temperature and light flux at that level.

DR. WALKER: The fact that oxygen and carbon monoxide are being produced by the dissociation of CO_2 does not mean that they have to have equal densities in photochemical equilibrium. The different densities provide no evidence for a current oxygen sink other than the recombination of carbon monoxide and oxygen. I agree that sometime in the distant past a small amount of oxygen has disappeared. That is what this observation indicates. But it certainly doesn't indicate any mysterious extra sink for oxygen right now.

DR. HANSEN: I'd like to comment on Mike Belton's earlier comment. The numbers which we give for the particles lead to a mean-free-path of about 5 kilometers at 50 millibars. That's a horizontal visibility of about 20 kilometers so, indeed, that's not a cloud like those we know on earth. But it's a matter of semantics. I think it's okay if we call it a cloud, because if you take an image and enhance it, it looks like clouds. And these particles with refractive index 1.44 are the visible layer. This stuff really is what we've called the Venus clouds for 50 years, so it's a little difficult to change it now.

DR. HUNTEN: This stuff is actually more transparent than the air outside that window.

DR. JONES: It still looks like clouds.

DR. BELTON: In such a thin cloud or haze, how are you going to get order of magnitude variabilities?

DR. HANSEN: In the number density? I don't think there are.

DR. BELTON: I mean in the water vapor.

DR. HANSEN: Yes, that's an unsolved problem. But my point is that these particles we are talking about are the visible clouds, not a thin haze layer above the clouds.

VENUS CLOUD MODELS

Steven Wofsy, Harvard University

Most of the material that I'm going to talk about was prepared by Nien Dak Sze and I for the Leige Conference, McCormac's Summer Study Institute on the Physics and Chemistry of Atmospheres. It is mainly in the nature of review material, but it's material which hasn't really been discussed in the context of Venus.

What I'm going to do in the first few minutes is try to move away from the simple ideas that Peter Gierasch started off with, which are so useful for getting a broad picture of what is going on. I'm going to try to debunk the idea of a reference level where all cloud phenomena can be re-ferred to. Figure 1 shows why I tried to do that.

For Figure 1 I've assumed that the particles are 1 μm in radius and that they're a 75 percent sulfuric acid solution. So the optical depth at any level is about 1.5×10^{-22} over the particle radius times the mixing ratio of sulfuric acid in the gas phase, times the column number density of CO_2. That's just a fast way of getting some idea of what the optical depth would be at some level if the mixing ratio of sulfuric acid is one of the indicated values.

I've also put on Figure 1 some experimental data. It's the best we can do to obtain the optical depth as a function of altitude. The point at the highest altitude is one a lot of people seem to forget about. It was obtained in Goody's analysis [Planet. Space Sci. 15, 1817, 1967] of old
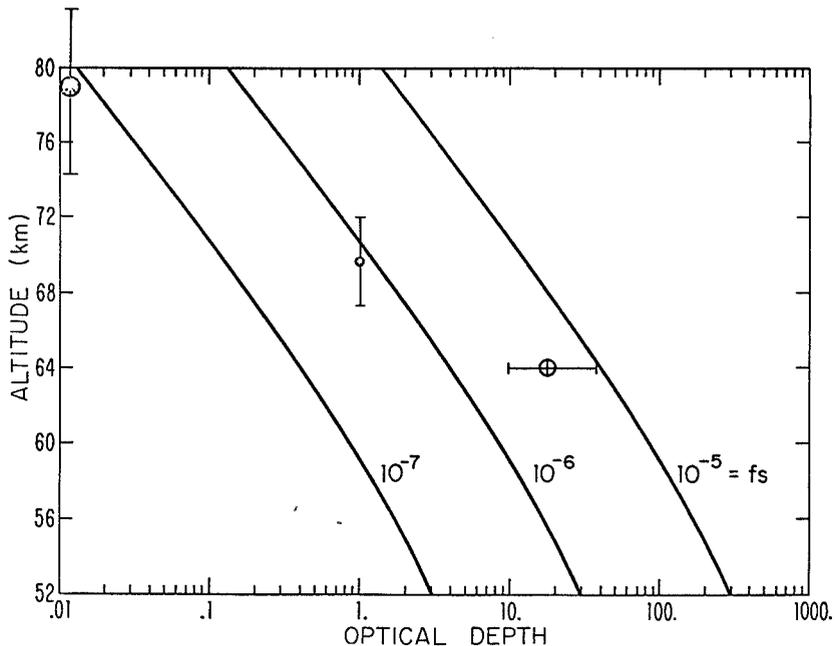
Fig. 1. *Optical depth as a function of height, for particles of radius 1 μm, with various mixing ratios (fs, v/v) of gaseous H_2SO_4 condensed into the cloud particles. The upper experimental point is from Goody [Planet. Space Sci. 15, 1817, 1967], the middle point is from Hansen and Hovenier [J. Atmos. Sci. 31, 1137, 1974], and the lowest point from Belton et al. [The Atmospheres of Mars and Venus, Gordon and Breach, 1968].*

solar transit data, and it gives an optical depth of about .01 at 78 km. One thing I'd like to point out is that at 78 kilometers the temperature is probably less than 200°K, which is very cold for a sulfuric acid and water solution.

The second point is from a paper by Hansen and Hovenier [J. Atmos. Sci. 31, 1137, 1974], in which they estimate that the optical depth of 1 is at 50±25 mb. The point I would like to make there is that, if you believe the radio occultation measurements, the temperature at 50 mb is about 230°K. And that's a good deal colder from the vapor pressure standpoint than 250°K.

The third point is an estimate for the optical depth where the infrared lines are formed. This is from a paper by Gierasch and Goody [J. Atmos. Sci. 27, 224, 1970] who say that the optical depth is 20 there; the pressure I get is 150 or 100 mb and the temperature is about 250°K.

So you can see where the cloud particles are known to exist from experimental data. It's a rather large height range, from 80 km down to about 64 or 62 km. And the temperature and pressure conditions vary considerably over that range. Now the rest of the talk is going to be a discussion of how the physical chemistry of H_2SO_4-H_2O particles makes it rather difficult to have the clouds made out of only one kind of particle.

Figure 2 is fairly complicated, but also rather important. On the y-axis is the water-vapor pressure in millimeters of mercury and on the x-axis is the temperature. The freezing point curve is drawn in the figure. Because the mixing ratio of water vapor is not known, three different values have been assumed, 10^{-4}, 10^{-5} and 10^{-6}. The triangles are the conditions on Venus at 68 km, 64 km, 60 km, etc.

Now, I think the first thing you notice is that the sulfuric acid particles will freeze if the water-vapor mixing ratio is less than about 5×10^{-5} at the level where Hansen claims there are spherical particles. So that's a problem. I should point out that frozen particles are not necessarily inconsistent with the data. The only thing the data tells you is that the particles are spherical and have an index of refraction of about 1.44. And Andy Young made the point that the various hydrates of sulfuric acid are kind of a mess when they freeze, so they might well freeze
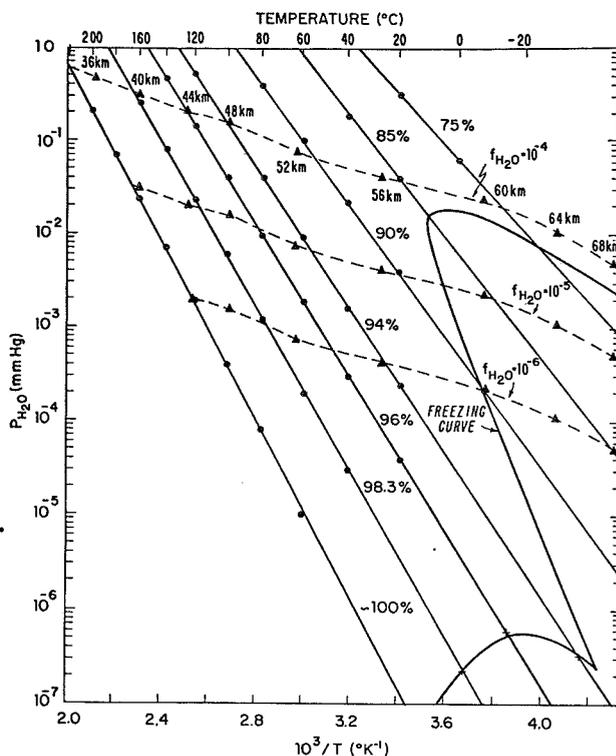
Fig. 2. *Equilibrium vapor pressure of water for various concentration of H_2SO_4 (wt %) in water, as a function of temperature. The data is from Luchinski [J. Fiz. Khim. 30, 1208, 1956] and Timmermans [The Physico-Chemical Constants of Binary Mixtures in Concentrated Solution, Interscience, 1960]. The freezing point curve according to Pickering [J. Chem. Soc., 331, 1890] is shown by the heavy line. Conditions on Venus are shown, by the dotted lines, using the NASA atmosphere [Models of the Venus Atmosphere, SP-8077, 1972] for H_2O mixing ratios of 10^{-4}, 10^{-5}, and 10^{-6}. The triangles (∆) designate the given altitudes on Venus.*
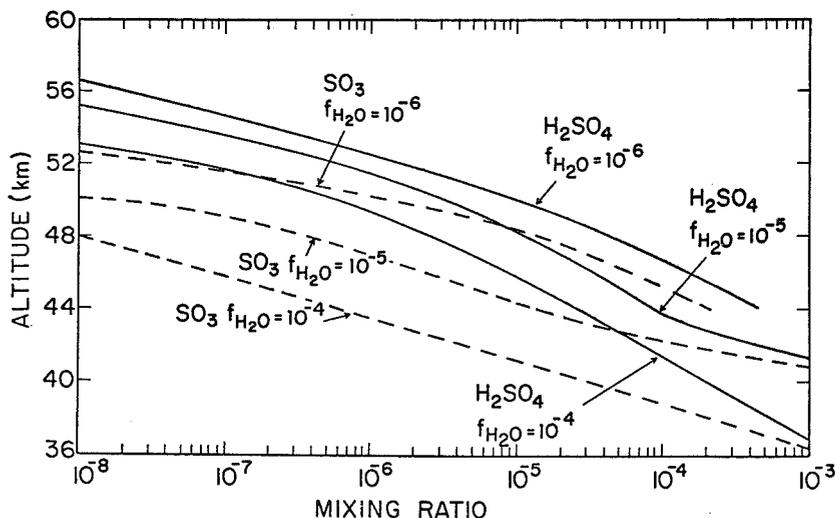
Fig. 3. *Equilibrium vapor pressures of H_2SO_4 and SO_3 (expressed as mixing ratios) as a function of altitude in the Venus atmosphere. Profiles are for assumed background H_2O abundances of 10^{-4}, 10^{-5}, and 10^{-6} mixing ratio (v/v).*

either to a glass or to a crystal and could well be spherical.

In any case, I'm going to make the point very strongly that a water-vapor mixing ratio less than about 5×10^{-5} in the absence of permanent supercooling is not consistent with liquid sulfuric acid water droplets.

There doesn't seem to be much in the literature about the vapor pressure of sulfuric acid in SO_3 over sulfuric acid water droplets. But there is a nice review in the literature [Luchinski, G. P., J. Fiz. Khim. 30, 1208, 1956], in Russian, from which I have taken the H_2SO_4 and SO_3 values given there for values down to 0°C, extrapolated them on the same kind of 1/T plot, and applied them to the conditions on Venus.

Figure 3 shows the results. These altitudes are rather deep in the atmosphere, and we don't even know what cloud material might be there. But Andy Young has suggested that there could be some sulfuric acid clouds down that deep. Let's see what kind of mixing ratios of sulfuric acid vapor and SO_3 vapor we would need to keep a cloud down there.

As you can see, the temperature sensitivity of the vapor pressure is very large, and the temperature is rather warm when you get down to 50 km. So the result is rather huge mixing ratios of total sulfuric acid in the atmosphere. You can see that changing the water-vapor mixing ratio by two orders of magnitude moves the curve for the mixing ratio of sulfuric acid over the droplets in the atmosphere by less than a scale height.

I like to consider Figure 3 as showing where the level of saturation is for given mixing ratios of sulfuric acid and SO_3 on the planet, and that reveals the lowest location of the cloud bottom for that mixing ratio of sulfuric acid in the gas phase. For a dry atmosphere (10^{-6} mixing ratio of water vapor) and a crude estimate of 5×10^{-5} for the abundance of gaseous sulfur the cloud bottom is above 50 km. It is around 49 or 48 µm for the wetter atmosphere.. My opinion is that these curves pretty definitely exclude the possibility of a sulfuric acid cloud going much lower than roughly 50 km.

Now, the caveat is that it could be very wet at great depths. If the water-vapor mixing ratio is 10^{-3}, the cloud bottom could be lower. But I

think that's a fairly big "if". And it's a fact of physical chemistry that if you're going to make the clouds that low you've got to have a very wet lower atmosphere. My personal feeling is that it probably isn't that wet and the cloud probably doesn't go below about 50 km.

One other point I'd like to make is that a lot of SO_3 is generated deep in the cloud. And SO_3 is fairly nasty stuff in a reducing atmosphere. At the relevant temperatures and pressures it seems pretty unlikely that that amount of SO_3 would be retained. It should oxidize some of the reducing specie, perhaps as a heterogeneous reaction, and it should be converted to SO_2.

Now we come back to the problem, "Well, if there's so much SO_2, why don't we see it?" I think that's a fairly serious problem if you want the cloud bottom to extend down fairly low. If you keep the cloud bottom high, however, you don't get very much SO_2. So a good way to get out of that problem is to not have the sulfuric acid cloud bottom go down very low.

Fig. 4. Solution composition profiles for Venus with assumed H_2O abundances 10^{-4}, 10^{-5}, and 10^{-6}. The regions where pure solutions could freeze are shown by the hatched areas. This figure is constructed using the NASA (1972) atmosphere.

Figure 4 is a summary of the previous two slides, with solution composition graphed as a function of altitude. It shows again that the pure solutions will freeze well below the $\tau = 1$ level at 68 km. It also shows that if the clouds extend very deep, the composition of the particles varies significantly with height. That's perhaps obvious, but it is a point which is frequently overlooked when a cloud with a single temperature and a single pressure is considered.

There is another way, besides supercooling, to have liquid cloud particles, and that is to have impurities in the particles which lower the freezing point. We know that if the particles are made of sulfuric acid they better have some impurities to change their spectroscopic characteristics, but whether or not they could change the freezing temperature by 10 degrees is another question because that would require quite a lot of impurities.

The last point I want to make concerns the relationship between photochemistry and time constants. The only model for cloud formation that we have is due to Professor Prinn, and his model requires that the atmosphere somehow supplies three-halves of an O_2 molecule every time it oxidizes the sulfur. Assuming that the lower atmosphere is reducing, it's very hard for it to supply any oxygen to the cloud. So the obvious place where oxygen might be obtained is from the upper atmosphere, where photolysis of CO_2 is taking place.

Now, there's a strict limit on how much oxygen photolysis of CO_2 can produce. It is determined by the number of solar photons that can
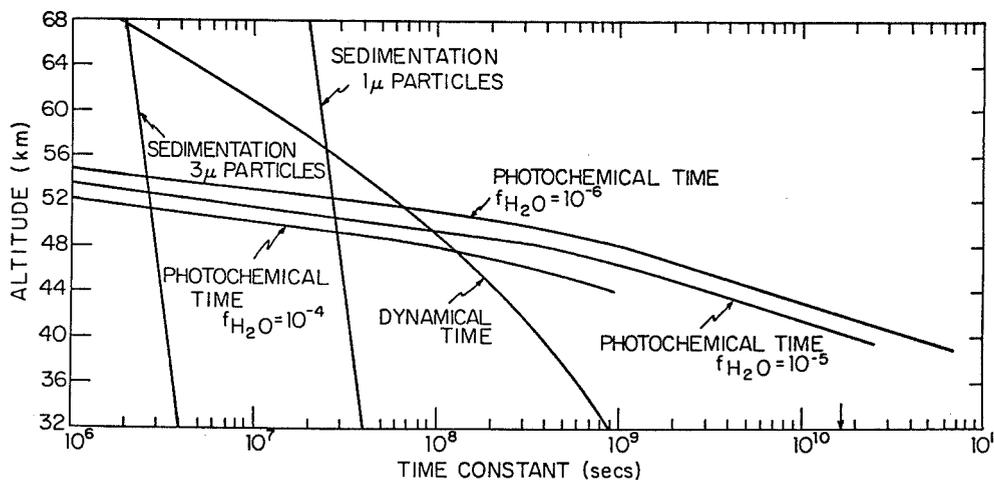
Fig. 5. Time constant profiles versus cloud bottom altitude, for cloud-related processes on Venus. The photochemical time is derived by assuming a well-mixed cloud above the level of saturation. The photochemical time is given by the cloud H_2SO_4 content (cm^{-2}) divided by the flux of O_2 into the cloud ($\sim 10^{12}$ cm^{-2} sec^{-1}). The sedimentation time is calculated from the formula given by Byers [Elements of Cloud Physics, University of Chicago Press, 1965] assuming viscous flow. The "dynamical" time is a rough estimate of the transport time for large scale motions; the curve shown is the time required to heat the atmosphere to the ambient temperature using 5% of the solar constant.

photodissociate CO_2, which is about 10^{13} cm^{-2} sec^{-1}. There have been a number of aeronomic models in the literature in which the amount of O_2 that could be released down into the cloud has been calculated, although usually the calculation is done in reverse. The desire is to get rid of all the O_2, to explain why it's not there. But if you turn off all of the recombination mechanisms in the lower part of the stratosphere, and you just have various processes that recombine O with CO in the upper part of the atmosphere, the most you can get out of the aeronomic models that I've seen is about 3×10^{12} cm^{-2} sec^{-1}.

By taking that sort of number we can estimate how long it will take to photochemically generate the whole cloud. By taking a fairly dry atmosphere, in which the cloud bottom is high, the time obtained is a lower limit.

Figure 5 shows the result. There are three curves for the photochemical time constant, corresponding to the three water-vapor mixing ratios. Where these curves should be terminated depends on the mixing ratio of sulfur, but it's pretty clear that it doesn't depend very strongly on the water-vapor mixing ratio. For a 2×10^{-5} sulfur mixing ratio the cloud production time is 10^7 seconds or longer. That is to be distinguished from the time that it takes to convert sulfur into sulfuric acid, which is presumably much shorter. If that's the case, then, as discussed by Peter Gierasch earlier this morning, the sulfuric acid droplets must be relatively well-conserved in the atmosphere; they would have to be recycled several times before being destroyed, if the cloud is to reach down to 50 km.

Figure 5 also shows sedimentation time constants. The time constant of about 10^7 seconds is larger than the time constants for coagulation, so the size distribution should not be maintained down to 52 km. It should be affected by the coagulation process.

DR. WALKER: It isn't necessary to oxidize sulfur for every traversal

of the cloud is it? If the particles evaporate the sulfur is still oxidized, and it comes back up and condenses again. You just have to oxidize it once, perhaps four billion years ago.

DR. WOFSY: Yes, that's a good point. I don't think it would have an infinite lifetime, but it could clearly be several years or something on that order. But the oxidized sulfur would not be chemically stable at the conditions expected in the lower atmosphere. The temperatures and pressures are high enough that there would be a noticeable tendency to move toward chemical equilibrium.

DR. WALKER: It will get reduced in the lower atmosphere again?

DR. WOFSY: If the atmosphere is anything like what people say it is, it definitely will, yes.

DR. POLLACK: I have some comments on your statement that particles may be frozen at the level to which polarization and other measurements refer. First of all, sulfuric acid has an enormous tendency to supercool. In the earth's atmosphere, you can find liquid droplets at temperatures as low as -40°C. So you really have quite a bit of latitude with supercooling.

DR. WOFSY: I'm glad you mentioned that. You're right.

DR. POLLACK: The second thing is that from an observational point of view, I think it's very hard to reconcile the infrared spectrum with sulfuric acid in the solid state, because having something in solid state in general leads to displacement of absorption bands. For example, in the 3 μm region, you get a displacement from 3.3 μm to 3.8 μm. And I rather suspect you would get rather noticeable shifts in the 8 to 13 μm region where liquid sulfuric acid fits the observations quite well. So my feeling is that the infrared spectrum is not consistent with solid state and gases.

DR. WOFSY: Actually I didn't mean to make a strong proposal that the particles were frozen. That's only an outside possibility. I think it's more likely that either, as you say, they are supercooled, or there's an impurity that lowers the freezing point.

The point I really wanted to make strongly was that we do have a bit of a problem with a very dry atmosphere in keeping those particles as far down as people want them.

DR. YOUNG: I disagree with you 100 percent.

INFRARED REFLECTIVITY AND CLOUD COMPOSITION

James Pollack and Edwin Erickson, Ames Research Center

The presentation by Pollack is largely contained in the paper by Pollack et al. which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

We summarize the evidence showing that the first optical depth of the Venus cloud layer is composed of a water solution of sulfuric acid, including our earlier aircraft observations of Venus' reflectivity in the 1-4 micron region obtained at a phase angle of 120° (Pollack et al., 1973 and 1974). Analyses of these aircraft results indicated that of all the proposed cloud candidates only a sulfuric acid solution with a concentration of 75% or more H_2SO_4 by weight was consistent with the observed 3 micron cloud feature. We present new aircraft observations of Venus obtained in the 1 to 4 micron region at a phase angle of 40° and in the 3-6 micron region at a phase angle of 136°. Comparing the two sets of observations in the 1-4 micron region, we find a striking phase effect: the reflectivity is much lower in the 3 micron region and there is a much more marked decline between 1.3 and 2.5 microns for the data obtained at the smaller phase angle. The observations made at the 40° phase angle are consistent with the theoretical behavior of a sulfuric acid cloud and imply that the sulfuric acid is present to at least many tens of optical depth below the cloud tops. Arguments concerning the concentration of the solution are reviewed and we conclude that the best current estimate is about 85% H_2SO_4 by weight.

DR. JONES: The theoretical reflectivity for 75 percent sulfuric acid looks higher than the observations at about 2.7 μm. Is that due to CO_2 absorption?

DR. POLLACK: Yes. I should have mentioned there's a fundamental of CO_2 at 2.7 μm.

DR. SAGAN: Why does 75 percent sulfuric acid solution absorb more than 95 percent solution?

DR. POLLACK: What is happening is that for 75 percent solution the band is smeared out, and so in effect it's a broader absorption. With 95 percent, it's actually deeper at 3.3 μm but then comes up because there's not as much smearing; in other words, there's not as much H_3O plus the sulfuric ions to smooth it out.

DR. BELTON: Are the Regas absorption line calculations fit to CO_2 or water or what?

DR. POLLACK: They're fit to both the CO_2 and certain water vapor lines. The reason I mention them is that Jim went through a fairly sophisticated multiple scattering model with line formation which involved a better analysis than some people had done in the past, so to my mind it's the most sophisticated.

DR. HANSEN: I have a question and a comment. The question is a little nasty. A few years ago you fit the same type of data, infrared reflectivities, to theoretical results and got fits with water ice that were even more impressive than those shown today. Is there any reason why we should believe you any more now than we should have believed you then?

DR. POLLACK: I think the answer really is a matter of the data that we had.   That's why I felt motivated to go out and actually collect some new data from the aircraft.   The things that were missing at the time we made the ice cloud calculations were, number one, we didn't have a good estimate as to what the drop in reflectivity was between 2.5 μm and a little bit longer than 3 μm.   The data were very noisy in that region.   So all you could say was that it dropped considerably.   A very important aspect in what we've done is to get an estimate as to what that drop is.

The second point also concerns an insufficiency of the data, namely that the available observations only went out to about 3.3 μm.   Out to that wavelength region water ice is, in fact, a very absorbing material, as you can see from the calculations I presented.   But the reflectivity of water climbs very quickly beyond 3.3 μm.   So the wavelength extension of our results was also very important.

DR. HANSEN: My comment is: it seems to be taken for granted here today that the composition is sulfuric acid.   I think that question is still open.   Polarization only tells you the refractive index.   The other two significant pieces of information, I think, are your near-infrared observations and the thermal infrared observations.   But I don't think either is definitely conclusive for the composition; for example HCl seems to fit your data almost as well as sulfuric acid.   So it's not just a question of what is the sulfuric acid concentration.

DR. POLLACK: Let me make several remarks on that.

First of all, I don't think there is anything more dangerous in science than having a bandwagon.   So I think Jim's remark is very apropos.   That's why we're trying to get more data ourselves, because we want to be really sure this time.   And that's also the reason that we went to such great troubles to try to match a whole variety of things rather than say, "Eureka, sulfuric acid fits it," and leave it at that.

On the question of HCl, I don't agree that HCl fits it as well.   HCl goes a factor of 2 below the observations near 3 μm, and a factor of 2 above them between 3.3 and 3.6 μm, whereas H_2SO_4 fits the data quite well in both regions.

And secondly, we're going to do some reflectivity calculations for our smaller phase angle information.   I suspect there will be a very big pickup for HCl beyond 3.3 μm, which will show up much more dramatically at the smaller phase angles.   This would enhance the validity of excluding HCl.

DR. YOUNG: I agree with you that the composition can't be HCl.   You can't get the right refractive index with HCl unless you raise the concentration of the HCl to enormously unacceptable levels.   You would then have about 10^4 times more HCl in the vapor phase than is actually observed.

The comment I want to make to you is that it's a little dangerous to use one point at 120° phase angle taken one year, and another point at 40° phase angle taken another year, and compare them and say, "Well, there's a difference, and this is due to that and the other," because we know there are long-term changes that take place from one year to another in the structure of the clouds.   We just may be seeing some long-term weather effect there rather than seeing genuine phase effects.

So I don't trust any kind of phase effect or anything like that which takes a long time to observe unless you've observed it at several different seasons and really confirmed that it's a real phase effect and not a seasonal effect or some long-term weather phenomenon.

DR. POLLACK: I think that certainly is an appropriate remark. I should say that we are going to continue to take data here. We don't think the game is over yet.

DR. HAPKE: The size parameter is close to unity at 4 μm wavelength. Did you use a Mie theory calculation for those curves?

DR. POLLACK: Certainly. I might also say that if you change the size parameter to make it smaller, in the hope of getting an absorption feature just because of that effect, you obtain much too gradual a decline. We did that for mercury and it just came down too slowly.

MICROWAVE BOUNDARY CONDITIONS ON THE CLOUDS

William Rossow, Cornell University

The presentation by Rossow is largely contained in the paper by Rossow and Sagan which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The dielectric properties of H_2O and H_2SO_4 at microwave frequencies have been calculated from the Debye equations. The derived frequency and temperature dependence agrees well with existing data. The dielectric properties of H_2O/H_2SO_4 mixtures are deduced and, for a well-mixed atmosphere, the structure of H_2O and H_2O/H_2SO_4 clouds is calculated. With the COSPAR model atmosphere and the calculated cloud models, the microwave properties of the atmosphere and clouds are determined. The 3.8 cm radar reflectivity of the planet, the Mariner 5 S-band occultation profile, and the passive microwave emission spectrum of the planet together set an upper limit on the mixing ratio by number of H_2O ~10^{-3} in the lower Venus atmosphere, and of H_2SO_4 ~10^{-5}. The polarization value of the real part of the refractive index of the clouds, the spectroscopic limits on the abundance of water vapor above the clouds, and the microwave data together set corresponding upper limits on H_2O of ~2×10^{-4} and on H_2SO_4 of ~9×10^{-6}. Upper limits on the surface density of total cloud constituents and of cloud liquid water are, respectively, ~0.1 gm cm^{-2} and ~0.01 gm cm^{-2}. The infrared opacities of 90 bars of CO_2, together with the derived upper limits to the amounts of water vapor and liquid H_2O/H_2SO_4, may be sufficient to explain the high surface temperatures through the greenhouse effect.

DR. ANDY YOUNG: The only thing that worries me about your interpolation between water and sulfuric acid is that the monohydrates and dihydrates and so on are distinct species that have their own characteristic absorption. There's a great deal of information about the DC conductivity and low frequency conductivity of various sulfuric acid solutions at various temperatures, above and below room temperature. Can you put those in and use them as a guide, and how well do they agree with your techniques?

DR. ROSSOW: I've looked at that. But it's DC and I need the AC. All I can say is that the parameters that went into my AC calculations are consistent with the DC numbers.

DR. YOUNG: But there is no guarantee that around the monohydrate the behavior is anything like either water or sulfuric acid, because in the monohydrate only 1 percent of the molecules are still water or sulfuric acid molecules. Mostly they're bound up as monohydrates. They have their own characteristic frequencies of absorption.

DR. ROSSOW: Right, but I think that's covered by these two assumptions. If they have their own characteristic frequencies and they're in this range, then they may have a similar kind of dipole behavior which is bracketed by my two assumptions. If their characteristic frequencies are completely out of the range, then I don't care about them.

DR. HUNTEN: I wasn't clear about what parameters you actually preferred.

DR. ROSSOW: That depends on the assumed concentration of the sulfuric acid cloud drops. I can fit the data with several different numbers. So

it's a matter of your prejudices, do you want a wet atmosphere or a dry atmosphere?

DR. HUNTEN: I want your prejudices.

DR. ROSSOW: Well 75 percent concentration of sulfuric acid gives you about 1 to 4 × 10^{-4} for the water vapor, below the cloud. The ratio of water vapor to liquid water is about 2 to 1; Peter Gierasch mentioned earlier something like 10 to 1. The amount of liquid is about 0.1 - 0.2 gm/ cm^2 -- about the right amount for a greenhouse effect.

SHORT-TERM PERIODIC VARIATIONS IN THE POLARIZATION

Edward Bowell, Lowell Observatory

Short-term variations have been noted in CO_2 line strengths, and of course morphological variations in the appearance of the cloud deck have been well documented. But up to now, no one has, to my knowledge, discovered any periodic short-term variations in polarization. There are seasonal variations which have been discussed by Coffeen.

Figure 1 is a composite showing both variations in polarization and in CO_2 line strengths. The CO_2 measurements are by Barker, who will talk about them later. The interval concerned is August to September 1973. The polarization observations were made in September, and the CO_2 measurements in August.

The sine curve should simply be interpreted as a reference curve. It doesn't purport to fit the observations. What it does purport to do is to show that both the CO_2 and polarization variations are in phase and have a common period of something like 5.5 to 6 days.

Let me explain the CO_2 and polarization scales. The peak-to-peak variation in CO_2 line strengths, in August 1973, is about 20%. The peak-to-peak variation in the polarization is something like 5 thousandths (0.5 percent). This is a fairly small number in polarimetric parlance. Observations of the whole disk, as these are, can be made to an accuracy of something like half a thousandth. Therefore, looking for a 1 or 2 thousandths variation is quite a difficult job.

What's causing the variation in polarization? Is it variation in particle size? Is it variation in the variance of the particle size distribution? Is it variation in the thickness of an overlying Rayleigh atmosphere?

Figure 2 is an attempt to show that the variation in the ultraviolet polarization is not coming from changes in refractive index of the particles. The phase angle at the time of the observations was in the range

Normalized relative CO_2 line intensity [○] and u.v. polarization [•] of Venus
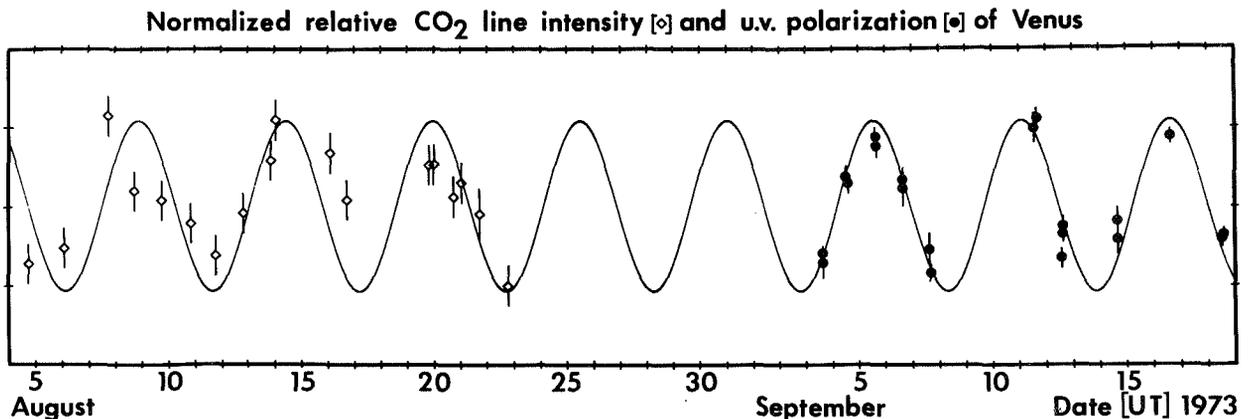
Fig. 1. Short-period variations in ultraviolet polarization (3540 and 3790 Å) and CO_2 line intensity. Ordinate units represent about 0.002 change in polarization and 10% change in CO_2 line intensity.
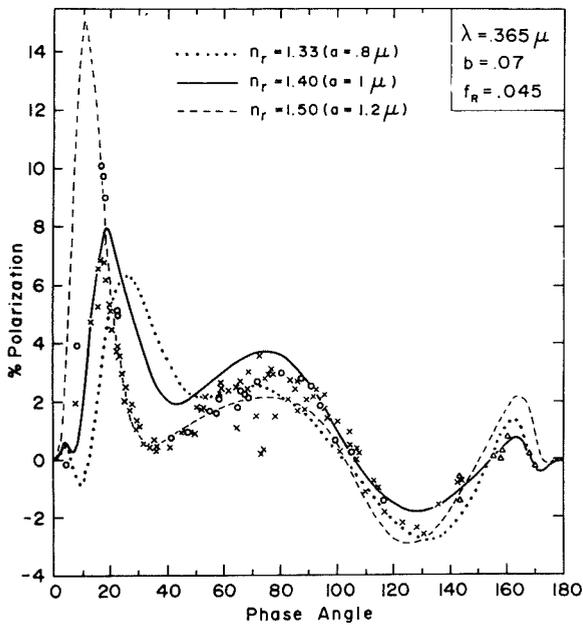
Fig. 2. *Observations and theoretical polarization-phase curves for the integrated disk of Venus. The effect of varying particle refractive index is shown.* [*J. E. Hansen and J. W. Hovenier, J. Atmos. Sci.* 31, 1137, 1974]
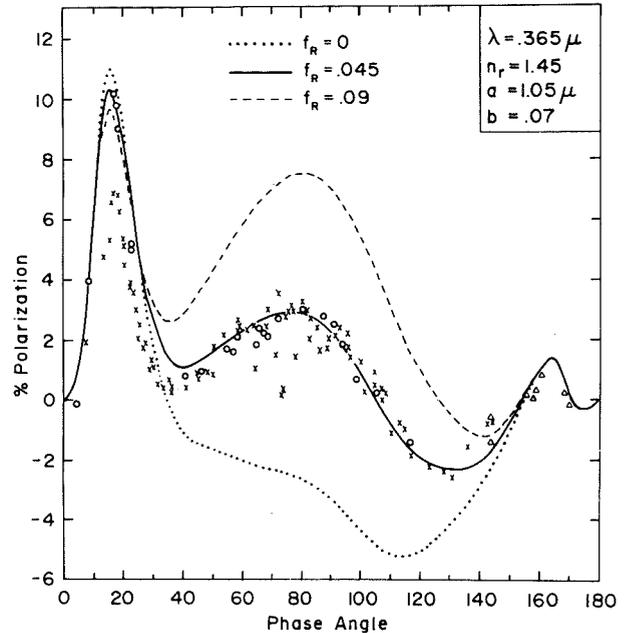
Fig. 3. *Observations and theoretical polarization-phase curves for the integrated disk of Venus, showing the effects of varying f_R, the Rayleigh contribution to the phase matrix.* [*J. E. Hansen and J. W. Hovenier, J. Atmos. Sci.* 31, 1137, 1974]

60 to 80 degrees, and this diagram, from a paper by Hansen and Hovenier, indicates that quite radical changes in refractive index would not change polarization very greatly. One should note that this doesn't preclude very large changes in refractive index causing a variation in polarization. Yet, in all other previous observations no great variation in polarization has been observed at small phase angles where there would be enormous differences in the observed polarization if the refractive index were to change on a short time scale.

Figure 3 is another polarization-phase curve, intended to show that the parameter known as f_R, the contribution of Rayleigh scattering to the phase matrix, could indeed change the polarization drastically over the relevant range of phase angles (60 to 80 degrees). In fact, a change of 0.001 in f_R corresponds to a change of about 0.001 in the polarization at phase angle 70 degrees.

I would suggest that the observed variations in polarization are indicative of changes in the height of the absorbing layer in the cloud and that is reflected in the changes in thickness of the Rayleigh scattering layer above the cloud. To give you some figures: assuming that the top of the absorbing cloud is at the 50-millibar level, which everyone else has done, then a change of 2-1/2 thousandths in the polarization would result from a change of pressure at the level of the cloud top of something like 3 millibars. So this is a change of 3 millibars in 50 millibars occuring planetwide on a time scale of days.

Figure 4 shows the amplitude of variation in polarization (units are thousandths) versus wavelength. It bears a resemblance to a figure by
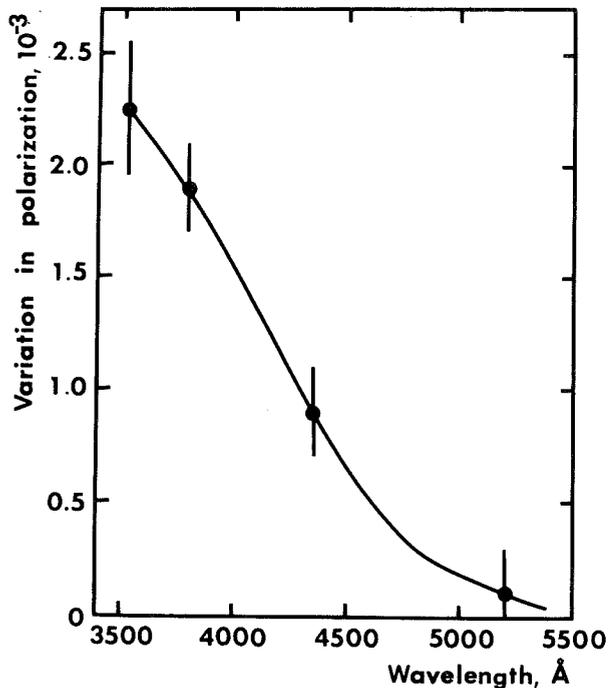
Fig. 4. Amplitude of the short-
term variation in the polarization
of Venus as a function of wave-
length. Observations of September
1973.
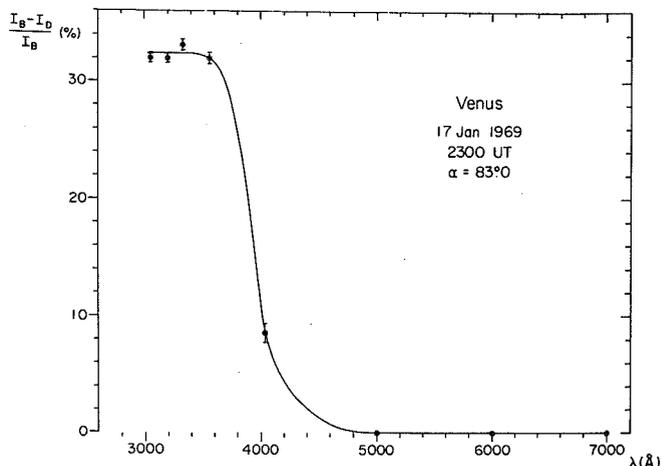
Fig. 5. Venus cloud contrasts as a
function of wavelength [D. L. Coffeen,
in "Planetary Atmospheres," eds. C.
Sagan et al., Reidel, 1971].

Coffeen (see Figure 5) in an article about contrast of ultraviolet
features on Venus. The drop from the ultraviolet to the blue is fairly
steep. In the green I can't readily measure any variation in polarization;
the spot marks the analytical position.

Figure 5 is Coffeen's diagram of contrast versus wavelength. There
is a steeper slope, but it can't readily be compared with the previous
slide of polarization variation as a function of wavelength because
Coffeen's contrast data were derived from a series of scans taken instan-
taneously, as it were, and refer to different regions over the planet,
whereas the variation in polarization is a temporal phenomenon which is
integrated over the whole planetary disk.

There seems, therefore, to be a tie-up between variation in polariza-
tion, between CO_2 line strengths, and if this bad comparison between
contrast and polarization variation is to be believed, between ultraviolet
features and variation in polarization.

I think this goes back to what Andy Young was saying earlier, that
all these different parameters we're measuring, probably referring to the
same 50-millibar level, just have to correlate. The next step will be to
refine all measurements: to link in the polarization and the CO_2 observa-
tions with greater certainty, to link those in turn with the visual
appearance of clouds in the ultraviolet, and eventually to move from a
global to a localized appraisal of these parameters. This is certainly
technically feasible with regard to polarimetry. One can make pretty
accurate local polarization measurements and, with luck, observe changes
in weather on Venus over a time scale of hours (this has already been done
to a limited extent).

DR. KURIYAN: Could you tell us how you measure the polarization to this accuracy?

DR. BOWELL: Dr. Dollfus could best answer that. It was his polarimeter. A half-wave plate rotates in front of an analyzer, usually. One measures the modulation of the output signal, and this is a measure of the polarization. You must have a fairly instantaneous method of measuring the change of the polarized signal.

DR. KURIYAN: And you could get it to an accuracy of .01 percent?

DR. BOWELL: I reckon it's on the order of .05 percent. This is a common accuracy astronomically. However, Venus is a difficult object because it has to be observed during the day. The sky is equal in intensity to Venus and is usually many times more polarized. So that's the real problem.

DR. SAGAN: You are proposing a pressure modulation of a few millibars to explain the polarization variations?

DR. BOWELL: Yes.

DR. SAGAN: What variation in ultraviolet contrast would result from the same pressure modulation?

DR. BOWELL: I haven't calculated that.

DR. SAGAN: I am wondering if these are compatible numbers. Jim Hansen thinks the answer is no.

DR. HANSEN: The magnitude of the changes in the Rayleigh optical thickness that you're talking about would have a negligible effect on the contrast, on the brightness.

DR. BOWELL: Therefore, you don't think the polarimetry would necessarily be correlated with the ultraviolet markings?

DR. HANSEN: That's right, not necessarily. I suspect that it may be correlated, but I think that that explanation for the contrast variation is wrong. I think your data are very impressive. The magnitude of the effect that you see is clearly much larger than the non-systematic errors. But it would be easy to construct half a dozen different models which could give you that type of variation, and I don't think you can choose between those models until you have local polarization measurements of bright and dark areas. But you need to have these as a function of phase angle, and since things are changing on a short time scale, you can't wait for the phase angle of Venus to change as seen from the earth. So far as I can see, the only way to solve the problem is with measurements from an orbiting spacecraft.

DR. YOUNG: I would point out that essentially we have this kind of observation from the ground. It's easy to see the CO_2 variations. We made a special effort to try to match up the CO_2 variations with ultraviolet features. We just don't see any variation at all between bright and dark ultraviolet features on the same day. I mean what we see is quite like what Ted Bowell sees, namely something like 5 or 10 percent variation in the apparent amount of gas. He sees it in Rayleigh scattering and we see it in CO_2 absorption, but it's the apparent amount of gas in the line of sight. But when you look at a bright area and a dark area in the ultraviolet photographs and ask if there is any difference between them, my answer is that the average difference is 1 ± 3 percent. It's very mysterious as to why the difference is so small. I surely expected I would see some difference and nothing came out.

MARINER 10 IMAGING SYSTEM

Edward Danielson, Jet Propulsion Laboratory

Dr. Danielson presented a collection of pictures taken by the TV system on Mariner and a 'movie' of the apparent global cloud motions on Venus. The movie was constructed from images taken at 2 hour intervals for 3½ days and 8 or 12 hour intervals for an additional 5½ days. Some of the pictures are shown, for example, in 29 March 1974 issue of Science. It is expected that a more extensive paper will be published in the Journal of the Atmospheric Sciences late in 1975.

The Mariner 10 spacecraft was launched in November 1973. Part of its payload were two twin cameras on a movable platform. The spacecraft trajectory was designed to use the gravitational assist of Venus to reach the planet Mercury. The prime objective at Venus was to look for the markings in the UV which have been studied for many years with earth-based observations.

The first pictures of Venus obtained from Mariner 10 were of the northern cusp in the visible spectrum, and these showed no detail.

The first unique features observed were haze layers at the limb. These pictures were also in the visible spectrum and the layers were about 80 km above the surface.

All the photos of Venus taken at closest approach in the blue and orange filter showed essentially no features. There is a little limb darkening.

About three hours after closest approach the computer automatically commanded the moveable platform to systematically mosaic the planet, with the images taken through the ultraviolet filter. The ultraviolet features were highly symmetric in the northern and southern hemispheres, and had a pronounced mottled appearance near the subsolar point with the presence of cellular-like features.

Bow-like waves were present about the subsolar point and their appearance suggests that the subsolar point acts like an obstacle.

Very light circumequatorial markings were present. The motion picture, obtained from successive images, indicates that these moved from higher latitudes toward the equator.

The cameras were very sensitive and could measure a relative contrast of the order of 1 percent or less. Computer processing was used at the Jet Propulsion Lab to enhance these contrasts.

Images were also taken of the earth shortly after launch, and we have compared these with ATS satellite photos taken at the same time. The main purpose of this was for calibration. It is also interesting to point out the cyclonic-type features, which are absent in the Venus images.

Finally, it should be emphasized that the pictures which are usually displayed have been filtered and contrast enhanced. For example, the popular mosaic which was on the cover of Science [183, no. 4131, 1974] was high-pass filtered which removed a gross dark band across the equatorial

region. That picture is a favorite of everyone because we had an artist go through and take off all the picture edges, reseau marks, etc. I just want to warn you that in interpreting the markings its essential to go back to the photometric data.

DR. SEIFF: Apparently you had to take the pictures of different sizes and enlarge them so they all appeared to have the same diameter before you made the movie, is that correct? Also, how much rotation of the view angle was there within the sequence of pictures due to the trajectory of approach?

DR. DANIELSON: We didn't take pictures on approach. These were all taken after the spacecraft passed the planet. The spacecraft design constrained us to not being able to photograph until we were right on the terminator. The first mosaic was made up of about 30 pictures taken at 24 hours out. In the time it took to take those pictures the planet size wasn't changing fast enough to require any scaling. But later mosaics were scaled to the size of the initial one, so they had a decreasing resolution.

The phase angle changed very little during the observations. It was fairly constant at about 25 degrees. We were on a very straight asymtote.

DR. JONES: I am interested in the correlations between IR features and UV features. Was there any overlap of measurements with the IR radiometer?

DR. DANIELSON: There was no overlap.

DR. GREYBER: The movie showed some white streaks around the subsolar point, and also at higher southern latitudes. Were these the white marks which moved toward the equator?

DR. DANIELSON: Yes, they were very faint light marks which moved toward the equator.

CLOUD MOTIONS ON VENUS

Verner Suomi, University of Wisconsin

I don't purport to offer a general review, but I think we will find some of the results which have followed from the movie you have just seen to be fairly interesting.

As you know, the images from the Mariner 10 camera are a part of a team effort; several of the team members are here today. When I talk about the results, you must appreciate that all of the individuals participated in the planning and execution, and of course in some of the arguing about the results of the experiment. The detailed measurements on the pictures were done by my colleagues from the University of Wisconsin, Mr. Bob Krauss and Mr. Sanjay Limaye. They did most of the work and deserve the credit.

I would like to talk very briefly about what clouds can tell us. Displacements of cloud markings or cloud texture can indicate winds. If one has sufficiently accurate navigation, that is if one can relate items in the object plane to positions on the planet, one can obtain velocities. In the instances in which one does not have good enough navigation to indicate the exact angles, one can still obtain an indication of wind shears.

Now there are, of course, difficulties with using clouds as atmospheric tracers. I want to emphasize these difficulties and take a few minutes to illustrate how carefully we treat them. How we handle these problems may affect how much of our conclusions you will be willing to accept at the end.

First, we must consider the cloud scale. On the earth, for example, a large-scale cloud system several thousand kilometers across might indicate the motion of the storm and not the winds. On the other hand, a very small cloud is difficult to resolve, and usually the small clouds have a shorter lifetime than the large clouds.

Moreover, we need inactive clouds, ones which merely drift with the wind, rather than those which are changing dynamically. For example, suppose a cloud on earth were a large rapidly expanding cirrus envelope; as the cirrus cloud expanded a component due to the expansion would be added to the general motion. These effects can be seen on a video tape recording of the navigated Venus images. The TV image is too small for all of you to see in this room, but it will be set up in the hallway this afternoon so you can see for yourselves. In some of the pictures you will clearly see that the clouds are growing. Thus it can make a great deal of difference if one makes the measurement on the forward edge of the cloud or on the following edge of the cloud. On the other hand if one uses the center of mass of the cloud as the marker, he may get somewhat different results.

Another difficulty arises from the unstable imaging geometry. Those who have used ground-based telescopes have both a stable platform and stable film. The geometry in the image is preserved very well indeed. On the spacecraft, on the other hand, we have a "rubbery" film. It is called a vidicon. This does not preserve image geometry very well, furthermore, it is not a very accurate photometer. The photometry must be corrected to remove image shading. In the movie which was presented, these effects were hidden. Fictitious velocity effects are easy to see in movies made from greatly enhanced photographs. One finds that the position of the terminator fluctuates. The terminator must be fixed in space on the time scale of the movie, so that error must be removed also. Despite these several difficulties,

it is possible to obtain fairly good results, if one is very careful.

I have tried to indicate some of the problems that must be overcome when one attempts to use TV images quantitatively. We will give you more details later. One must not ignore the real advantage over ground-based observations, i.e., higher resolution, shorter time intervals, perfect seeing. But we will be making greater demands on the observations. We will want to observe the global distribution of the planetary motions, but before we do, we want to point out that there is other valuable information in the images.

Except for atmospheric motions, images are not useful indicators of atmospheric state parameters such as temperature, pressure, or composition. Images are, however, surprisingly good indicators of processes underway in the atmosphere. For example, the images reveal convective activity which implies certain vertical motions. It is also possible to see wave motions. There are good examples of these processes in the earth view shown in Figure 1 which was taken from Mariner 10 on its way to Venus.

In the upper left part of Figure 1 one can see waves in the cloud field which could be fairly small-scale waves or billow clouds. In the lower middle of the picture, one can see polygonal-shaped cells. This is very typical of shallow convection, i.e., that which occurs when the atmosphere is heated from below or strongly cooled from the top, but with a definite lid on convection under a strong inversion. In another part of the image, there is a cloud cluster which indicates deep vertical motion.

Images can reveal processes, at least qualitatively. Convection and wave motion are typical examples, but one might go so far as to say that there may even be indication of the global heat budget. Areas where the energy received by the planet is greater than the energy lost by the planet tend to have convective clouds, whereas areas for which the loss from the planet exceeds the gain from the sun tend to have more stratified clouds. This seems to be true on the earth, too. Images can be a useful qualitative indicator of processes but we have not reached the stage where one can be quantitative about it.

Clouds can be indicators of the general circulation also. The most obvious examples are the circulation zones. This is certainly true for the earth, and may be true on Venus and other planets as well. We have jet streams which are fairly easy to identify, and large-scale storm features which we have already mentioned.

Images of the planetary cloud field behavior can also show long-time instabilities. The Venus flyby shown in the movie is a very limited 8-day sample of the Venus circulation. On the other hand, the large number of ground-based observations show that other motions or changes in the circulation pattern could be present.

I do not propose to review the ground-based observations to an audience containing individuals who have been busy taking these beautiful observations for many years. They are far more familiar with them than I could ever hope to be. What I can say, however, is that the close-up views given by the Mariner 10 flyby have made these observations even more valuable, because we can now better interpret what can be seen in the ground-based observations. I emphasize again that the Venus flyby observations encompass only 8 days, enough for only two trips of the clouds around the planet. There are decades of ground-based observations. Lifetimes of the large-scale features can be studied using ground-based observations. They cannot be studied as well from Mariner 10.

The ground-based observations record the features of the whole planet at the same time or for the same time interval. The close-up pictures of

Fig. 1. Mosaic of Mariner 10 images of earth. Most of the cloud features in the lower half of the mosaic are over the Pacific ocean. In the upper half of the figure clouds over the Gulf of Mexico and the United States can be seen. Polygonal cells can be seen in the lower middle of the mosaic and wave associated clouds can be seen in the upper left of the picture.
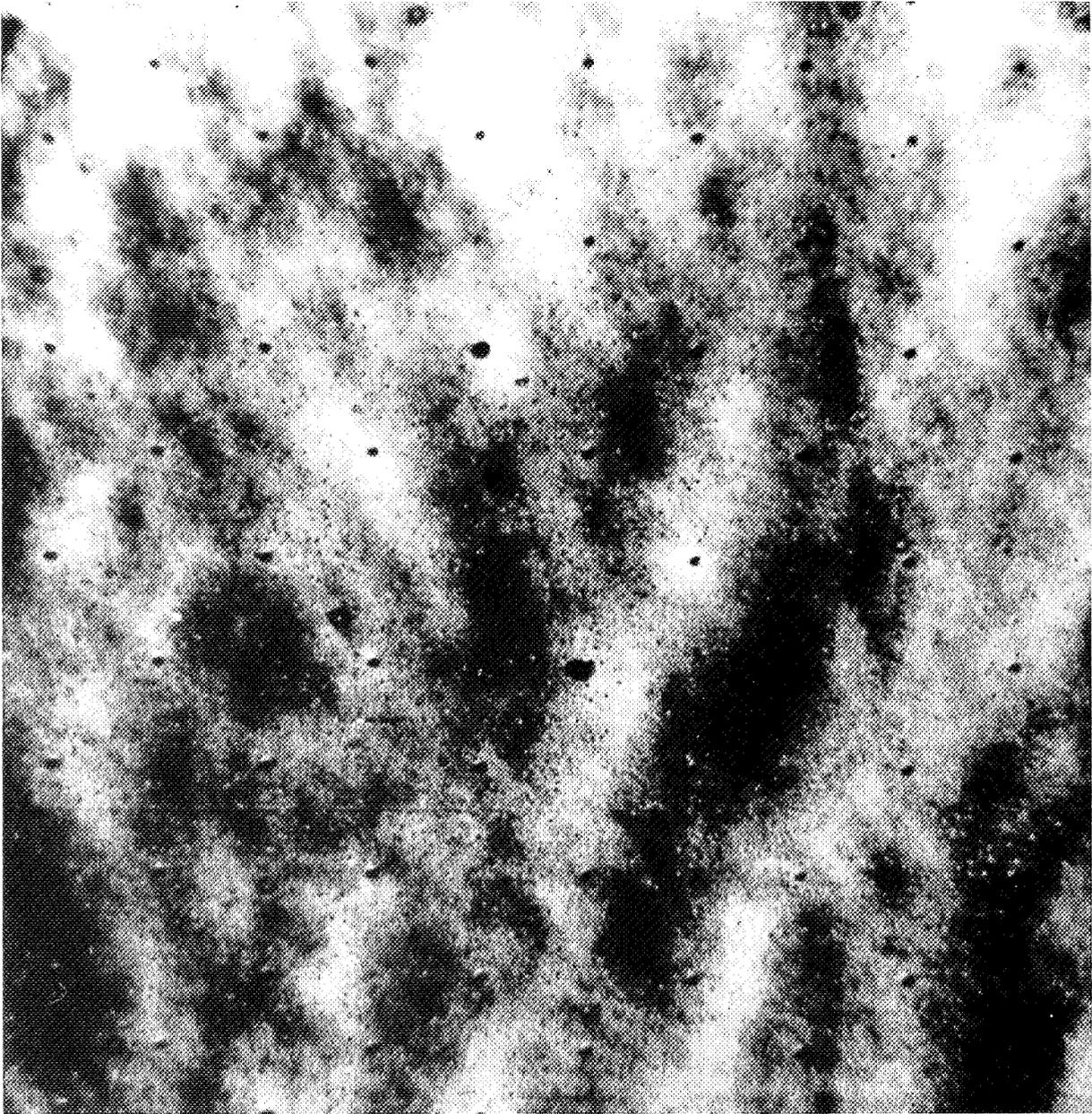
Fig. 2.   Mariner 10 photograph of Venus showing the sub-solar region.   The image has been stretched to enhance contrast.   Some cellular structure can be seen in this and other images of the sub-solar region.

Venus shown in the early frames of the movie are a mosaic of many pictures which span a good fraction of an hour.   There is a distortion in the UV markings because all parts of the image were not photographed simultaneously. We will say more about this later.

Figure 2 of Venus is illustrative of things which I mentioned already. It is a Mariner 10 photo of the subsolar area, where there is evidence of convection.   This photo, taken early in the flight, has fairly high resolution and has hexagonal cells in several places.   Polygonal cells are clearly
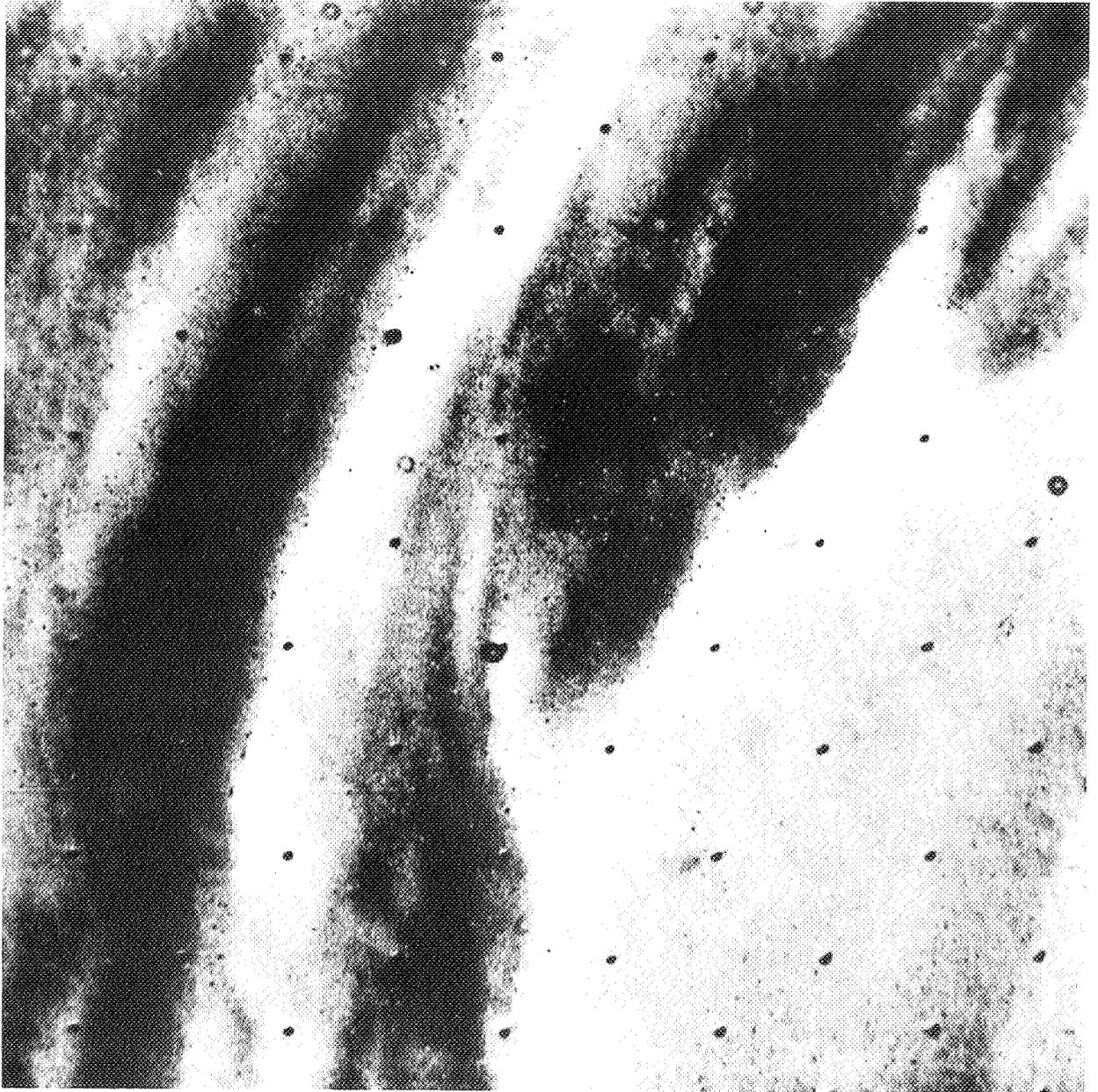
Fig. 3. Mariner 10 view of a high latitude region of Venus (southern hemisphere). The large bright region in the lower right has its periphery approximately along the 50°S latitude circle. The streamers along this edge may be caused by large velocity shears.

evident many places in the picture and it is evident that convection is occurring.

Figure 3 is a photograph taken at a high latitude where streamers torn from the main cloud indicate a stratiform cloud with strong horizontal shear. This photo was also taken when the spacecraft was quite close to the planet.
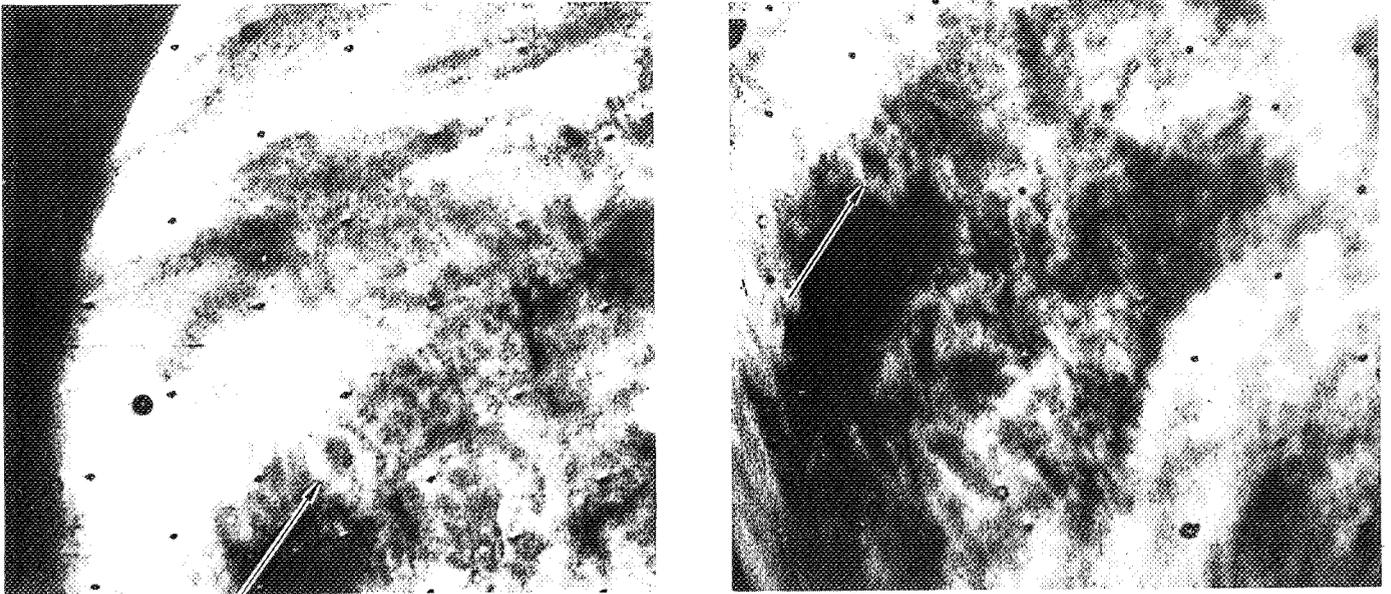
Fig. 4. *UV markings in the sub-solar region which look like convective clouds. Such features are fairly long-lived. The left figure shows one feature which could still be seen two hours later in the right figure (arrow shows the position).*

DR. SAGAN: Vern, could you point out one or two polygons?

DR. SUOMI: It is possible, depending on the degree of convection, to have polygons where the walls are cloudy and the space in between clear. Here [in Figure 2] is such a polygon.

DR. INGERSOLL: You are making the assumption that dark spaces are clear and light spaces are cloudy?

DR. SUOMI: Yes, I am making that assumption. Obviously, if I wanted to show a picture of Venus as you and I would see it, I should just take out the slide and show you a bright blank screen! In the ultraviolet, however, there is much contrast and Figure 2 is what you see in an enhanced photo. On the earth we have two kinds of cellular cloud systems, those where the walls of the hexagon are clear and the space in between cloudy and others where the walls are cloudy and the space within the hexagon clear. Generally speaking, when the convection is strong one tends to have the cloudy walls. For our purposes, i.e., to illustrate a convective process on Venus, it makes little difference if we assume black clouds -- cumulous bituminous -- or white clouds in a dark aerosol background. The effect is the same. It's only a matter of degree. But, as the lawyers say, a change in degree is a change in kind.

There are very many Venus photos which show evidence of convection, and many more which give evidence for horizontal wind shear. Now whether or not the convection we see is evidence for the presence of condensates is a different story. In this case the time scale is very important. If clouds or markings have a long lifetime there could be dust-like material forming the markings, but if the time scale of formation and decay is short then it is more likely that the markings are a result of some condensation processes.
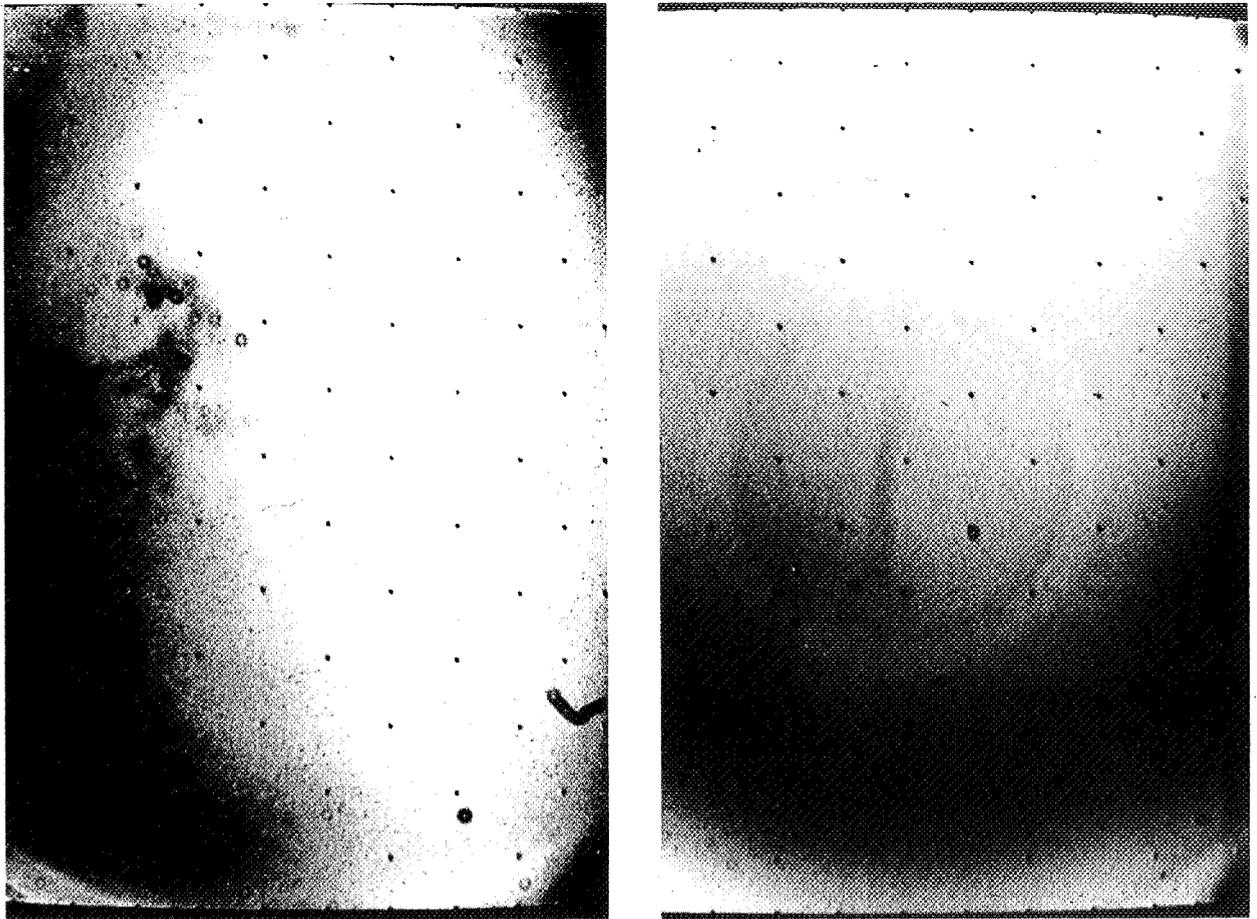
Fig. 5. Contrast enhanced flat field vidicon image, camera A left, and camera B right.

Figure 4 shows examples of markings which look like convective clouds. The contrast has been greatly stretched and it is a much clearer example than you saw in the movie. The same figure shows examples of convective cells which are preserved in two pictures taken about 2 hours apart. These patterns are very characteristic of the cloud structure one sees in satellite pictures of clouds in a sub-tropical high on earth.

My point of showing you evidence of convection is not to dwell on the Venusian convective processes in themselves, but to indicate that the motions of the markings are likely to represent motion of the atmosphere also. Furthermore, the changes in the markings over time scales of an hour or so do, as I have already mentioned, effect the accuracy of our measurements. If a marking were to remain fixed in shape over a time period of hours, one could get quite accurate results. However, if the cloud is changing shape rapidly one is not certain he is tracking the same target. On the other hand, a cloud does not change shape very much in time intervals of minutes, but it doesn't move very far in that short time interval, so the percentage accuracy of the *distance* measurement is poorer over short time intervals. As you will see later, an automated computer analysis requires that there be little shape change, but it can measure displacements very well. On the other hand an operator can track a cloud even if it undergoes considerable changes in shape, but he cannot measure the distances as well.

Ed Danielson already mentioned that it was necessary to correct for vidicon shading. Figure 5 is an example of a photo of a uniform field, but with a maximum contrast stretch to show that the response is far from uniform. What appears to be a signature of Channel 7 television is actually caused by a reflection from the cathode in the vidicon tube. Blemishes appear everywhere on the faceplate. Actually these represent only small signals, but they are visible here because of the very high contrast stretch. The photos in the movie and those used in our analysis appear to be uniform, but you must appreciate that they have had extensive shading corrections and considerable contrast stretch. Moreover, because each target in a sequence of pictures is normally photographed on a different part of the vidicon target area these corrections can actually contribute to further errors in cloud displacement observations.

The movie was made from a large number of pictures. The measurements we have made came from a very small number of these pictures. In order for you to understand just what was done, I am going to describe the important procedural details listed in Table 1.

IMAGE PREPARATION

At Jet Propulsion Lab

FICOR - Vidicon Shading Correction

GEOM   - Automatic Reseau Finder

       - Remap to Object Space

       - Scale Factor Proportional
         to Distance from Venus

Prepared Data Tapes Sent to
University of Wisconsin

Table 1. *Preliminary image processing needed before images may be used for feature tracking.*

FICOR, a Jet Propulsion Lab analysis program, removes vidicon shading. That's number one. Secondly, GEOM uses the Reseau marks and remaps the scene in object space rather than image space. Remember that image space not only contains minor optical distortions, but also contains larger distortions which are due to non-linear raster scanning and charge distribution distortions. In addition GEOM takes into account the changing scale factor due to the change in distance to Venus. The output of GEOM is a new magnetic tape. We wish to acknowledge JPL's effort to provide us with these processed tapes.

The next step in the analysis is to superpose one image matrix on another so the planet's reference frame is fixed. We call this process image navigation. This is fairly easy to do for a sequence of images of the earth because landmarks can be used as reference points. It is much more difficult on Venus since we do not have any reference points. Worse yet, the limit cycle in the Mariner 10 spacecraft stabilization system and the need to move the camera scan platform caused very large changes (measured in terms of the camera field of view) in pointing angle. If one looks at a series of raw pictures it is as though one were looking at a

Best fit to Sphere:

Choose
Points
on
Limb

Planet
Center
+

Lat - Lon Grid Definition:

Clock/Cone
Coordinates
Point to Sun
to ±0.25°

(<1 pixel
tilt)

Sub
Solar
Point

Sub
s/c
Point

Single
TV Scan
Line

INPUTS

- Lines per Frame

- Elements per Frame

- Height of Frame (deg)

- Width of Frame (deg)

- Distance to Venus Surface

- Latitude of Sub s/c Point

- Longitude of Sub s/c Point

- Latitude of Sub Solar Point

- Longitude of Sub Solar Point
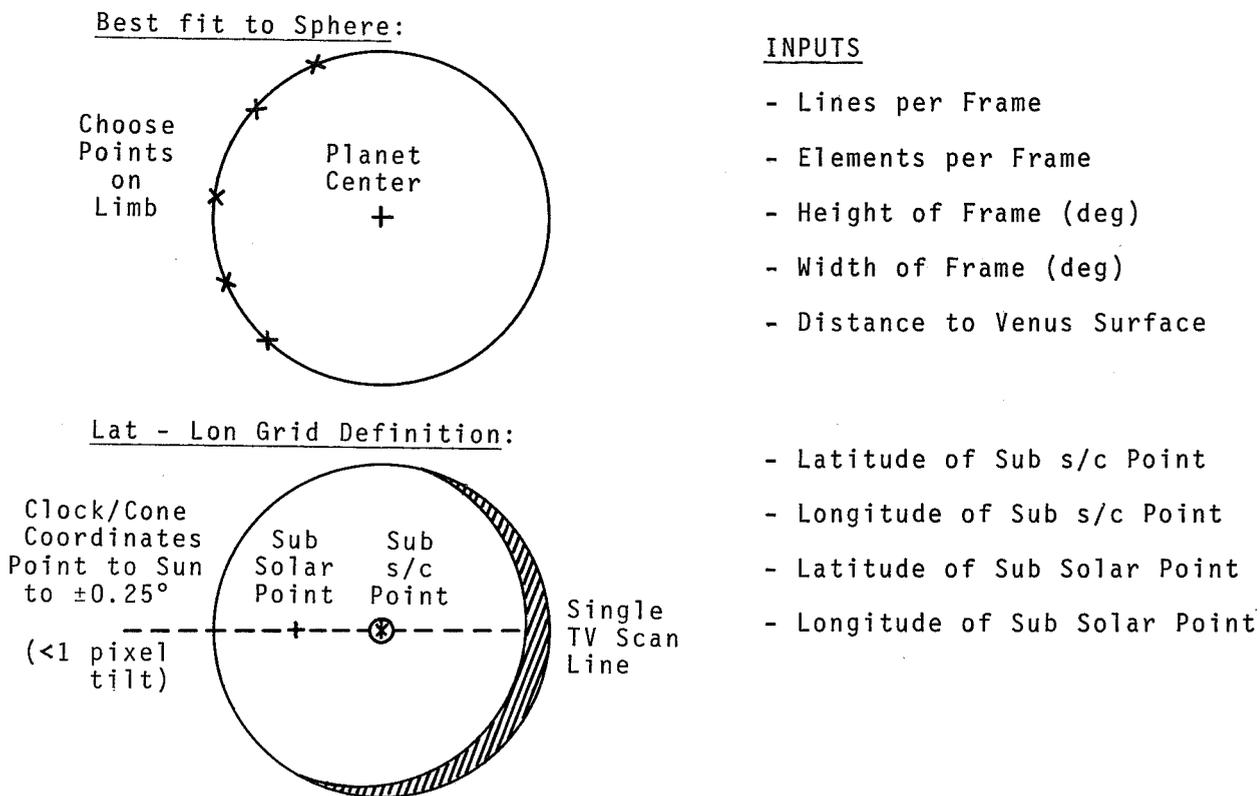
Fig. 6. The basic inputs to image navigation. The contribution due to roll error is ignored in the first approximation.

planet through a telescope hanging on a string in a high wind! The picture sequence moves very erratically.

The errors in pitch and yaw sometimes amounted to half the image or more -- and sometimes were even greater since Venus didn't even show up on the picture. Fortunately the roll error was much less, only about a quarter of a degree, since the roll control was tied to Canopus and was very stable. Large yaw and pitch errors are easily removed by displacing an image left or right and up or down. There was no need to rotate the image. Under these circumstances it is possible to use the planet's limb as a reference. The navigation scheme used the best fit to a sphere for 5 points on a bright limb. Other inputs were lines per frame, elements per line, the height of the frame in degrees, the width of the frame in degrees, and the distance to the Venus surface. The navigation program was developed at the University of Wisconsin by Dennis Phillips.

Figure 6 shows the navigation geometry used in the program. Both the sub spacecraft point and the subsolar point are known precisely. A line passing through these points can be represented as a single scan line in the TV image. From this information a conventional grid of latitude and longitude can be obtained.

Table 2 shows the various error sources. The geometric rectification in the FICOR program was accurate to about 1 pixel out of the 700 lines and

ERROR SOURCES

GEOMETRIC RECTIFICATION	~ 1 pixel
NAVIGATION MODEL FIT	~ 1 pixel
LAT - LON GRID DEFINITION	~ 1 pixel
ROUNDOFF & TRUNCATION	~1/2 pixel
RMS ERROR	~ 2 pixel

Single-Point Tracking Adds 1 Pixel Granularity
and Up to Several Pixels for Operator Accuracy

RMS ~4 - 5 pixels

Table 2. Various sources of error in the measurements of motion of the UV markings. In addition to these the spacecraft roll error also contributes somewhat to the final results. This component has been ignored as a first approximation.

832 pixels in a horizontal line. The navigation model fit was accurate to 1 pixel. The latitude and longitude grid definition is good to about 1 pixel because it is the result of a best fit. Round-off and truncation errors account for half a pixel. The total error, then, could add up to 2 pixels.

Single-point tracking, a technique in which a cursor is manually tracked on the moving TV image, could add about 1 pixel due to granularity plus several pixels due to the operator. The same operator may get a pixel or two variation each time he tries to track a specific object.

Table 3 shows how pixel errors and time intervals affect velocity errors. It is clear that one can exchange time interval length for greater accuracy even though the pixel error is large. On the other hand if the time interval is too long the cloud changes shape. A compromise must be made between trying to take as short an interval as possible in order that cloud shape is preserved, and a long enough interval to minimize distance error.

We used four techniques to track the clouds. One of the ways is single point tracking. In this technique the operator controls a cursor superimposed by the computer on the image; thus there is no parallax error. The accuracy is mainly limited by the skill of the operator.

In the other three schemes, the operator merely chooses an area in the first picture to be tracked and a corresponding larger area in the second picture which includes the same cloud or marking. The remaining processes are completely objective: one image matrix is correlated with a second image matrix in the computer. The techniques are summarized in Table 4. The peak in the correlation surface is a measure of the displacement. I apologize for going into such great detail on the techniques of analysis but it may be helpful in assessing the results of our analysis. Please understand that we have just begun our image analysis following these procedures and the results I now show you are what we obtained from the first few pictures.

Table 5 shows that 47 targets were measured repeatedly, with four norms applied over six different time intervals. The correlation failures occur

VELOCITY ERROR

PIXELS SHIFTED	km SHIFT	VELOCITY INCREMENT FOR A GIVEN TIME INTERVAL (m/s)				
		15 MIN.	30 MIN.	1 HR.	1-1/2 HR.	3 HR.
1	15 km	16 m/s	8	4	3	1.5
2	30	32	16	8	6	3
4	60	67	33	16	10	5
6	90	100	50	25	18	9

Table 3. *Resulting velocity error as a function of the time interval between images and the tracking error in km or pixels (for 15 km/pixel resolution). Shutter times for the four TV frames used were: T1 = 2^h57^m, T2 = 3^h49^m, T3 = 4^h02^m, T4 = 5^h33^m, all on day 39.*

CORRELATION TECHNIQUES

NORM	PREDOMINANT SENSITIVITY
EN - Euclidean Norm	Detail in Image
CC - Cross Correlation	Edges & Details
LP5 - Fifth Power Norm	Light & Dark Patches
SP - Single Point Tracking	Operator Opinion

Table 4. *Different image correlation techniques employed. EN, CC, LP5, use quadratic interpolation to search for maximum in correlation matrix - good to <0.1 pixel. SP uses line and element selected by operator - good to 1 pixel.*

STATISTICS FOR CLOUD VELOCITY MEASUREMENTS

TOTAL MEASUREMENTS	1194
CORRELATION FAILURES	187
SUCCESSFUL CORRELATIONS	1007
SUCCESSES WITHOUT T2-T3	887
>15 m/s DEVIANTS FROM TARGET MEANS	267
GOOD VECTORS REMAINING	620

Table 5. *Statistics for Venus cloud velocity measurements at the University of Wisconsin. 47 targets were measured repeatedly with 4 norms over 6 different time intervals.*
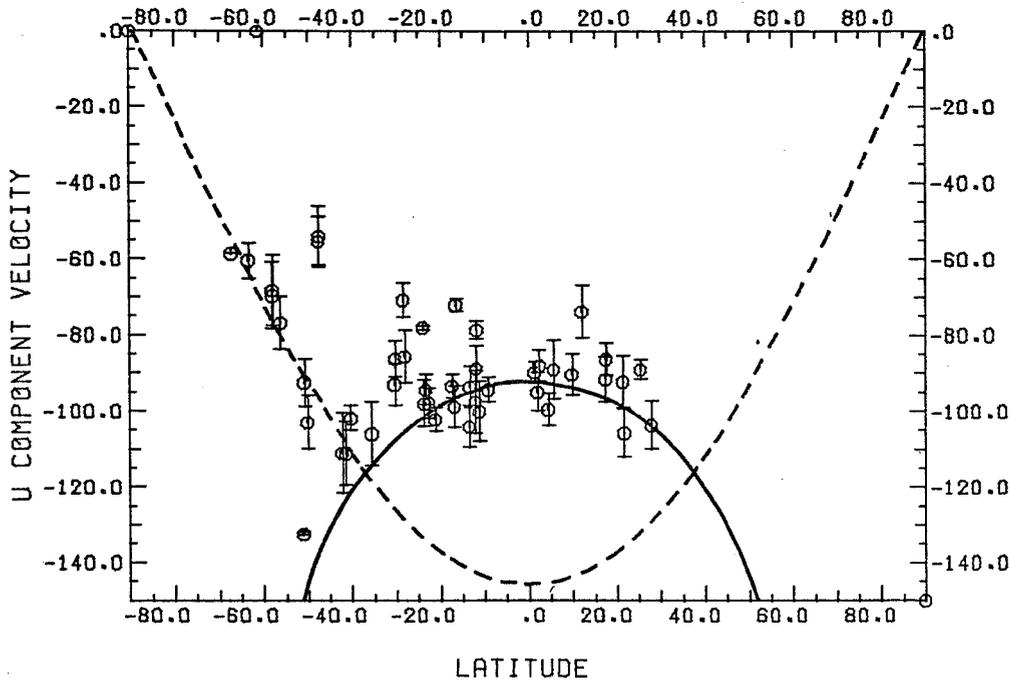
Fig. 7. Meridional profile of the zonal component of cloud motions measured by computer correlation techniques.

when the correlation peak is at the image matrix edge due to effects of different cloud patterns in the second scene. These correlation failures were rejected. The measurements over T_2-T_3 were also rejected. This interval was very short and one needs very high resolution to be able to use very short time intervals. In order to trust a measurement over a series of time intervals, one had to obtain more-or-less the same velocity in several intervals. We rejected those which did not repeat within 15 m/s or 2σ for a given cloud target. With these rejections 620 measurements of the over 1000 on the 47 targets remain.

Figure 7 shows velocities as a function of latitude using the completely objective computer technique only. A solid curve which represents a velocity profile for which angular momentum is conserved has been superimposed. The dotted line represents a velocity profile of constant angular velocity. Note that the curve is slightly different for each figure but the functional relationship is essentially the same. Figure 8 is similar but the velocities were obtained by single pixel tracking, which gives slightly larger error bars (rms deviations after 15 m/s edit) but less scatter since the computer operator is less sensitive to changes in cloud shape.

Although the data is noisy, there is evidence that the winds in mid-latitude regions of the planet are blowing slightly faster than they are in equatorial regions.

What we have is the following possible structure. In the polar zone, there is an indication of solid rotation or constant vorticity but outside this region, on the equatorward side of the polar ring cloud, there is conservation of angular momentum. Such a velocity profile would require some meridional motion - we have measured a small amount.

Leovy has shown that the North-South horizontal pressure gradient which exists on Venus could be balanced by the horizontal component of the
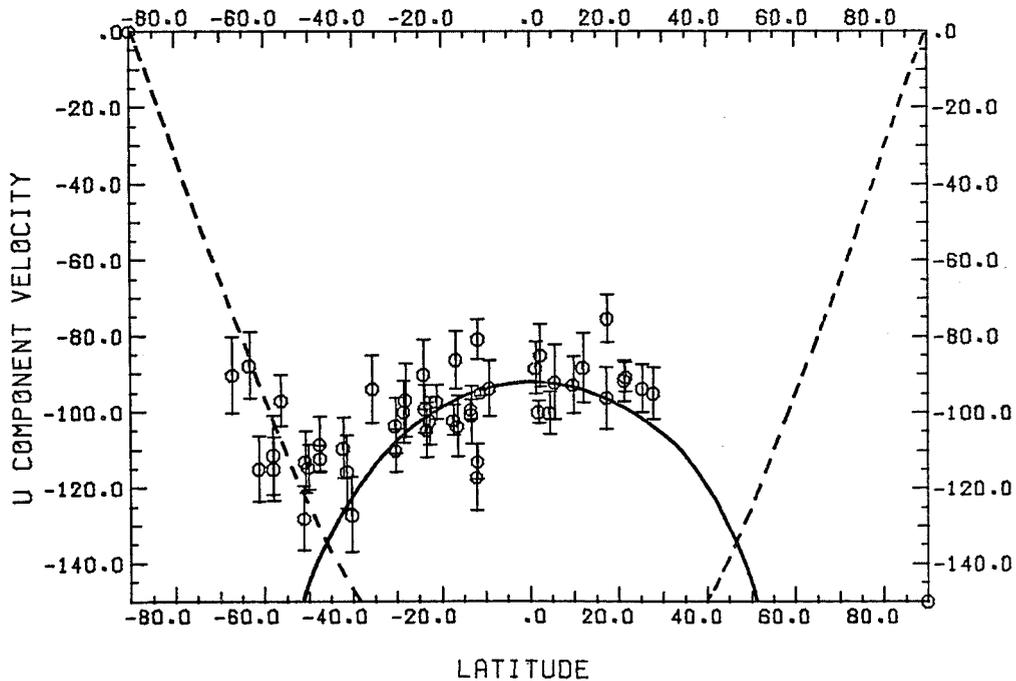
Fig. 8. Meridional profile of the zonal component of cloud motions measured by single pixel tracking. The computer operator follows the targets on a TV screen with a cursor.

centrifugal force. Using this scheme and the velocity profiles we have just shown, it is possible to derive a North-South *pressure* profile also. Clearly, a low must exist over the pole with pressure rising toward the equator to form a high pressure belt over the equator. But because of solar heating a bulge must exist on the belt.

This is one man's picture of what the circulation looks like, but clearly credit should be given to Hadley who thought it up in the first place. The Venus circulation looks like modified Hadley circulation.

Figure 9 shows a vortex generated using a spinning cage. The diagram is from a thesis by Nicholson who has developed a model of a vortex which contains friction. This vortex is only a few centimeters in diameter, but he finds the same structure for much larger vortices including hurricanes. In the outer regime one has conservation of angular momentum. There is solid rotation in the central portion. His model requires a mass sink.

Meridional motions on Venus also require a mass sink in the polar region. Also if there is conservation of angular momentum and a mass flow as suggested, prograde motions are required elsewhere on the planet so that the total angular momentum is conserved.

Figure 10 is further evidence for the velocity profile just suggested. It is a photograph due to Mike Belton, who used a central meridion section of a number of Venus photos and assembled them as a mosaic.

It is interesting to use the velocities which we obtained and ask: How far would a cloud in the polar belt move zonally compared to one in the equatorial region. If one uses our values and of course takes into account that we have a crude Mercator projection, one finds that the polar cloud should move about twice as far, much as it appears to have moved in the

mosaic. Moreover, there is a
curvature in these bands. The
shapes of these bands could be an
indication of the lifetime of the
particular cloud entity. If a
cloud had a very long lifetime it
would pass around the planet many
times, and would be stretched
virtually into constant latitude
lines. On the other hand, if the
residence time is very short, one
would not get these spiral streaks
at all. I am proposing that the
shapes of the UV markings are
indications of North-South
velocity shear and meridional
motion.

It seems to me, using some
imagination to be sure, that it
would be possible to use these
features in some of the ground-
based pictures as an indication
of the cloud velocity profile.
The angle of streakiness, it seems
to me, might be used as an indica-
tor of the meridional shear, and
possibly even as an indication of
the meridional motion.

Well, these are the main
points I wanted to make. I would
guess from now on the debate will
be very lively.

DR. POLLACK: What evidence
have you that the motion you see
of individual spots is really a
motion of air rather than wave
motion?

DR. SUOMI: Many years ago
when I first proposed using clouds
for obtaining wind motions on the
earth, there was very little acceptance of the idea. In fact, when I first
proposed it, I thought I would be shot at sunrise.

Fig. 9. A vortex model depicting the
tangential velocity as a function of
radial distance from the center of
the vortex. [After F. Nicholson,
Ph.D. Thesis, Univ. of Wisconsin,
1971].

I can only say that the preponderance of evidence on earth is that the
clouds form a very good indicator of the air motion, providing one is care-
ful. It is possible to see gravity waves. It is possible to measure the
motion, the phase velocity of those waves. In that regard you are absolutely
right. However even with orographic clouds, "fixed" to mountains, it is
possible to measure the motion of the cloud texture.

I cannot guarantee that these are indeed the actual air motions. I
would think, though, that as one got to higher and higher resolution, we
could see smaller clouds. The smaller the cloud the better the marker.
With large clouds, the dynamical effects might predominate. The motion of
a large cloud system could represent the motion of the storm rather than the
winds in the storm. So the motions of the markings might not be the air
motions. But from my experience looking at many clouds, and here of trying
to be as objective as possible, including results "untouched by human hands,"
so to speak, they seem to be moving as shown. I would have to fight with
the data to change the profile you saw.

Fig. 10. A mosaic of central portions of the Venus disk from Mariner 10 images made over a 4-day period, forming a pseudo-mercator projection.

DR. ANDY YOUNG: I am a little disturbed by the fact that in your selection of correlation, and so on, you wind up throwing out something like a third of your data.   I wonder if your results aren't partly a result of some selection effect, in which you've thrown out the data that don't agree with what you subconsciously expect to find and you've kept the ones that look right to you.

DR. SUOMI: Well, we tried not to do that.   There is something wrong when one gets a 2 sigma error on the same cloud for several time intervals. We thought of using all the data.   It would be noisier, but the profile would still look the same.

DR. RICHARD YOUNG: For what latitude were you quoting the meridional velocity?   Did you see structure with latitude?

DR. SUOMI: We did not see any structure with latitude for the meridional velocities.   I think you would have to say that we didn't see strong meridional velocities, but we clearly saw at least a couple of meters per second.   I think that most of our measurements are on the equatorward side of the bright cloud, and that our meridional velocities may have been reduced by the so-called cigar effect problem.   My guess is that the meridional velocities may be slightly higher.   What we should have done was balance out the meridional velocity errors attributed to one side of that cigar cloud with some on the other side.   So far this exercise has taught us how to go at it, and these are the preliminary results.

[Post conference note: Figure 11 illustrates the meridional component of the SP cloud motions as a function of latitude, showing why a -2 ± 5 m/s average was obtained.   Least squares fits indicate a slope of about 1 m/s per 10 degrees latitude with zero velocity near the equator.   Scatter is greatest in equatorial regions, indicating that convection, local turbulence, or vertical shear are probably present.]

DR. RICHARD YOUNG: The 5 m/s meridional velocity was at what latitude?

DR. SUOMI: That was not 5 m/s.   That was the variance.   The -2 m/s was the average for all latitudes.

DR. JONES: What was the smallest scale feature you could see in all of
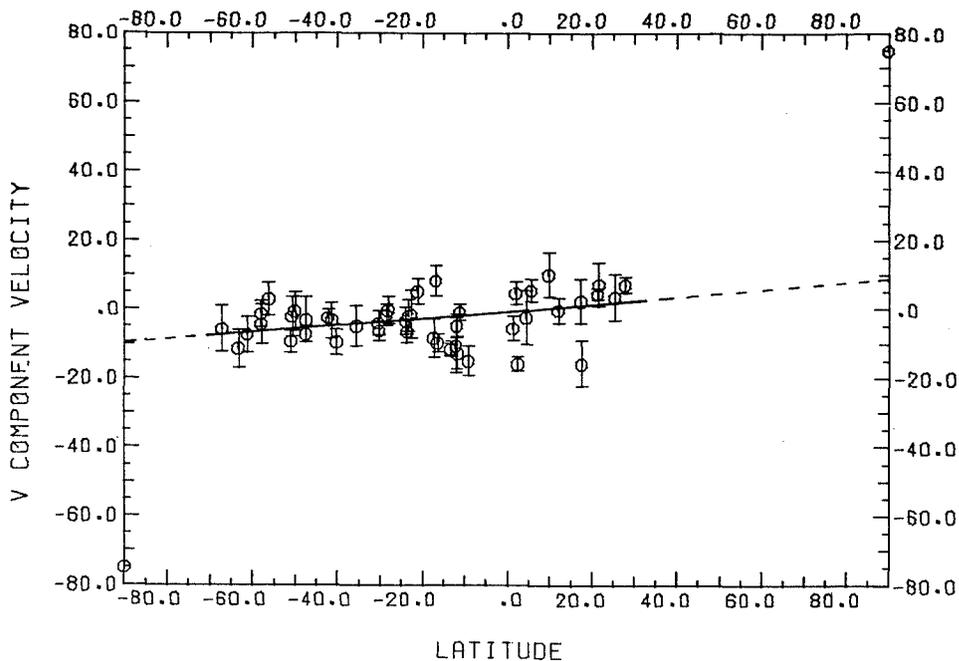
Fig. 11. A profile of the meridional motion component as a function of latitude, measured by single pixel tracking. The line represents the linear least squares fit to the data.

these photographs and how did that compare to your theoretical resolution?

DR. SUOMI: Of course, it depends on what we are using. If we are using the correlation schemes, then we are dealing with patterns. When we were dealing with single pixel trackings, we tried to do it by looking at some feature. We made a judgment about where the brightest spot was, where the edge was.

DR. JONES: There are two things that are seemingly in contradiction. Up until today, in looking at these pictures, I had always assumed that these patterns appear to show a flow spiraling toward the pole. You see these wide things and you see individual cloud lines, all of which are in the form of a helix going up toward the pole.

The other thing, of course, is that these patterns seem to rotate in a rather fixed pattern, going about the planet as a fixed pattern. This latter fact is consistent with the fact that you find that there is practically zero poleward velocity, -2 m/s, with an uncertainty of 5 m/s.

So I wonder if you have an explanation for how these patterns could simply rotate in a fixed configuration.

DR. SUOMI: Well a streamline and a trajectory are not necessarily the same. Let us assume we had a barber pole. In fact, we had a herring-bone barber pole. I would see the strip move from the middle toward the top and bottom of the barber pole.

DR. JONES: How do you paint the barber pole?

DR. SUOMI: That is up to the painter.

Now imagine a fly on the barber pole, and the fly to be at rest. He will obviously describe a circle, and will appear to describe a horizontal line as you view him at the same height. On the other hand, if the fly decides to walk up, you get a different slanted line. And I think these are the things we ought to consider.

I think the clouds are marks, and the marks get distorted by shear. If I could have an ideal cloud, from the pole to the equator, then I claim that cloud would bend because of the meridional shear, and it would not move very much at the pole. In the zone from the equator to 40° it would move faster. So the cloud line would bend to an extent depending on its residence time. If it stayed, not moving out of a latitude, it would eventually generate a fixed straight line. Since Figure 11 shows tilted streaks, the clouds which compose them must move meridionally.

DR. STONE: I found it very interesting that the explanations that have been proposed in the past for the zonal circulation have emphasized the Halley-type circulation, diurnal circulation. You emphasize the Hadley-type circulation which does not explain the zonal motion per se. My own feeling, for reasons that I will give tomorrow, is that you've got to have some of both.

DR. SUOMI: I wouldn't be surprised.

DR. STONE: But nevertheless, it is distressing that you don't find any meridional circulation.

DR. SUOMI: It is incorrect to say there is *no* evidence of meridional motion. We got -2 m/s. True, it is buried in a large variance, but that's what we got. I think when one asks how one can get a motion which is going faster at high latitudes where there is less solar energy, one requires some mechanism, and the conservation of absolute angular momentum is such a mechanism. For any mechanism which provides the greatest amount of motion where there is the greatest amount of heat, the highest velocities should be on the equator. But the highest velocities are not on the equator.

DR. SCHUBERT: I got the impression from your plots of velocity versus latitude that the preponderance of evidence was that the velocity was constant with latitude, and there were very few observational points at the highest latitudes with a faster velocity.

DR. SUOMI: Let's look at the figures again [Figures 7 and 8] which show the velocities obtained from the computer. You, of course, have to be aware that it is easier to measure near the equator than near the pole. We have few low velocities in the polar region, and more high velocities at mid-latitudes. You could argue that the navigation is in error and gives us a spurious result. That is possible. But the reason I went into all that detail describing our procedure was to show you that we tried our damndest to not have that error. I want to say again that most of the actual measurement work was done by Bob Krauss and Sanjay Limaye. They are the ones who put the hours in to try to resolve these things.

GROUND-BASED UV PHOTOGRAPHS

Bradford Smith, University of Arizona

Vern Suomi and Ed Danielson have shown you some exciting pictures of Venus, but they *do* represent only eight days in the life of Venus. So the ground-based telescopic photographs should continue to give us valuable information.

There seem to be a number of forms for the UV clouds, which bear some resemblance to one another. First of all, there is the so-called classical horizontal Y-shaped feature, sometimes with a tail on it. There is a horizontal psi-shaped feature, with an extension of the equatorial bar through the arms; the arms may be rounded or angular. Then there's another feature that simply looks like a reversed letter C. There are occasionally just a pair of parallel bands. In every case the arms are always, *with no exceptions*, open in the direction of motion.

The features are not always well-formed. The pattern is almost always symmetrical about the equator, within a few degrees of latitude. However, occasionally there is a feature which appears to be displaced by as much as 10° in latitude.

Another important point is that the features are always in motion. I know of no instance where we have been able to observe a discrete feature and have had a sufficient time base to look for motion, in which we have not seen zonal motion. The motion tends to be in a range from around 50 or 60 m/s to as high as 125 or 130 m/s, with a mean zonal motion of just about 100 m/s. The meridional motions must be less than 10 m/s. That gives an idea of the scatter we get from the telescopic measures. So this range of, say, 50 to 130 m/s is largely real.

Occasionally Venus exhibits cusps which are dark in ultraviolet light. These occurences are rare, but they were observed on two consecutive days in 1964. Preceding and following those two dates, the polar regions were bright, so it shows that changes do take place on relatively short time scales in the polar regions.

Andy Young mentioned this morning that the optical Doppler measurements do not show rotation, yet we certainly see something moving across the planet. This is clearly shown on the ground-based pictures, and also shown very well in the Mariner 10 photographs. Perhaps the Doppler measurements should be redone.

Another explanation might be phase changes, but we have been told this morning that cloud particle growth rates, dissipation rates and fallout rates tend to be rather long, of the order of 10^7 seconds, whereas the motions that we see in the telescopic photographs would suggest a need for changes on the order of 10^4 seconds. Indeed some of the Mariner 10 pictures that Vern showed suggest changes in perhaps 10^3 seconds. Perhaps the cloud physicists might find it in their hearts to identify some particles which can, in fact, grow or dissipate within these short time scales.

Alternatively we are left with Andy Young's suggestion that somebody is painting the planet. The only problem is that he must have a paintbrush in one hand and paint remover in the other and be running across the surface at about 100 m/s.

DR. SAGAN: That sounds like, to add to the Maxwell and Laplace demons, we now have a Young demon.

DR. JONES: He has been around for years.

DR. BELTON: Do you often see the bright polar rings which seem to be so obvious on the Mariner 10 photographs?

DR. SMITH: Yes.   From the ground we can't resolve them as rings as they were shown in the Mariner 10 photographs.   They show up as cusp brightening or polar brightening in our UV photographs.

DR. SAGAN: I might mention that 100 m/s, for a CO_2 atmosphere at the temperature of the Venus clouds, is Mach .5, and the upper limit of the spectroscopic velocity dispersion was Mach 1.   This surely must set some limits on the believability of the data.

DR. SMITH: I thought that Andy Young buried all of that this morning.

DR. TRAUB: I don't think it's fair to say that there is no evidence for mass motion.   What Andy was talking about this morning was only the errors that are induced by the solar Fraunhofer lines.   Tomorrow I hope to show that, indeed, there is motion.   I don't think there is any basis for saying that the motion is buried.

DR. SAGAN: In any case, if I understood Andy's presentation, he is subtracting 30 m/s from the published results, which does not leave 0 m/s.

DR. ANDY YOUNG: I don't really have any very helpful comment for people who want to look at the different types of data and try to match things up.   I might mention that a long run of observations, extending over three weeks in September and October 1972, shows large-scale features very similar to what the Mariner pictures show, and at about the same phase angle.   The people who are used to looking at the UV pictures say that the UV markings were quite contrasty at that time.   Since Ed Barker has water-vapor measurements on the planet during that time, and we have the CO_2 amounts and temperatures, some people might want to try to pull all this stuff together and try to make some sense out of it.

GROUND-BASED UV MOVIE

Reta Beebe, New Mexico State University Observatory

This film is a preliminary result of a project Vern Suomi suggested to us. We have taken some of our ground-based UV photos and attempted to make a time-lapse movie in order to see some of the long-term weather variations. I selected the apparition of 1967. During that period of time we had a long interval of rather good seeing which included several different types of features.

In this case we are looking at the evening terminator, so the apparent propagation is moving away from the subsolar point. From that point of view it is supplementary to the Mariner 10 movie.

The time sequence ranges from the 2nd of May to the 17th of June, 1967. Photographs were obtained roughly on 24-hour centers, at zero Universal time plus or minus 3 hours. The time slot for missing photographs was filled with a featureless disk, so as not to interrupt the time sequence. The phase angle ranges from 63 to 87 degrees. The plate scale was kept constant throughout, so there are variations in the apparent disk size and the position of the terminator that are caused by the seeing.

During the first week of this sequence there were V-shaped features that are typical when the motions are moving toward the evening terminator. Then the appearance lapsed into a banded shading by the 11th of May. That was followed by a strangely off-centered asymetric V-shape on the 12th of May (cf. Figure 1).

On the 17th, there was a rounded bull's-eye structure. And that reappears on the 21st, giving confirmation of the four-day cycle. On May 24, there was a large dark V in which considerable detail can be seen in the flow patterns. On June 9th there was quite a pronounced dark V which is illustrated in Figure 2. In the positive transparencies of this plate, turbulence shows along the equator. The remainder of the sequence shows typical features as it gets into the situation where the phase angle is increasingly large and less and less of the disk is visible.

So the short film contains six weeks of weather. The angular pattern changing into the circular pattern and back to the angular pattern is something that is relatively common. This is fairly representative of a highly featured situation in which the flow patterns are moving into the evening terminator. But in some other apparitions at the same elongation, the appearance is more blotchy. In those cases there are long periods of time in which the cloud structure is definitely less developed.

DR. JONES: What is the time between frames?

DR. BEEBE: Twenty-four hours.

DR. SUOMI: What is the longest time that it will be possible to photograph Venus with reasonable clarity on the same night?

DR. BEEBE: From one station it is about five hours.

DR. DANIELSON: What is the maximum contrast that you usually get when you have a photograph with strong features?

DR. BEEBE: What I've shown are composite photographs which are considerably enhanced. If one photometers these features, it becomes

apparent that the usual intensity profile should be subtracted before computing contrast values, because limb-brightening enhances the intensity toward the poles. In preliminary work in which I have included this, the contrasts of features range from 5 to 10 percent.

DR. BRAD SMITH: The highest contrast that I've seen for any well-defined feature is about 25 percent.
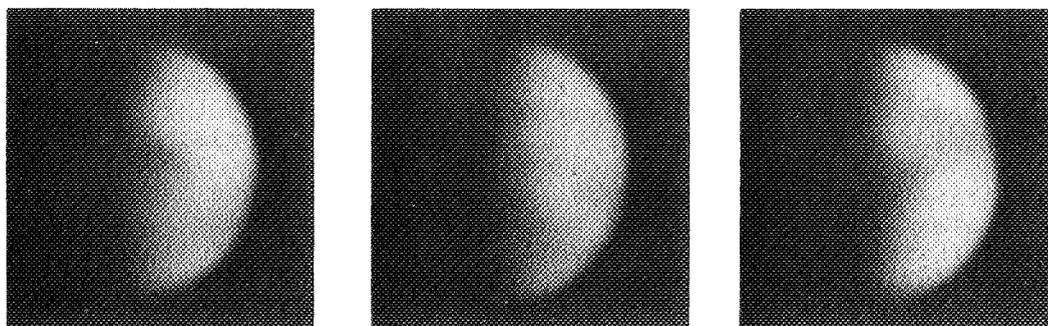
Figure 1. *Ultraviolet photographs with an effective wavelength of .36 μm and a bandwidth of .05 μm. The photographs were obtained 05 May 1967 - 0128 UT, 11 May 1967 - 0211 UT, and 12 May 1967 - 0153 UT, respectively. A classical "V"-shaped pattern, banded structure, and considerable asymmetry are illustrated.*
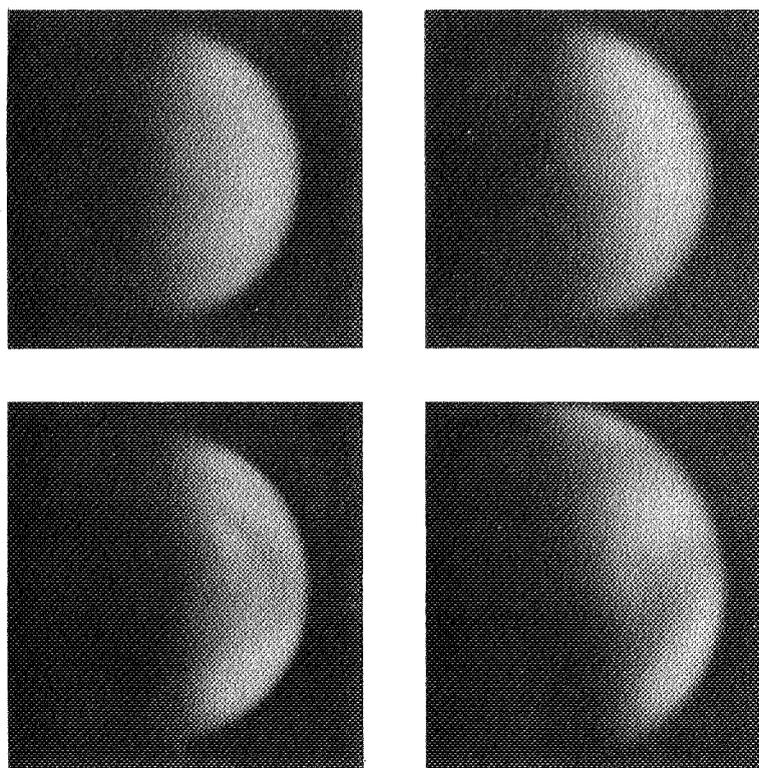
Figure 2. *The top photographs obtained 17 May 1967 - 0032 UT and 21 May 1967 - 0302 UT illustrate the "4-day" cycle and show a typical rounded pattern. The bottom photographs from 24 May 1967 - 0132 UT and 9 June 1967 - 0212 UT show angular features with small-scale structure.*

COMMENT ON MARINER 10 AND GROUND-BASED UV OBSERVATIONS

Brian O'Leary, Hampshire College

We have only 10^{10} bits of information to deal with here so I will try to compress them into three minutes.

The fourth day after the encounter we saw a feature on Mariner 10 that looked very much like the classical earth-based Y feature (Figure 1). This immediately suggested to me to do a calculation to test the 4.065794 ± 0.000001 day rotation period. When I first did it, much to my pleasant surprise, it came out to be at the correct phase predicted by the ground-based observations 8 years ago. But I checked my calculations and found an error. So there was no eureka, and that is not very surprising. But nevertheless the morphology is very similar in the ground-based and Mariner 10 pictures.

We have taken Mariner 10 pictures, projected them onto a globe, and looked at the globe from various directions to simulate the earth-based situation (Figure 2). The general configurations show a lot of similarity in the two situations. If you sort of blur your eyes or remove your glasses, some of the Mariner results resemble characteristic patterns that are evident in earth-based observations. In some cases you can see the Y-like divergences in the Mariner 10 results. In other cases there are bands parallel to the equator. And of course the bright polar ring that appears in Mariner 10 is very evident on many earth-based pictures.
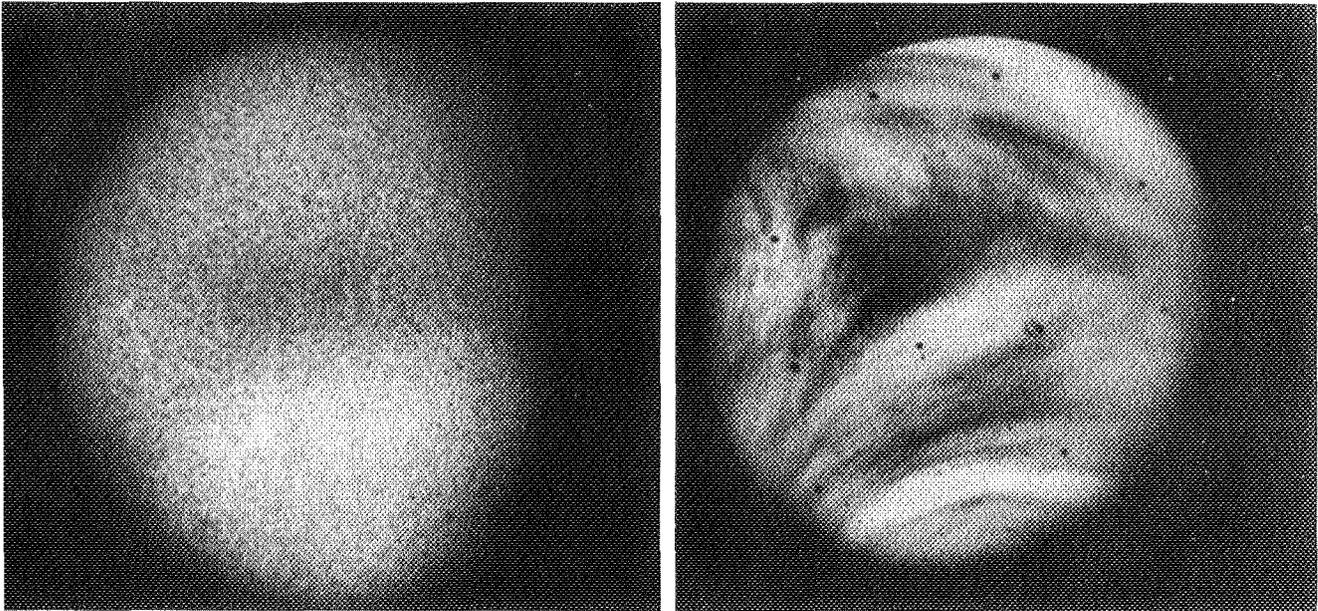
Fig. 1. A Y-shaped feature can be seen in UV light. The picture at the left was taken at the Pic du Midi Observatory, France (04:47 U.T., 24 July 1966); it has a resolution of about 500 km. The Mariner 10 picture at the right was taken from 3,300,000 km (03:57 U.T., 10 February 1974); it has a resolution of 65 km [B. C. Murray et al., Science 183, 1307, 1974].
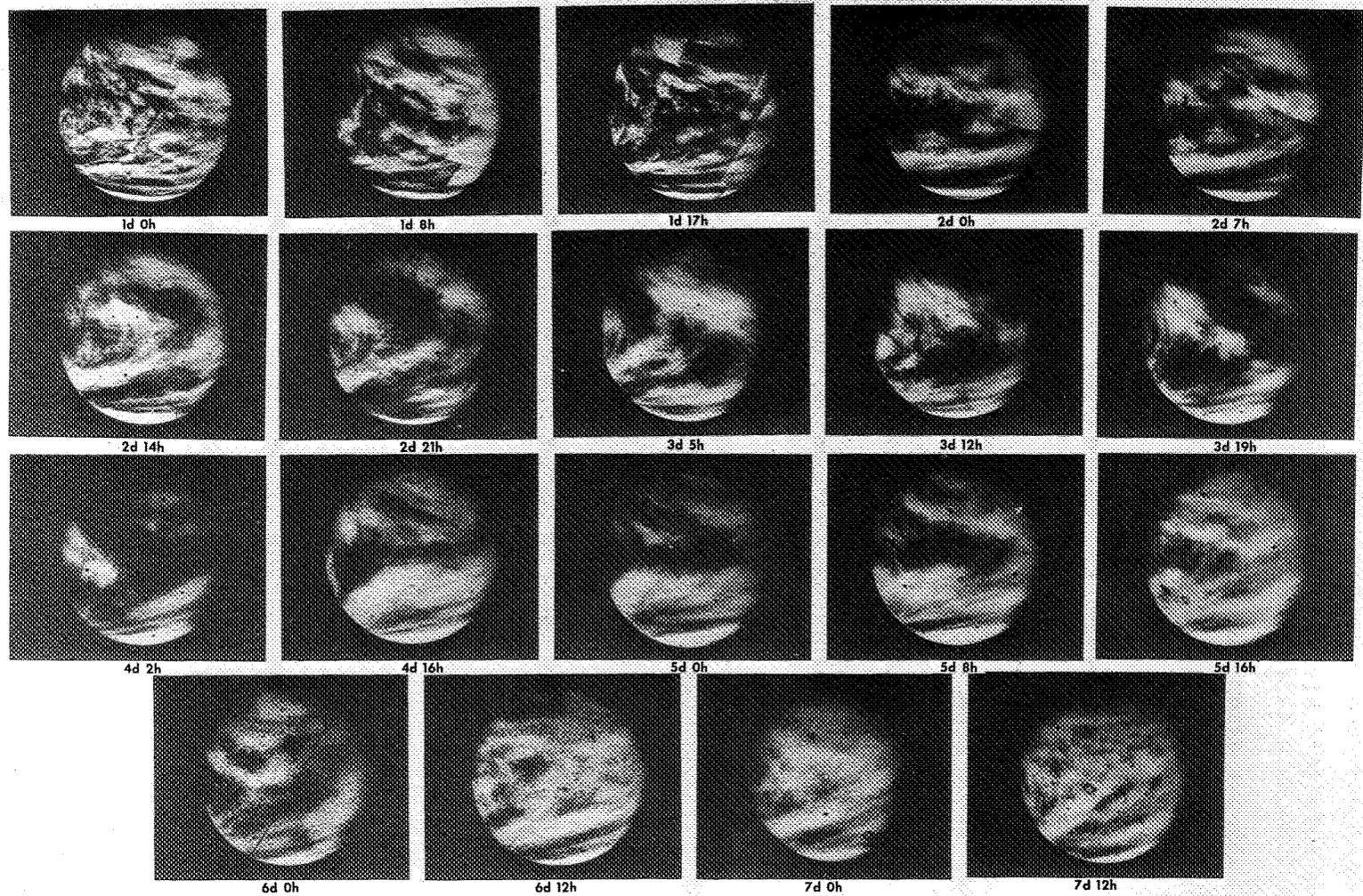
Fig. 2a. Mariner 10 Venus pictures projected on a globe and rephotographed from views over the equator at a central longitude corresponding to the subspacecraft point. Times indicate days and hours after closest encounter.
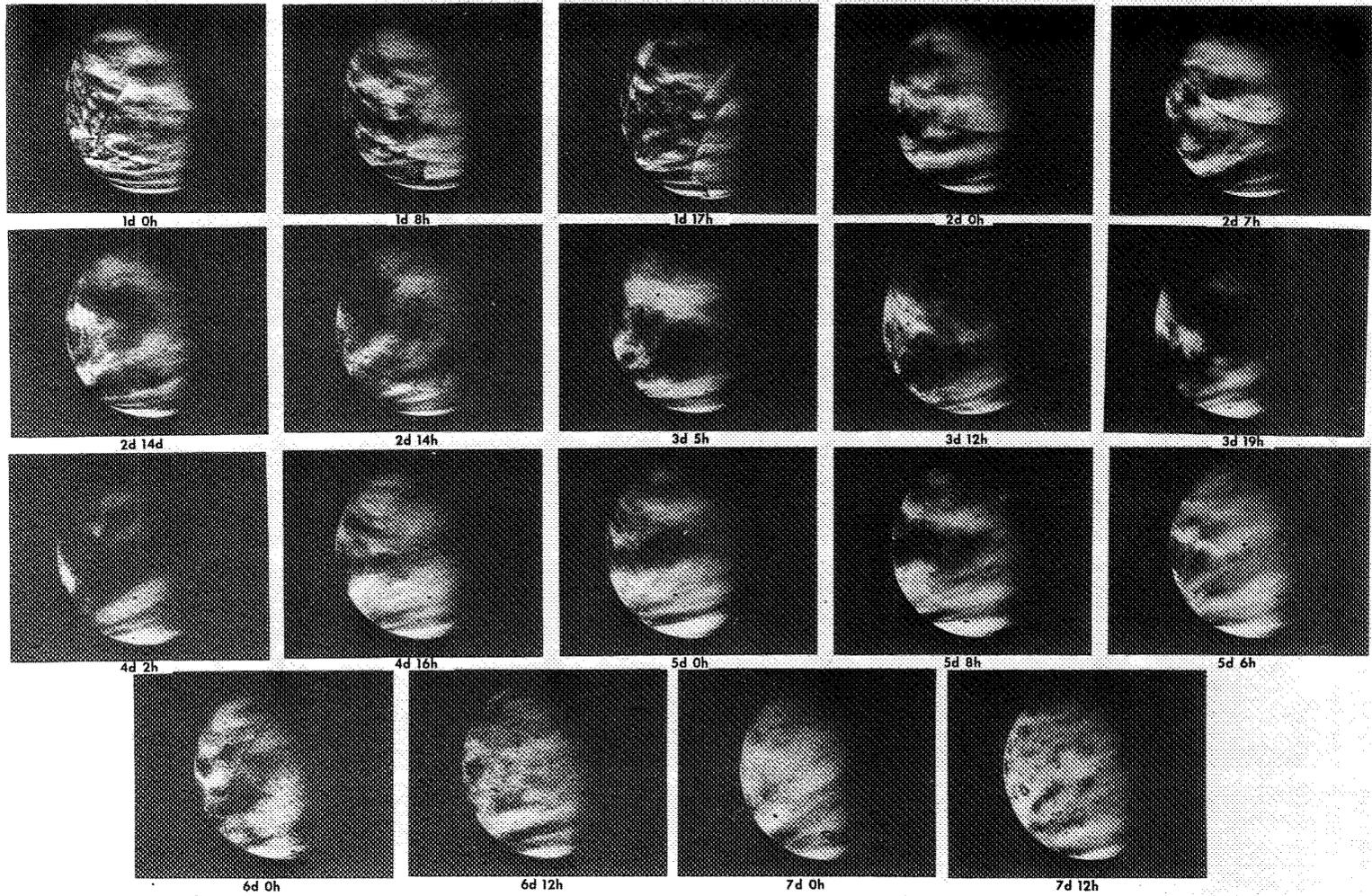
Fig. 2b. Same as Fig. 2a, but from a longitude 40° toward the morning terminator

Fig. 2c. Same as Fig. 2a, but from a longitude 40° toward the evening terminator.
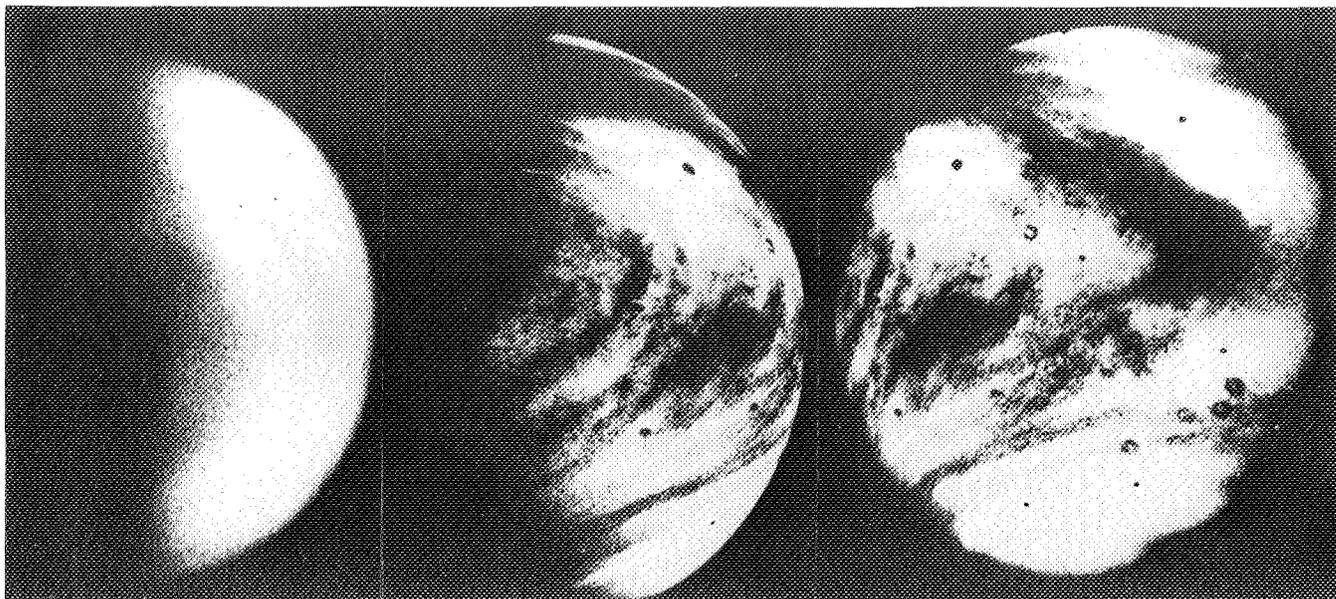
Fig. 3. (Left) Earth-based UV photograph of a reverse C feature on the evening terminator of Venus on 24 May 1967, 01:35 U.T. Courtesy of New Mexico State University Observatory . (Center) Mariner 10 picture 4 days after encounter, projected on a globe and rephotographed to give an unfore-shortened view of regions near the evening terminator. (Right) The same Mariner 10 picture viewed from the direction of the spacecraft. [B. C. Murray et al., Science 183, 1307, 1974]

I have not made a thorough analysis of whether the same features recur after four or five days in the Mariner pictures. This is a very obvious thing one should do. But the result is not at all obvious, even though there are some features which do seem to recur.

I spent a day at New Mexico State and looked over their large, im-pressive collection of earth-based pictures of Venus to try to see whether or not there were any kinds of trends or recurrences of different types of features. And the one thing that seemed rather interesting and perhaps. worth pursuing is that the reverse C-type feature, which shows a very distinctly curved morphology rather than an angular type of divergence, seems to preferentially form in the evening terminator. In going through the Mexico State collection, I found many, many more reverse C's on the evening terminator than on the morning terminator, by a very large factor (for example, see Figure 3).

Now, an intersting speculation -- and I think it's something that dynamicists should think about -- can be made about these bow-like waves observed by Mariner 10, and it is perhaps more intriguing if they really are bow waves. They seem, from both the earth-based and the Mariner 10 experiences, to form preferentially toward the evening terminator, that is, downwind from the subsolar direction. This may or may not be an indication that we are seeing, from both earth-based observations and from the Mariner 10 data, some basic asymmetries in the circulation pattern of Venus.

DR. SCHUBERT: If there are bow waves, wouldn't you expect them to form upstream of the subsolar point?

DR. O'LEARY: I am not going to make any comment about that.

DR. SAGAN: Maybe Mike Belton would like to.

DR. BELTON: I don't call them "bough" waves. I call them "bōw" waves.

DR. O'LEARY: This must be the effect of the monosodium glutamate.

DR. SAGAN: No, he always talks like that.

Bruce Hapke, University of Pittsburgh

We were very pleasantly surprised to find we were able to do relatively good photometry using the Mariner 10 TV system, in contrast to previous Mariner missions. I think the main reason was that the previous Mariners were plagued with a bad residual image problem. Mariner 10 used the technique of light flooding of the vidicon between taking each picture, and this removed the problem. We believe we can do absolute photometry of moderately high quality with the TV system.

To back up that statement, let me give you a couple of numbers. For Venus, based on ground-based photometry, the intensity (radiance) of the subsolar region on Venus has the value 310.3 w m^{-2} ster^{-1} integrated from 2000 Å to 7000 Å. We could measure this quantity through three different filters, orange, blue and UV. Measured through the orange filter, it came out to be 308, and through the blue filter, 302. Measured through the UV filter, with a light area at the subsolar point, this value is 330, and with a dark area at the subsolar point, 302. Taking the planet to be half light and half dark, the average is about 320. All of these are within 4 percent of the ground-based values. For Mercury the expected geometric albedo, based on earth-based measurements, is .125. The Mariner 10 albedo is .13.

DR. IRVINE: How do you get wavelength dependent numbers from something which is integrated over wavelength?

DR. HAPKE: Ask me privately, Bill. It's a long story. It's a fairly involved calibration. The FICOR photometric decalibration program is not trivial.

Figure 1 shows the three filters, normalized to unity independently. The effective wavelengths are about 3600 Å for the UV filter, 4800 Å for the blue filter and 5800 Å for the orange filter.

As Brian O'Leary said, we have 10^{10} bits of data, and we have only looked at a small fraction of that. One of the things I did was to take a few of the pictures and try to see what interpretations concerning the nature of the cloud structure could be obtained from the photometry.

Figure 2 shows a scan along the luminous equator of the planet taken through the orange filter. The first thing I tried was to fit the data with the simplest theory I know,
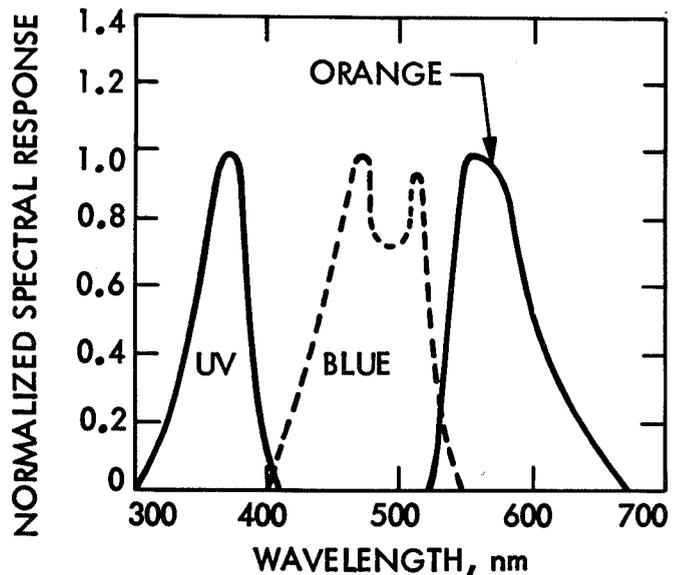
Fig. 1. Normalized spectral response of Mariner 10 TV filters.
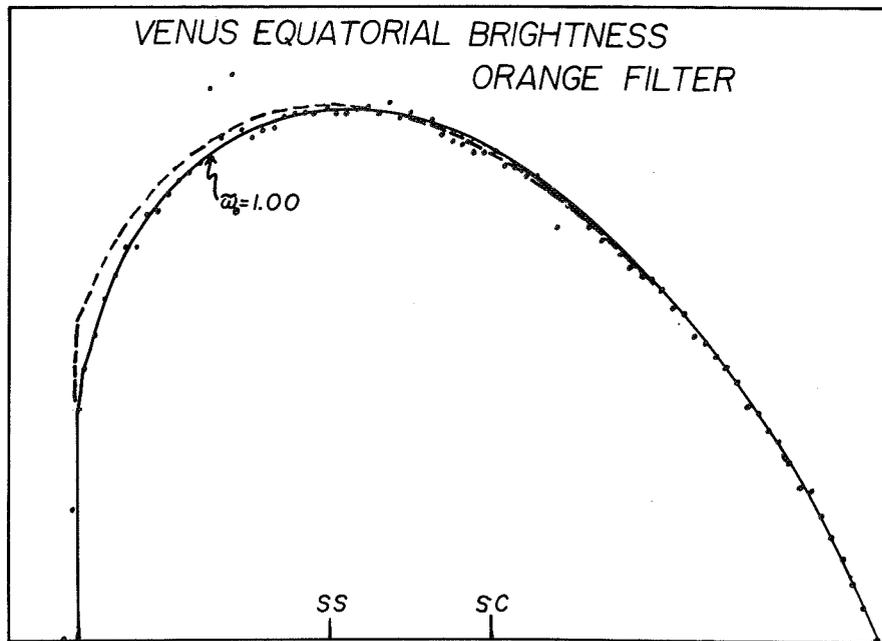
Fig. 2. Relative distribution of radiance along luminance equator of Venus in orange filter. Solid line: theoretical brightness for cloud of isotropic scatterers. Dashed line: theoretical brightness of cloud of Mie scatterers (after Lenoble, et al.).
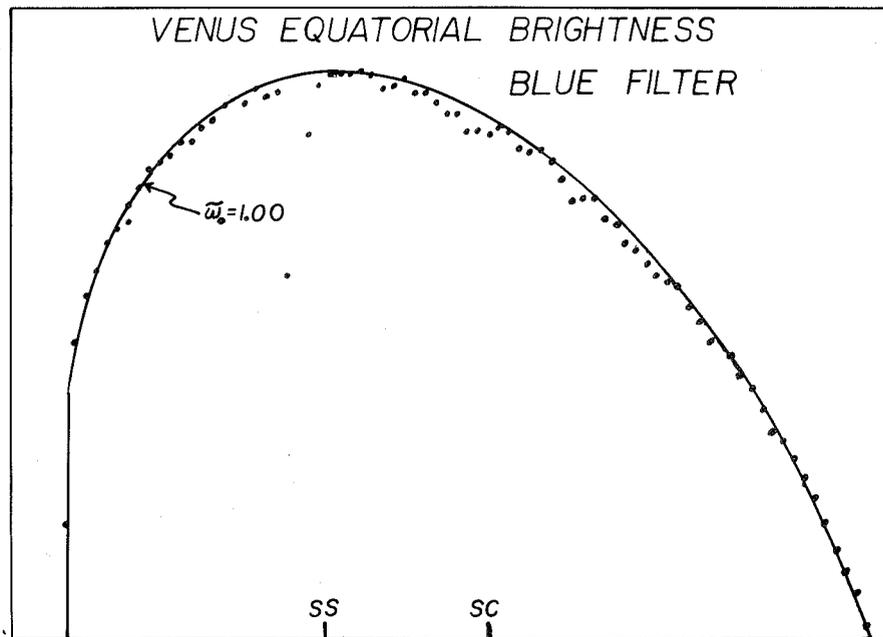
Fig. 3. Relative distribution of radiance along luminance equator of Venus in blue filter. Line: theoretical brightness for cloud of isotropic scatterers.
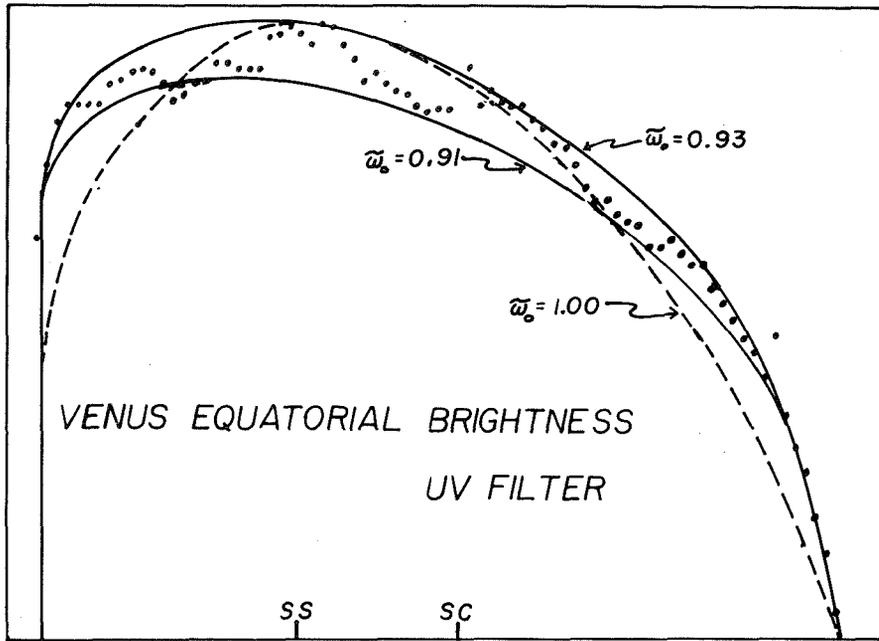
Fig. 4. Relative distribution of radiance along luminous equator in UV filter. Lines = theoretical brightnesses for clouds of isotropic scatterers of various albedos.

namely the Chandrasekhar model with isotropic scattering particles of single scattering albedo unity. And to my amazement, the results fit beautifully. I guess what this proves is that Chandrasekhar is a darned good theoretician.

This observation agrees with earth-based photometry, which gives an albedo of Venus in this wavelength region of about 92 or 93 percent. This Bond albedo requires a single scattering albedo of something like .999. The shape of the reflection function versus angle is not sensitive to differences between single scattering albedos of .999 and 1.

Figure 2 also shows some results of Lenoble and her group in France, which she'll talk about in detail in a few minutes. They tried to fit a detailed Mie scattering calculation, using Hansen particles, 2 μm in diameter with an index of refraction of 1.44; as you can see, it doesn't fit as well. This is very surprising. I'll speculate on reasons for this in a few minutes.

Figure 3 shows a scan, again along the equator, taken through the blue filter. And again, isotropic scattering with a single scattering albedo close to 1 is a very good fit to the data. The scatter is a little bit greater than for the orange filter because some of the cloud markings begin to show up in the blue.

Figure 4 shows results for the UV filter, and the scatter is much greater. A single scattering albedo of 1 does not fit at all. But the outer envelope of the points can be fit using isotropically scattering particles with a single scattering albedo of .93, and the inner envelope of the points with a single scattering albedo .91. These values would correspond to a bond albedo in the UV of .52, which again is very close to earth-based photometry. So the relative distribution of brightness more or less confirms the ground-based photometry of Irvine et al., as far as the albedo is concerned.
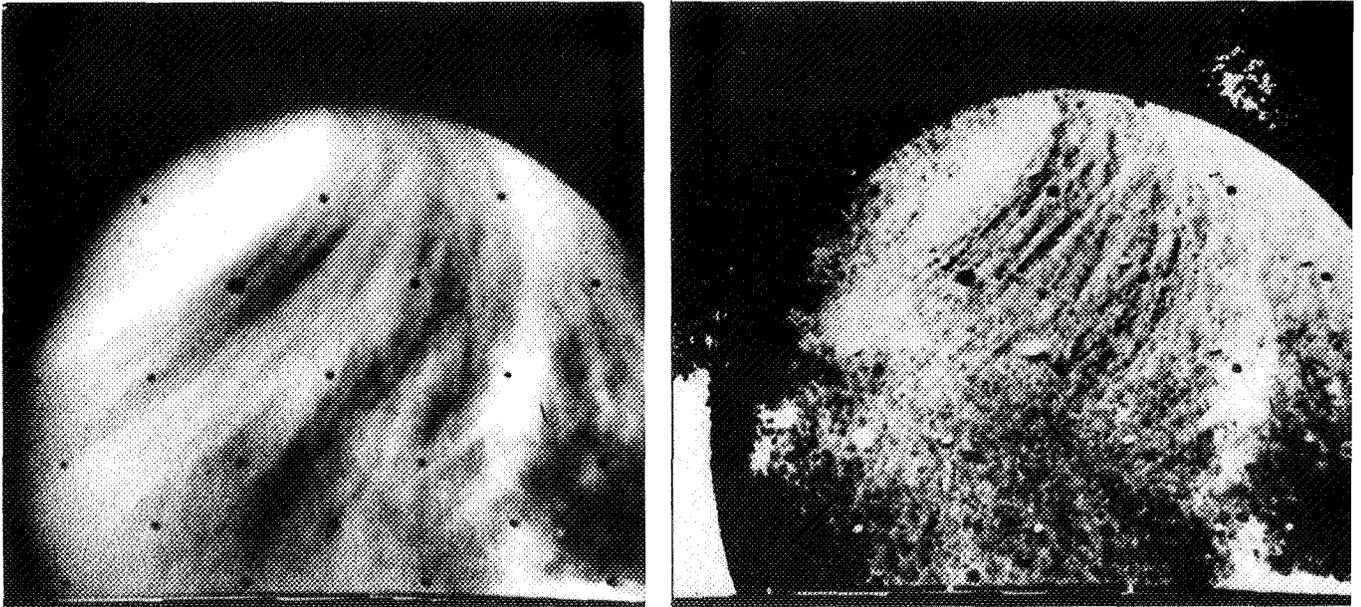
Fig. 5.   Left: UV image of Venus.   Right: Relative polarization image.

In addition to the UV filter, we had a UV polarizing filter with the polarization axis oriented parallel to the scattering plane. From these the linear polarization can be deduced, if it is assumed that the direction of polarization is either parallel or perpendicular to the scattering plane. Figure 5 shows the UV picture and the polarization picture made by super- imposing the UV and UVP pictures.

Now, I want to waffle a little bit here. We have problems with cali- bration of the polarization, in contrast to the photometry. I'm not abso- lutely certain of this picture. But what it does seem to show is that there is a correlation between the UV markings and polarization. And the sense of the polarization is just the opposite from what you would expect on the basis of most simple-minded polarization models. The darker areas have lower polarization.

This immediately demolishes one class of theories for the cause of cloud contrast, namely the theory that the contrasts are due to particle size differences. If there were a 10 or 20 percent difference in particle size, with a UV absorber dissolved or somehow in suspension in the cloud particles, then the clouds with the larger particles would be darker because of the longer pathlength and greater absorption in them. But the polariza- tion differences for such a particle size variation, based on Hansen's Mie scattering diagrams, are just the opposite of what we observe.

DR. HANSEN: What are the wavelength and phase angle?

DR. HAPKE: 3600 Å and 27 degrees.

DR. SAGAN: Can you tell why it goes the opposite way?

DR. HAPKE: One possible explanation would be what was suggested earlier, that the bright areas might be a little lower, so there is a little more atmospheric Rayleigh scattering over the bright areas. The amount of differential Rayleigh scattering required is about 50 mb. So if the cloud
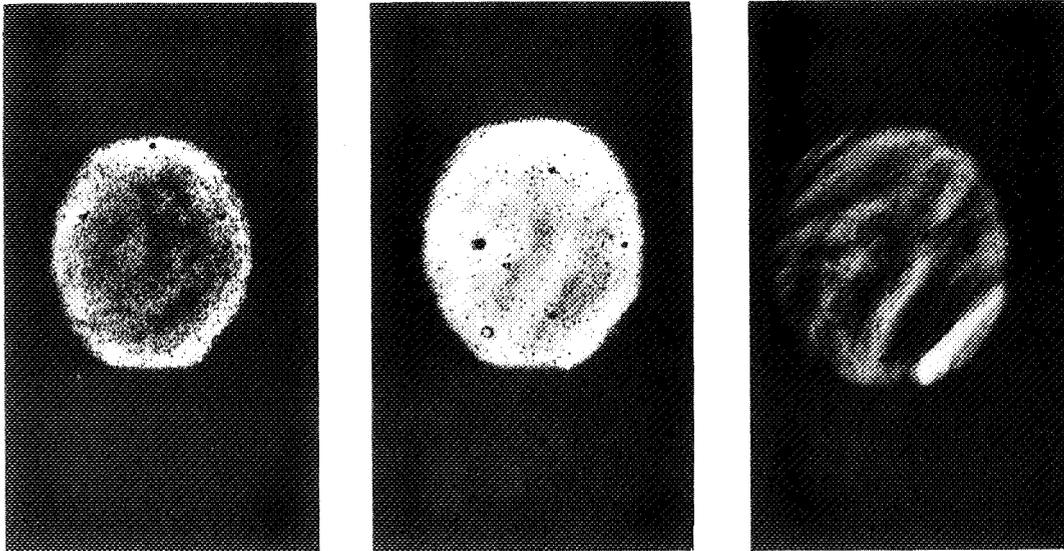
Fig. 6. Appearance of Venus through the orange (left), blue (middle) and UV (right) filters. Extreme contrast enhancement has been applied to all three images.

structure is a bit wavy, that could account for this polarization difference.

DR. SAGAN: Are you saying there is an observational result that where there are bigger particles things are brighter?

DR. HAPKE: No, I'm saying one theory for the contrasts can be ruled out. If you look at Hansen's polarization diagrams, the bigger the particle, the darker the cloud and the higher the polarization.

DR. HANSEN: The darker areas would have a higher polarization with that model because the polarization is essentially the ratio Q over I; where I is lower the polarization is higher.

DR. HAPKE: I should mention that this observation does agree with an earlier ground-based observation by Fountain [Planets, Stars and Nebulae Studied with Photopolarimetry, ed. T. Gehrels, Univ. Arizona Press, Tucson, p. 223, 1974] at LPL, who observed at 90 degree phase angle and found roughly the same effect [but cf. the discussion by Travis in his paper in the special issue of the Journal of the Atmospheric Sciences, 32, June 1975].

DR. JONES: If the bright areas are lower, that could account for the polarization?

DR. HAPKE: That's right. There are other models that could account for the polarization. For instance, you could have a second particle in addition to sulfuric acid and in greater abundance at one area than at another. That certainly cannot be ruled out, and in fact would help explain the differential photometry.

Figure 6 shows cloud contrasts in the orange, blue and UV. This really had the devil stretched out of it, so if you set your mind to firmly believe, you can even see contrasts in the orange picture. The shape is grossly

VENUS REFLECTIVITY
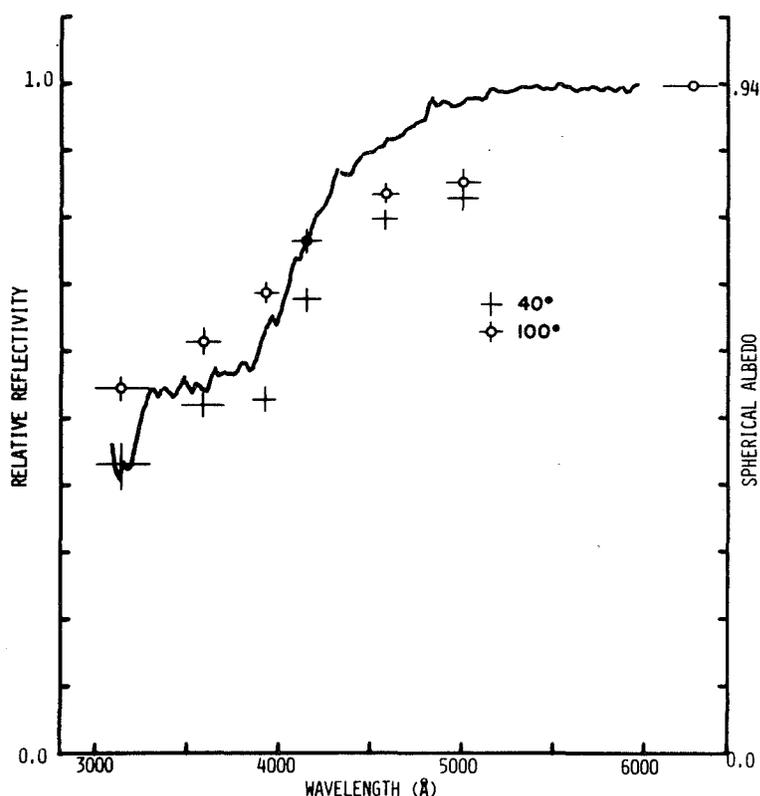
Fig. 7. Ground-based measurement of the relative reflectivity of Venus obtained at McDonald Observatory (solid line). Circles and crosses are data points of Irvine, et al.

distorted because it has been so badly stretched. The polar ring is barely visible in the orange frame. At least the gross features do occur for all three wavelengths at the same places.

Figure 7 is a ground-based spectrum recently obtained at McDonald Observatory. It goes from 3000 Å to 6300 Å, which includes the spectral regions of our orange, blue and UV filters. I think that the contrasts in all three photographs are due to a greater concentration or lesser concentration of whatever is causing this absorption. In other words, I think we are seeing the same phenomenon in all three wavelengths. It is just more accentuated in the UV because the absorption band is much deeper there.

Next, I looked at the contrast as a function of the size of the features and as a function of wavelength. Clearly the most contrasting feature on the whole planet is the polar ring. Compared to a very dark area near the polar ring, the contrast is 60 percent in the UV, 8 percent in the blue and 5 percent in the orange. Other areas on the planet of the order of 1000 km in size were much less contrasty, about 10 to 20 percent in the UV, 5 percent in the blue and 1 to 2 percent in the orange.

I could not discern any small-scale contrasts in the blue or orange. In the UV there are small-scale features of the order of 25 to 150 km in size. The maximum contrast of these was 12 percent, with the average contrast more like 4 percent. The contrast gradient is usually not more than a change of about 3 percent in brightness over a distance of about 15 km. There are no sharp contrast gradients on Venus. The maximum gradient I spotted was 3 percent contrast over 7 km.

Figure 8 is the highest resolution picture ever taken of Venus. It is within a few degrees of the terminator. If anything could be seen the ground resolution would be about 100 m. The figure is very important for what it does not show, rather than what it does show. If there were any cloud tops, cumulonimbus towers or holes in the clouds with altitude differences much greater than a few hundred meters in vertical extent and much greater than a few kilometers in horizontal extent, we certainly would have spotted them in these pictures. They are just not there.

This certainly has important implications. It argues against the class of models which assumes that the cause of the contrast in the UV features is vertical relief in the cloudtops.

The next question is, why are the contrast gradients so small? There are two explanations that might occur to you. One is that the clouds are inherently diffuse, with no sharp edges. This is rather peculiar if a condensable is the cause of the contrasts,

Fig. 8. High resolution image near the evening terminator of Venus taken through the clear filter. Image is about 100 km on a side.

because on the earth, at any rate, where there are convective zones there are fairly sharp boundaries between upflowing and downflowing currents. In some places on earth where horizontal winds spread clouds out there aren't sharp boundaries, but certainly in many places there are.

The contrasts seem to be more in the nature of some kind of mixing. If this is true, the distance required for appreciable contrast, of the order of 10 or 15 km, is a measure of the effective mixing length between whatever is causing the light and dark regions, some mixing length of the dark absorber in the atmosphere.

An alternative possibility is that the clouds do have sharp boundaries, but we can't see them because they are imbedded in a very diffuse haze with the scattering mean-free-path of the order of 10 or 15 km. That scattering mean-free-path leads to a number density of particles of about 30 cm^{-3}. That is certainly a reasonable alternative explanation.

I want to wind up by stating what I think to be two reasonable types of models for the part of the clouds that we are seeing in the pictures. Brian O'Leary is going to tell you tomorrow about some of his deductions from the limb haze layers. He finds that the optical thickness unity ($\tau=1$) for the horizontal path occurs at an altitude about 80 km, and that the scale height in this haze layer is about 1.5 km. If this scale height continues down into the cloud deck, the vertical $\tau=1$ level occurs somewhere around 70 km, where the pressure is 50 mb.

The trouble with that model is that the motions of the UV markings that Vern Suomi described earlier seem to be phenomena characteristic of the

troposphere, that is, in some turbulent layer below this diffuse strato-
spheric cloud deck where the atmosphere is very stable. If the tropopause
is identified with the glitches in the radio occultation temperature pro-
file, then it occurs at an altitude of about 63 km. If the 1.5 km scale
height continues down to 63 km the optical depth there would be about 7 - 10,
and there's just no way that we could see that deeply.

What is required is an abrupt cloud top with a scale height of 1.5 km
and then a much more gradual increase with a scale height about 10 km. In
this case the tropopause would be at about the $\tau=1.5$ level.

A wide variety of models for explaining the cloud contrasts all lead to
this same result. For example, there could be a scattering medium over a
two-tone absorbing cloud base, an absorbing medium over a two-tone cloud
base, a scattering or absorbing medium over an undulating cloud base, and so
on. All these lead to an upper limit to the optical depth of something like
1 to 1.5. You cannot exceed that optical depth and explain the cloud con-
trast. So either the optical depth 1.5 is at the tropopause, or alterna-
tively the UV markings may represent stratospheric rather than tropospheric
motions.

DR. SAGAN: We will save discussion of this paper until after the next
one which is on the same subject.

INTERPRETATION OF MARINER 10 PHOTOMETRY

Jacqueline Lenoble, University of Lille

The presentation by Lenoble is largely contained in the paper by Devaux, Herman and Lenoble which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The purpose of this work is to deduce information on the structure of the Venus clouds from the radiance of the solar radiation scattered backwards by the planet and observed at various points on the planetary disc by Mariner 10.

This radiance depends on many parameters: shape, size and refractive index of the particles; albedo for single scattering; optical thickness and vertical structure of the cloud; possibly ground reflection.

Most of our present knowledge of the Venus clouds has been deduced from the polarization measurements which are the most sensitive to the optical parameters of the particles. But the polarized light gives only information on the very upper part of the cloud, and we may expect radiance measurements to allow a deeper sounding of the cloud.

Assuming a reasonable model of the Venus atmosphere consistent with our present knowledge, we will compute, by numerical solutions of the equation of transfer, theoretical values of the radiance to be compared to the experimental ones. Then we will modify the model until the best agreement is obtained. Such a method has been applied successfully to the ground-based measurements of the relative distribution of radiance on the Venus disc obtained by Dollfus; this work has shown that the radiance is more sensitive to the variation of the parameters near the limb than in the central part of the disc. But near the limb it is difficult to have good results with the limited resolution of the ground-based measurements.

The good quality and high resolution of the Mariner 10 pictures should allow a much more detailed and accurate interpretation, even considering the fact that they correspond to a small phase angle range and only three wavelength intervals. Moreover the radiances of Mariner 10 are given in absolute energies ($W\ cm^{-2}\ sr^{-1}$) and, knowing the incident solar flux, it will be possible to get comparisons on an absolute scale, which may constitute a useful check on the detector standardization.

DR. ANDY YOUNG: I would like to ask Bruce Hapke, since he finds different polarizations in the light and dark areas, what difference in the apparent amount of gas would you expect to see in the light and dark areas. In our results when we compared different parts of the planet we found essentially no difference in the amount of gas.

DR. HAPKE: It would correspond to a difference of about 50 mb.

DR. ANDY YOUNG: That's enormous. We don't see anything like that at all.

DR. HAPKE: In your original paper you thought you saw a difference between the light and dark areas. But apparently you no longer believe that. Also, as I said, I am putting a disclaimer on the measured polarization.

I am waffling for now about it because of our problems with calibration.

DR. ANDY YOUNG: Let me briefly explain where that earlier statement of ours came from. On the days when we see more CO_2, the light features dominate the disk. On the days when we see less CO_2, the dark UV features dominate the disk. In that sense there's a correlation between light features and more CO_2, but not in the variation over the disk on any given day.

DR. JONES: Dr. Hapke, what difference in the magnitude of polarization did you observe between the light and dark features?

DR. HAPKE: The dark features are about 2 percent and the light features about 4 percent polarization.

DR. JONES: What's the wavelength?

DR. HAPKE: It is 3600 Å, and the phase angle is 27°.

DR. IRVINE: I have a question for both of you. When you compared the theory and observation did you normalize at some point or do an absolute theoretical computation?

DR. HAPKE: I initially normalized when I fit the theoretical curves; I then calculated the absolute value at the subsolar point and compared with the ground-based observations. That is where the albedos came from, so it is absolute.

DR. LENOBLE: I am surprised that your comparisons are on an absolute scale because you have a fixed albedo for single scattering of 1, which will mean a spherical albedo for the planet of 1. So you should have theoretical values much larger than the observed values.

DR. HAPKE: I normalized to the maximum. I normalized to the sub-spacecraft point when I made the plots. But I then compare the calculated value at the subsolar point with the ground-based observations. This is differential photometry after all. It was arbitrarily normalized because of initial uncertainties in absolute calibration. But it did predict the right absolute value, compared to the ground-based observations.

DR. IRVINE: You remarked that the high resolution visual observations rule out the presence of cumulus towers or that sort of thing on a scale of 100 m. Is that correct?

DR. HAPKE: Yes. Conservatively, you could say cloud top differences in altitude of about 1 km, and extents of a few kilometers, could be ruled out.

DR. IRVINE: That's over quite a region?

DR. HAPKE: Yes.

DR. O'LEARY: I want to add a comment to Andy Young's. We attempted to take a dark feature and an adjacent bright feature and follow them as they went across the disk to get the limb darkening for each, and therefore to see whether or not one was higher than the other. A preliminary look at that shows that they nicely follow a Chandrasekhar H function, so some limits will eventually be placed on the differences in the amount of Rayleigh scattering above the two regions.

DR. JONES: You [Dr. Lenoble] said you thought the effect of atmospheric curvature was unimportant. Is the magnitude of the effect just a

question of the ratio of the scale height to the radius of curvature?

DR. LENOBLE: We checked that very carefully. The thickness of the layer which is important in scattering is very small in comparison to the radius of the planet. And secondly the disagreement between theory and observation appears even at a large distance from the limb, where the incident angle and the scattering angle are of the order of 30°.

DR. SAGAN: I didn't understand your comment on your buried second layer. You needed it in order to have your theory match the photometry. On the other hand, apparently you could deduce nothing about its phase function and single scattering albedo?

DR. LENOBLE: I said we cannot deduce anything about the phase function, but we can deduce its single scattering albedo.

DR. SAGAN: What was the single scattering albedo?

DR. LENOBLE: We don't know it now.

DR. SAGAN: You mean in principle you can deduce it?

DR. LENOBLE: In principle we hope to deduce it.

DR. IRVINE: Did I understand that the discrepancy between your simpler model and the observations of the limb darkening occurred for all wavelengths of observation?

DR. LENOBLE: Yes, that's true.

DR. IRVINE: In the same sense.

DR. LENOBLE: Yes, in the same sense.

DR. GREYBER: You pointed our that there are no cumulus towers taller than 1 km. At the same time, you said an explanation of the polarization might be the bright areas being lower. Is that 1 km sufficient to give you the change in Rayleigh optical depth and thus polarization?

DR. HAPKE: No, not at the cloudtops. However that could refer to altitude differences of a lower cloud layer rather than the upper cloud layer. What we see essentially is the upper cloud layer, if there is such a thing as an upper and lower cloud layer.

DR. LENOBLE: If I may comment on your paper, you find a very good agreement between the experimental results and a model with isotropic scattering. But in order to explain the polarization measurements, we know that we need at least to see the upper layers with particles of radius of about 1 μm. I think if you put such a layer above your isotropic model, you will probably find results very similar to our results.

DR. HAPKE: I think you're right. The resolution of this discrepancy is not clear.

DR. BELTON: I am very impressed with Dr. Lenoble's deductions, because I think they represent the only direct observational evidence that we have right now for vertical structuring of the clouds. What worries me is something that Jim Hansen said this morning about polarization. Those being strong arguments, the point was made that somehow when you look at polarization you are observing a cloud that is very homogeneous in a sense, a well-defined cloud, and that it's the main cloud. It seems to me there's a slight incompatibility between these two results.

DR. HANSEN: Not necessarily. The polarization refers to particles down to optical depth unity. Photometry is certainly sensitive to greater depths than that. How thick is your [Dr. Lenoble's] top layer?

DR. LENOBLE: We did not find the optical thickness yet, but we tried the value 0.5 to find the results of the basic model. I think 0.5 is enough to explain the polarization measurements. You don't think so?

DR. HANSEN: Perhaps.

DR. LENOBLE: If we varied it between say 0.5 and 2, we could probably find agreement with both the polarization and radiance measurements.

GROUND-BASED SPECTROPHOTOMETRY FROM 3000 TO 6000 Å

Edwin Barker, McDonald Observatory

The presentation by Barker is largely contained in the paper by Barker et al. which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The relative spectral reflectivity from 3067 Å to 5960 Å for the integrated disk of Venus is presented. The reflectivity is essentially flat from 5960 Å to about 5200 Å then decreases smoothly to a flat region between 3950 Å and 3400 Å at 55% of the value at 5960 Å. Below 3300 Å the reflectivity appears to drop again to possibly another flat region between 3200 Å and 3100 Å.

Temporal changes in the reflectivity curve are of the same magnitude as the changes over a range of phase angle from 40° to 76°. These changes appear to be only in the amount of UV absorption and not in the shape of the reflectivity curve. The narrow band data of Irvine, et al. (1968) is compared to the average reflectivity curve.

The relative reflectivity curve of a dark UV feature compared to a bright UV feature has the same shape as the curve for the integrated disk of Venus. The comparison of these two curves leads to the conclusion that at least a significant amount of the UV absorption must occur above even the bright UV features.

ORIGIN OF ULTRAVIOLET CONTRASTS

Larry Travis, Goddard Institute for Space Studies

The presentation by Travis is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

Models for the origin of the contrasts in the ultraviolet images of Venus are examined in an attempt to determine the physical differences between light and dark regions fundamental to a clear understanding of the significance of the apparent cloud motions. To evaluate the meaning of the wavelength dependence of the contrasts, an improved determination of the spherical albedo curve for Venus in the $0.225 \leq \lambda \leq 1.06$ μm range is made by fitting appropriate theoretical models to the observations of monochromatic magnitudes as a function of phase angle. It is shown that, because of differences between the spectral dependences of spherical albedo and contrasts, at least one major absorber other than the one causing the contrasts is almost certainly required.

A popular model employing differential Rayleigh scattering due to variations in cloud height can be ruled out, but several classes of models are compatible with present observational evidence. The contrasts and the absorption associated with them may in fact be occurring below, within, or above the main visible cloud layer, and thus an unambiguous interpretation of the apparent cloud motions is not possible.

Ground-based observations of the polarization for the regions of contrast may permit the field of acceptable models to be narrowed. Observations planned for the Pioneer Venus orbiter and entry probes should provide the information on local cloud properties and vertical structure necessary to reveal the physical nature of the UV markings.

DR. HAPKE: I think the observations of contrast by Coffeen and by Woodman and Barker also allow you to rule out Rayleigh scattering as being the main cause of the contrasts.

DR. TRAVIS: Certainly, the spectral variation of that type of model is not in agreement with the observations. I showed that on one of my graphs.

DR. HAPKE: I would also like to point out Irvine's albedo for Venus at 5000 Å may be too low. Woodman, Barker and I have measured the reflectivity spectrum of Venus and we find an albedo at 5000 Å of about 90 percent.

DR. SAGAN: The existence of a few percent contrast in visible light suggests that all those creaky old-time visual observers who claimed to see things on Venus may not have been only seeing things in their eyeballs.

DR. ANDY YOUNG: I want to comment about the disadvantage of a model in which you put the dark stuff down below. If it is down below you would imagine that you have to clear away the light stuff above in order to see it. But when we try to isolate light and dark areas in our CO_2 observations we do not find any difference in the amount of gas. And in observations of the whole planet on the days when we see more dark areas we ought to be seeing deeper and therefore see more gas. Unfortunately, on the days

when we see more dark areas we see less gas.

DR. TRAVIS: Indeed. One would assume that result would fit best with the dark material above. In fact that's why, at least as a compromise, I would think that perhaps the best approach would be to have the dark clouds floating in the diffuse main cloud layer.

DR. IRVINE: You said that the addition of an absorbing gas would not affect the polarization, but what you must have meant was that it would affect it in the wrong way.

DR. TRAVIS: Well, it would depend on the location of the absorbing gas. If it were above the clouds it would not affect the polarization.

DR. DANIELSON: Do you have a candidate for this 1 micron absorbing particle?

DR. TRAVIS: No. As I said, it's a very restrictive model. We chose 1 micron particles simply to avoid difficulties with the polarization in the visual.

DR. SAGAN: Our last paper is by Godfrey Sill of the University of Arizona, who sometimes has had a mysterious collaborator called O-Carm, who does not seem to be here today.

COMPOSITION OF THE ULTRAVIOLET DARK MARKINGS

Godfrey Sill, University of Arizona

The presentation by Sill is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The ultraviolet dark clouds are an ephemeral phenomenon in the Venus atmosphere, apparently just near the limit of stability. The UV dark material is moderately abundant, perhaps 10%, since the contrasts between light and dark material are some 20%. The material should absorb light between 3000-4000 Å, and ideally should also have visible absorptions in the blue, as the overall spectral albedo of Venus indicates. Such a material is bromine dissolved in hydrobromic acid. Solar radiation near 2500 Å is sufficient to partially photolyze HBr into Br_2. HBr in the Venus atmosphere is inferred to have a mixing ratio of 10^{-4} versus CO_2. With a water vapor mixing ratio of 10^{-3}, droplets of hydrobromic acid are possible. These droplets would eventually evaporate in the drier upper atmosphere. The refractive index of these hydrobromic acid droplets of 52% (by weight) composition is 1.46, within the limits set by polarization studies of Venus.

DR. SAGAN: This paper seems to put us on the verge of exhausting the various halogens, but I fully expect to see a paper on hydroastatinic acid at the next conference on Venus.

DR. SILL: No, the next one would be HI.

DR. SAGAN: I know. But, that's so obvious, I'm waiting for astatine.

DR. IRVINE: I didn't understand why you thought HBr droplets might be there, since you said they would be destroyed by the sulfuric acid.

DR. SILL: HBr would be destroyed because the sulfuric acid would lower the ambient water-vapor pressure. How do the HBr droplets form in the first place? They would have to come from a region of the atmosphere where the water-vapor mixing ratio is higher, about 10^{-3}.

DR. POLLACK: Are reasonable concentrations of water vapor also consistent with very low HBr vapor pressures?

DR. SILL: You can make a droplet if the HBr mixing ratio is 10^{-4} and the water vapor mixing ratio is 10^{-3}. If either of those numbers are lower, you're out of luck. If HBr is very, very low, you'd have to have a monstrous amount of water vapor, almost as much as required to have ice.

DR. POLLACK: Isn't a 10^{-4} mixing ratio for HBr ruled out by obervation?

DR. SILL: There is no observational evidence on HBr because the absorptions are right in the CO_2 band and thus impossible to see.

DYNAMICS OF THE ATMOSPHERE

Peter Stone, Massachusetts Institute of Technology

During the past ten years, a substantial amount of evidence has accumulated about the motions in the atmosphere of Venus. We now have the ultraviolet observations, we have the spectroscopic observations of Doppler shifts, we have the measurements by the Venera probes.

However, when I look at all this data, I feel that it is more frustrating than informative. I think it is fairly clear that we do not have anywhere near enough data at this point to define a general circulation for the atmosphere.

About the best one can say from the contradictory data, it seems to me, is that in the upper atmosphere there are strong retrograde zonal velocities, that can be as large as 100 m/s. But the data seems to indicate that there is variability in those zonal velocities. And in the deep atmosphere there are substantially weaker velocities. According to the results of the Venera probes, down in the deepest part of the atmosphere near the ground, the velocities are apparently no more than a few m/s.

But that doesn't really tell us very much. If there is a zonal wind just running around the planet, which doesn't transport any heat, it doesn't tell you anything about the nature of the drives for the dynamics.

I think that the temperature measurements have actually been more informative with respect to the dynamics than the velocity measurements. From the Mariner and Venera spacecraft we now have considerable information about the temperature profile in the atmosphere.

Figure 1 illustrates the so-called standard atmosphere profile adopted for Venus by NASA based on the spacecraft measurements.
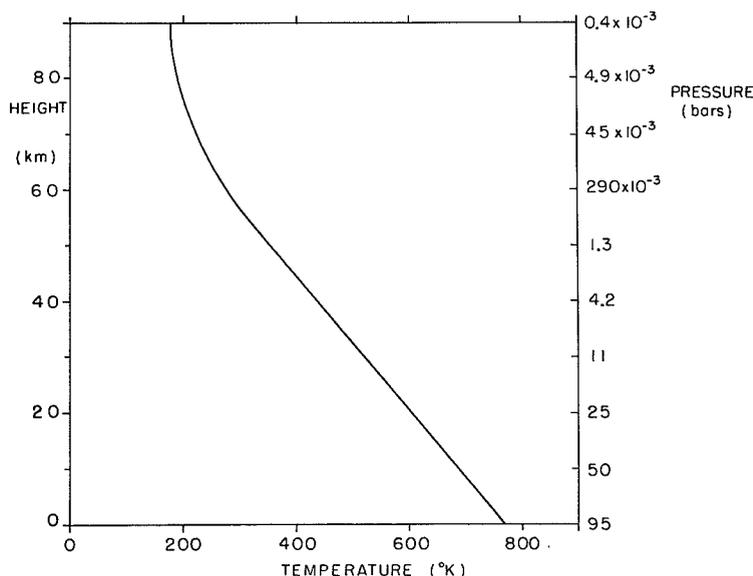
Fig. 1. *Temperature profile of the Venus atmosphere.*

The interesting thing about this profile is that in the upper atmosphere there are stable lapse rates, subadiabatic lapse rates, whereas in the deep part of the atmosphere there appear to be lapse rates very near the adiabatic lapse rate. And that is, I think, significant. It is a sign that there is dynamical activity throughout the deep atmosphere all the way to the ground. And, in fact, that is consistent with the Venera 8 measurements of solar flux, which indicate that of the order of 1 percent of the solar flux actually does penetrate to the ground. And 1 percent is not really so small when you think that the temperature drive for the motions is roughly proportional to the 1/4 power of the flux. So apparently there is a drive for motions in the deep atmosphere.

In addition to the measurements of the temperature profile, we have the thermal maps showing thermal emission from the upper part of the atmosphere, and I think these are also very instructive. The important feature is that the thermal emission shows very little contrast between both the day and the night side of the planet, and between low latitudes and high latitudes. And this, to me, is another sure sign of dynamical activity. If the atmosphere were simply in radiative equilibrium, one would expect the horizontal temperature contrast to be as large as the mean temperatures themselves. And the fact that there are not large contrasts in these thermal maps implies that there are substantial transports of heat by the motions in the horizontal direction.

So I think that the temperature observations provide us with a couple of very important constraints for any discussion of the dynamics. You must explain lapse rates which are near adiabatic in the lower atmosphere, and you must explain the apparent small contrast in temperatures in the horizontal directions, both latitudinal and longitudinal, in the atmosphere.

I think one of the most fruitful ways to think about what happens in the atmosphere, without explicitly looking at observations, is to look at the time scales which characterize the important processes in the atmosphere.

The first, prime processes, are the radiative processes. It is radiation which supplies the heating of the atmosphere and, through differential heating, supplies temperature gradients for driving motions. Fortunately, we have the calculations of Goody and Belton [Planet. Space Sci. 15, 247, 1967] for the radiative relaxation time of a carbon dioxide atmosphere. From their calculations we can find the radiative relaxation times for the Venus atmosphere, at least in order of magnitude.

Their calculations show that this radiative time scale has considerable variation. It goes all the way from 10^9 seconds in the deep atmosphere, to 10^5 seconds in the higher parts of the atmosphere. It is a considerable range.

The prime response to this differential heating is, of course, the dynamics. So you would also like to estimate a dynamical time scale, and this time scale is essentially the advective time scale, the time it would take motions to transfer a property such as heat around the planet, that is, on the global scale. So this can be written essentially as some space scale, divided by some typical velocity. Then you must say what that typical velocity is going to be.

Now a priori, about the only velocity scale you can form from the basic external scale parameters is the square root of the product of the acceleration of gravity and the scale height. The dynamical significance of that particular velocity scale is that it is the scale which the velocities would have if the temperature structure of the atmosphere were indeed in radiative equilibrium. In general, the atmosphere is not going to be in radiative equilibrium; but that, at least, is the significance of
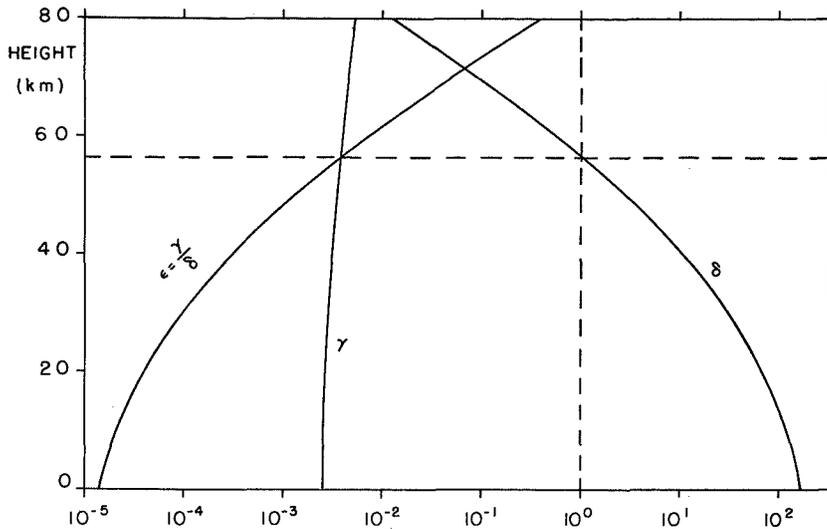
Fig. 2. Ratios of time scales in the Venus atmosphere as a function of altitude.

this particular a priori scale. There are other interpretations that you can give. It is also essentially the phase speed of external gravity waves, or, in order of magnitude, the phase speed of acoustic waves.

If you take this time scale and put in the typical scale height, acceleration of gravity, and planetary scale you find that this dynamical time scale is of the order of 10^5 seconds in the Venus atmosphere.

There is a third time scale of importance, which is a time scale imposed externally. This is the rotation period of the planet. Actually, the time scale of significance here is not the absolute rotation period of the planet, but the length of the Venus day, which we know from the radar measurements is of the order of 100 Earth days. That is 10^7 seconds, which in order of magnitude is the same as the rotation period of 244 days. For my discussion here I don't need to worry about differences of factors of two.

So we have these three basic time scales, describing the radiative and dynamical processes, and the externally imposed scale. The important things that you can conclude from these three scales involve ratios of the time scales. These tell you something about which processes are dominant.

Let me first put down definitions of two ratios, and then we will look at a figure which shows these ratios. I define δ to be the ratio of the radiative time scale to the length of the Venus day. And I define γ to be the ratio of the a priori dynamical time scale to the length of the Venus day. You can only find two independent ratios from these three scales.

Figure 2 shows these ratios calculated from the NASA standard atmosphere and from the calculations for the radiative relaxation time. You see that the variation of δ with height is large. It goes all the way from values small compared to unity in the upper atmosphere, to values large compared to unity in the lower atmosphere.

Now, since δ is the ratio of the radiative relaxation time to the length of the Venus day, in the deep atmosphere these very large values of delta imply that diurnal effects will be very weak, and, in fact, that the

length of the Venus day is too short for the lower atmosphere to cool off
at night. Consequently there will be very small temperature changes
diurnally in the deep atmosphere.

By contrast, in the upper atmosphere, where δ is of order one or less,
the diurnal effects cannot be neglected. They will be significant, and, in
general, both diurnal temperature gradients and meridional temperature
gradients can be expected to appear in the upper atmosphere.

Of course, the relative heating of low latitudes compared to high lat-
itudes is not eliminated, just because δ is small. In fact, because of the
moderating effect of the lower atmosphere, you can't be sure, a priori,
that the diurnal effects in the upper atmosphere will be as large as the
latitudinal differential heating. After all, if you have weak diurnal
effects in the lower atmosphere, and this is supplying a lot of thermal
emission to the upper atmosphere, this will tend to moderate any diurnal
effects in the upper atmosphere. I think it is interesting that the recent
analysis of the thermal maps by Ingersoll and Orton [Icarus 21, 121, 1974]
did find the result that the contrast in the latitudinal direction appeared
to be larger than the contrast in the longitudinal direction.

The largeness of δ in the lower atmosphere is nice from a theoretical
point of view. Because if you can neglect the diurnal effects, then you
have a chance of getting away with a two-dimensional model in the lower
atmosphere, with the temperature gradient and motions directed meridionally.

Now let's look at this other parameter, γ, which is the ratio of the
dynamical time scale to the length of the Venus day. This can be regarded
as a measure of essentially the rotational effect in the dynamical equations
of motion. In effect, this is an inverse Rossby number. If γ is very
small, i.e., if the dynamical time scale is very small compared to the
length of the Venus day, then you would not expect coriolis forces to play
an important role in the dynamics.

Now, as you can see in Figure 2, γ seems to be very small throughout
the atmosphere. So your first guess would be that in fact you don't have
to worry about rotational forces in discussing the dynamics.

Now, in the deep atmosphere, you have this particular combination in
which δ is large and γ is small. On the one hand the largeness of δ says
that the diurnal heating is insignificant, and therefore the length of the
Venus day is not going to enter the equation describing the heat balance in
the lower atmosphere. And on the other hand, the smallness of γ says that
the length of that Venus day is not going to enter the equations of motion
either. So you would think that, to a first approximation, the length of a
Venus day just isn't going to matter to what happens in the lower atmosphere.
Therefore, there should be a single parameter of importance there, which is
that particular ratio of time scales independent of the length of the Venus
day, and that is just this other ratio ϵ, which is γ/δ. That is
just the ratio of the dynamical time scale to the radiative time scale.

In fact ϵ is just the dimensionless parameter that Golitsyn, using
similarity theory, deduced would be the controlling parameter for a non-
rotating planet [Icarus 13, 1, 1970]. Golitsyn assumed that the essential
external parameters were the solar constant, the planetary radius, the mass
of the atmosphere, and the specific heat of the atmosphere, and also, the
Stefan-Boltzman constant, if you want to regard that as a parameter.
From those five quantities, he was able to form one dimensionless parameter,
which turns out to be just this ratio ϵ, which I introduced here in a
different way in terms of these time scales.

The dynamical significance of this parameter ϵ was first demonstrated

by Gierasch et al. [Geophys. Fluid Dyn. 1, 1, 1970] in a scaling analysis of the deep atmosphere. They showed that the size of ε tells you pretty much what you might guess a priori, given that this is the ratio of a dynamical time scale to a radiative time scale: If ε is very large, then the radiative processes dominate in determining the structure of the atmosphere, whereas, if ε is very small, the dynamical processes dominate.

As you can see from Figure 2, in the deep atmosphere there are very small values of ε and this implies a very strong control by the dynamics on the structure of the lower atmosphere.

The fact that the parameter δ ranges from very small to very large values leads me to make some definitions which are very convenient in discussing different regimes in the Venus atmosphere. I define the lower atmosphere to be that part of the atmosphere where δ>1, and the upper atmosphere to be that part of the atmosphere where δ<1. And in the NASA standard atmosphere, this division occurs at about 56 km.

In the lower atmosphere, we expect this parameter ε to be the important parameter in determining what happens. In the upper atmosphere where diurnal heating may also be important, we need two parameters, and you can take any two of these three: say δ and γ.

It is interesting to note that this division into an upper and lower atmosphere also appears to show up in the observations. The strong zonal velocities seem to be confined pretty much to layers where δ≲1, whereas in the lower atmosphere there seem to be much weaker velocities, judging from the Venera probes. It also seems to show up in the temperature profile. From the temperature profile in Figure 1, you find that the dividing level between the subadiabatic and adiabatic lapse rates is essentially this same level, at about 50 km to 60 km. I think that is very suggestive.

I will also define the *deep* atmosphere as that part of the lower atmosphere where δ>>1, say 10 or larger, which means the atmosphere below about 40 km. This part of the atmosphere, where diurnal effects really should be negligible, is where there is a good chance of obtaining a two-dimensional model of the dynamics.

Okay. That is an introduction that will enable me to describe the various analyses that have been made of the dynamics, and put them all in a single framework.

Suppose we look first at the planetary scale motions in the lower atmosphere. The first in-depth discussion of the dynamics of this part of the atmosphere was that by Goody and Robinson in 1966 [Astrophys. J. 146, 339]. They suggested basically that in the deep atmosphere where there are regions of net heating, there would be rising motions, and where there are regions of net cooling, there would be sinking motions; and this would give rise to an overturning convection cell, which in their original discussion was viewed as being directed from subsolar to antisolar point.

It is now clear that the radiative time constants are very large in the deep atmosphere, so we have to reinterpret that and say that the overturning convection cells will be directed from equator to pole, with rising motions in low latitudes, and sinking motions in high latitudes.

Figure 3 is an illustration of this kind of motion, which is generally referred to as a Hadley cell. This is an illustration taken from one of Rivas' numerical calculations, and is just to illustrate the rising motions in low latitudes, and the sinking in high latitudes. It is characteristic of these so-called Hadley cells in geophysical problems, that they are asymmetric; there generally are much larger regions of rising motions than

regions of sinking motions.

In their original discussion of the Hadley cell circulation, Goody and Robinson assumed that all the solar radiation was absorbed high up in the atmosphere. And they assumed that below the levels where absorption occurred, the balance in the equations of motion would be between dynamical cooling and heating on the one hand, and small-scale diffusion on the other hand.

If you make that assumption you tend to get very strong boundary layers and a Hadley cell with the strong motions confined near the upper layers in a boundary layer adjacent to the region where most of the solar radiation is absorbed.
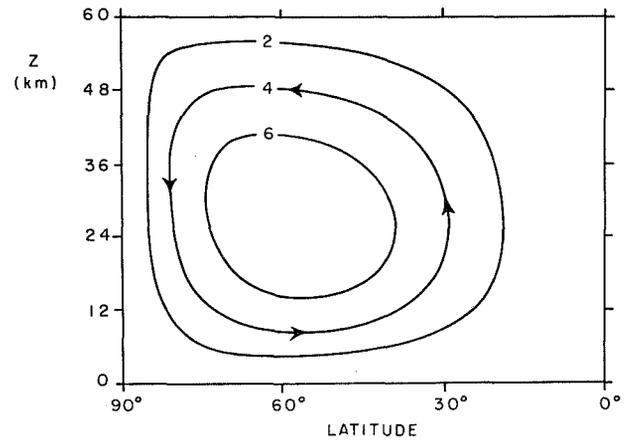
Fig. 3.   Streamlines in a Hadley cell circulation.

DR. INGERSOLL: I would like you to expand on why the motion is from the equator to the pole, if the length of the day at the surface doesn't matter?   I think it is a subtle point.

DR. STONE: It matters in one sense, and not in another.

It matters in the sense that as far as the lower atmosphere is concerned, the sun is going around so rapidly that you don't get any diurnal effects, and therefore you get latitudinal heating rather than diurnal heating.

But it doesn't matter in the sense that, because it is going around extremely rapidly, there aren't any diurnal effects, just the latitudinal heating which is not formally dependent on the length of the Venus day. It is only dependent on the differential heating between low latitudes and high latitudes.   And that is why the circulation is directed equator to pole.

In their original discussion, Goody and Robinson suggested that perhaps this Hadley cell would transport heat downwards, and account for the very high surface temperatures that are observed.   I think that that suggestion cannot be reconciled with the basic nature of the Hadley cell, which by definition is thermodynamically direct.   By definition you have warm air rising and cold air sinking, and that means that on the average you have a net transport of heat upward and that would tend to cool the lower atmosphere.

In fact, I think the chances of explaining the high surface temperatures dynamically are not very good.   If you want to explain them by a dynamical transport, you are going to have to invoke a thermodynamically indirect circulation.   And in view of the fact that the motions are thermally driven, that seems implausible to me.   On that basis, I would think that the greenhouse explanation is the most plausible one for the high surface temperatures.

DR. BELTON: I am a bit confused. You said a little earlier that the dynamics would tend to make the lower atmosphere adiabatic.

DR. STONE: I haven't really said that yet. I said the adiabatic lapse rate was one thing we want to explain.

DR. BELTON: Now you are saying the dynamics would tend to make it isothermal.

DR. STONE: Right.

DR. BELTON: Which is right?

DR. STONE: The dynamics, a Hadley cell at least, will transport heat upwards, and therefore, indeed, tend to stabilize the atmosphere. But that doesn't say what lapse rate will be produced when you put the radiation and the dynamics together. I will be getting to that in just a moment.

DR. INGERSOLL: I think that with respect to both these questions — the equator to pole circulation and the dynamical heating of the lower atmosphere — you should try to say what Goody-Robinson did wrong, or what new observations we now have to make their hypotheses no longer valid.

DR. STONE: Well, with respect to the equator to pole circulation, it is simply that at that time it was not realized that the deep atmosphere had a huge thermal inertia, which will essentially eliminate diurnal effects. There simply isn't time for the atmosphere to cool off at night.

Now as for the dynamical heating or cooling of the surface, it is a bit hard to say just what did lead them to that conclusion. They talked in their paper about the descending branch of the Hadley cell taking heat downward. And that is true in a local sense. But what I am pointing out here is that you must consider the net effect. Not only do you have descending motion, but you have rising motion, and since it is thermo- dynamically direct, on the average you have a higher temperature for the rising motions than for the descending motions. So if you average across a level surface on net you are going to have an upward transport of heat, even though in a local region you may have a downward transport of heat.

DR. GIERASCH: What Goody and Robinson did was assume that there was a small thermal conductivity in the deep atmosphere which makes it easy for the cells to be thermodynamically indirect. So I think that their model could be perfectly consistent.

DR. STONE: But their cells are not thermodynamically indirect.

DR. GIERASCH: It had to be thermodynamically indirect, just as you said.

DR. STONE: But diffusion will not do that, because diffusion will just eliminate gradients.

DR. GIERASCH: If diffusion is weak enough, then the cell does not have to do much work. You don't have to have a very strong engine to fight against the leakage of heat upwards by diffusion.

DR. STONE: But you still need a downward transport to fight against leakage.

DR. GIERASCH: But very, very small, if diffusion is very, very small.

DR. STONE: But how are you going to get that downward transport if you have warm air rising and cold air sinking?

DR. GIERASCH: It would be thermodynamically indirect.

The comment that I want to make is that if the thermal diffusivity is very, very small then it is quite easy for the system to be thermodynamically indirect.

DR. STONE: It is easier.   I would not say it is easy.   For thermally driven motion, I would be very skeptical.   Let me leave it at that.

I have expressed my opinion, I think it unlikely that the motions are thermodynamically indirect.   If they are thermodynamically direct, then they would tend to stabilize the lower atmosphere.

The other interesting point about a direct circulation like this, is that if you wish to have a substantial poleward transport of heat to explain the small contrast in the thermal emission, then the poleward branch of this Hadley cell must on average have a higher potential temperature than the equatorward branch.

And that means that the Hadley cell must on average see a subadiabatic lapse rate, at least slightly.   Let's just leave that for the moment and come back to it later.

It seems to me that the general idea of a Hadley cell for the deep atmosphere is very hard to argue with since it is essentially a statement of direct thermodynamics, and since in addition many experiments with laboratory fluids show that nonrotating systems subject to differential heating do have overturning motions of this kind.

As a matter of fact, all the discussions of the deep atmosphere have implicitly or explicitly assumed a Hadley cell circulation.

There have been two types of analyses of the circulations in this Hadley cell.   One kind is the kind I would refer to as a scaling analysis, in which you take the equations of motion and the equation of heat balance, and do not attempt to solve them in any detail, but simply look at what balances will occur in those equations.   And on that basis you deduce the orders of magnitude of the important dynamical parameters.

The original discussion of Goody and Robinson made an analysis like this, in which, as I already indicated, they assumed that the balance in the deeper part of the atmosphere was between dynamical cooling and heating and small-scale diffusion.

Well, it seems to me that that kind of balance is not too plausible in view of 1) the Venera 8 measurements did show a drive for dynamics in the deep atmosphere, and 2) the Venera measurements indicated an approximately adiabatic lapse rate, which is also a sign that you are getting local heating down in the deep atmosphere.

It seems to me that the more plausible balance is the kind that has been assumed in the more recent scaling analysis, by Gierasch, Goody and Stone [Geophys. Fluid Dyn. $\underline{1}$, 1, 1970] in which they assumed that the dynamical cooling and heating in the deep atmosphere is balanced by radiative heating and cooling.   If you make that assumption, you can then look at the heat equation and by assuming that you have a global balance, you can come out with an estimate of a typical scale for the variations of potential temperature.

Now in the original scale analysis by Gierasch et al. the assumption was made that there was just one characteristic scale for these variations,

whether you are looking at merid-
ional variations, or vertical
variations. Actually, you don't
have to make that assumption.
More recently I looked at the
problem when you relax that as-
sumption, and then instead of
obtaining a single global
balance equation from the heat
equation, you obtain two equa-
tions, assuming that you have a
balance when you average that
equation either in the meridional
direction of in the vertical di-
rection. Then you have two
equations for two scales of the
potential temperature field. One
is characteristic of the merid-
ional variations, and the other
is characteristic of the vertical
variations.

Now because the parameter ε
is so small, and it is the main
parameter which enters the equa-
tion, it is easy to solve the
equations for the order of mag-
nitude of the typical meridional
and vertical scales.

In order to solve the
equations you have to specify in
addition how much solar heating

Fig. 4. Static stability produced
by a Hadley cell circulation vs. the
static stability of the radiative
equilibrium state.

actually gets down into the deep atmosphere, and we don't know that at this
point. So we can leave that as a parameter and find solutions as a func-
tion of how much solar radiation is being absorbed in the deep atmosphere.
The results of that scaling analysis are shown in the next two figures.

In Figure 4 a parameter is plotted which effectively measures how much
solar heating does get into the lower atmosphere. $\Delta\theta$ is essentially the
vertical contrast of potential temperature across the deep atmosphere,
normalized by some mean temerature.

On the x-axis is plotted the value that contrast would have for
radiative equilibrium. If this quantity is zero, that means you have just
an adiabatic lapse rate, when you are in radiative equilibrium. Positive
values correspond to static stability in the radiative equilibrium state
and negative values correspond to static instability. Just to orient you,
a value of +0.2 roughly corresponds to the value this parameter would have
if you had an isothermal atmosphere.

On the y-axis I have plotted this same measure of static stability
but now the effects of the Hadley cell on the static stability are included,
so it shows the balance between the Hadley cell fluxes and the radiative
fluxes.

You can see there are essentially two possible states for a Hadley
cell in the deep Venus atmosphere. On the one hand if there is not enough
solar radiation penetrating down to give a greenhouse effect, i.e., if the
radiation would not give you those high surface temperatures by itself,
then the Hadley cell has virtually no effect on the static stability, and
there is essentially the same lapse rate as in radiative equilibrium.

On the other hand, if there is sufficient greenhouse effect to explain the high surface temperatures, the Hadley cell has a very strong stabilizing effect on the lapse rates, strong enough to eliminate the statically unstable lapse rate in the radiative state and give something which is very nearly adiabatic. The difference from adiabatic depends on the parameter ε, and because it is so small you get a very small difference. I put in typical values and calculated for the static stability only 4×10^{-4}, which in dimensional terms corresponds to a static stability of 0.01 °K/km. So, the second state is essentially an adiabatic state, but very slightly subadiabatic.

I think there are two interesting points about this. One, it does show that you need not invoke small scale convection to get an adiabatic lapse rate. You can do it with the large scale overturning in a Hadley cell. And, two, the fact that it does come out to be subadiabatic is interesting in view of the Venera probe measurements which did not detect any turbulence below about 40 km.

Here is a possible explanation of that. Maybe the lapse rates are slightly subadiabatic. The degree of subadiabaticity here is very small, too small for the Venera probes to have measured.

Fig. 5. Equator to pole temperature contrast produced by a Hadley cell circulation, vs. the static stability of the radiative equilibrium state.

Figure 5 shows the solution from the scaling analysis for the meridional contrasts of the temperature, again expressed as a fraction of the mean temperature versus the same parameter describing how much solar radiation is absorbed in the atmosphere. Again there are essentially two kinds of states. The statically stable states have very, very small contrasts because in those states there is large static stability, which means there is a large potential temperature difference between the poleward and equatorward branches of the Hadley cell. That makes the dynamical transports very efficient, and they virtually wipe out the temperature difference.

In the adiabatic state, there are much larger temperature differences. Still, they are very small because of the smallness of the parameter ε. The dynamical control is very strong when ε is very small. ε is $\sim 10^{-5}$ in the deep atmosphere, and that is what dictates the small temperature differences there.

If you assume, based on the observations, that the only consistent state is the adiabatic state, and if you take this typical value for the meridional temperature gradient, you can then use the equations in the scaling analysis to deduce typical velocity scales. In this way you find that typical horizontal velocities are ~ 2 m/s, and typical vertical

velocities are ~½ cm/s. That 2 m/s velocity is at least consistent with the small velocities that were measured by the Venera probes in the deep atmosphere.

The second kind of analysis of the deep atmosphere motions have been integrations of numerical general circulation models. There have been four of these that have been presented.

There was a calculation presented by Turikov and Chalikov [Izv. Atmos. Ocean. Phys. 7, 705, 1971] in which they allowed the motions in the deep atmosphere to be three dimensional. They divided the deep atmosphere essentially into two layers to simplify the vertical integrations.

But unfortunately in their analysis they assumed that the thermal inertia of the lower layer was zero, so that the lower atmosphere would respond instantaneously to solar heating. In other words, they assumed $\delta = 0$. And that is really not relevant for the Venus atmosphere. δ is very large, and the diurnal effects will not be strong when δ is large. As you might expect, in their calculation they did get strong diurnal effects. But I don't think that calculation is very meaningful.

The other three analyses were calculations presented by Hess [The Atmospheres of Venus and Mars, Gordon and Breach, 1968], Sasamori [J. Atmos. Sci. 28, 1045, 1971], and Rivas [J. Atmos. Sci. 30, 763, 1973].

In their calculations these people all took a similar approach to the problem. They assumed that the motions were two-dimensional; they specified an arbitrary initial condition; and then they integrated the equations of motion and of heat balance in time. They followed the integration until it appeared that the solutions had reached equilibrium; and then they looked at the equilibrium state.

In these integrations, the apparent equilibrium was attained after times of the order of a few hundred days — 200 to 400 days. And as Rivas pointed out, this is a bit suspicious, because of the very long radiative relaxation times in the deep atmosphere. The time scale there is about 30 years.

So you really have to ask, are those calculations indeed achieving equilibrium? This is another problem that you can address through scaling analyses. And when I did the scaling calculations, I also looked at this problem. You can inquire what the time scale is for an arbitrary initial condition to adjust to equilibrium.

Not surprisingly, you again find essentially two possibilities, depending on whether the atmosphere is in the region of statically stable states, or in the region of adiabatic states.

In the statically stable states, as it turns out, the adjustment time is the radiative relaxation time. In all those numerical calculations, the assumption about the amount of solar heating in the deep atmosphere was such that in radiative equilibrium you would have these statically stable states. And that, to me, is an indication that the calculations had not reached equilibrium. So you have to interpret those calculations with a grain of salt.

On the other hand, in the adiabatic states, the indications from the scaling analysis are that there is a different adjustment time, which is essentially a dynamical time scale, of the order of weeks.

And this seems to be the most relevant state for the Venus atmosphere, given the observations, such as the observations of an adiabatic lapse rate.
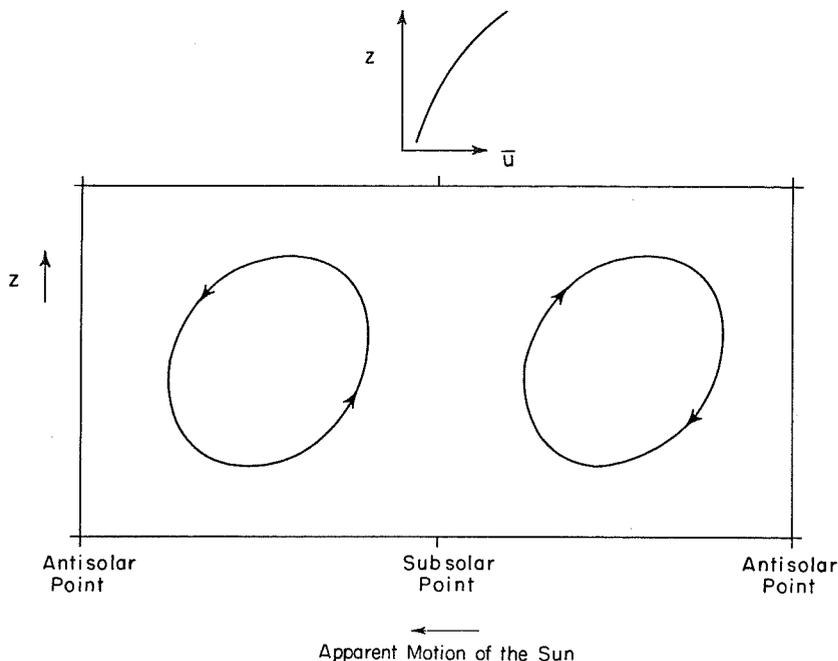
Fig. 6. Schematic diagram of the streamlines in tilted convection cells which drive an upper level zonal motion opposite to the apparent solar motion.

This is a state that has not yet been studied with the numerical general circulation models, and I think it would be very interesting to do so, and it might be particularly easy because of the shorter adjustment times.

DR. SEIFF: Would you comment on the absolute magnitude of these contrasts? Horizontally, the contrasts seem to be less than 0.1°C. Do these values lead to the 2 m/sec velocity?

DR. STONE: Yes. And that, at least, is consistent with the Venera probes' not finding any significant horizontal variations in temperature. This is a sign of a strong dynamical control.

DR. SEIFF: What happens if, on Pioneer Venus, we find temperature contrasts of the order of a few degrees?

DR. STONE: Why, I'd be disappointed.

DR. SEIFF: Disappointed?

DR. STONE: Yes, because obviously a different mechanism must be invoked to explain that.

DR. BELTON: Don't you think you would be excited?

DR. HESS: First, disappointed. Then excited.

DR. STONE: Yes. I am speaking as a theoretician here.

Let me turn now to a discussion of the planetary scale motions in the upper atmosphere.
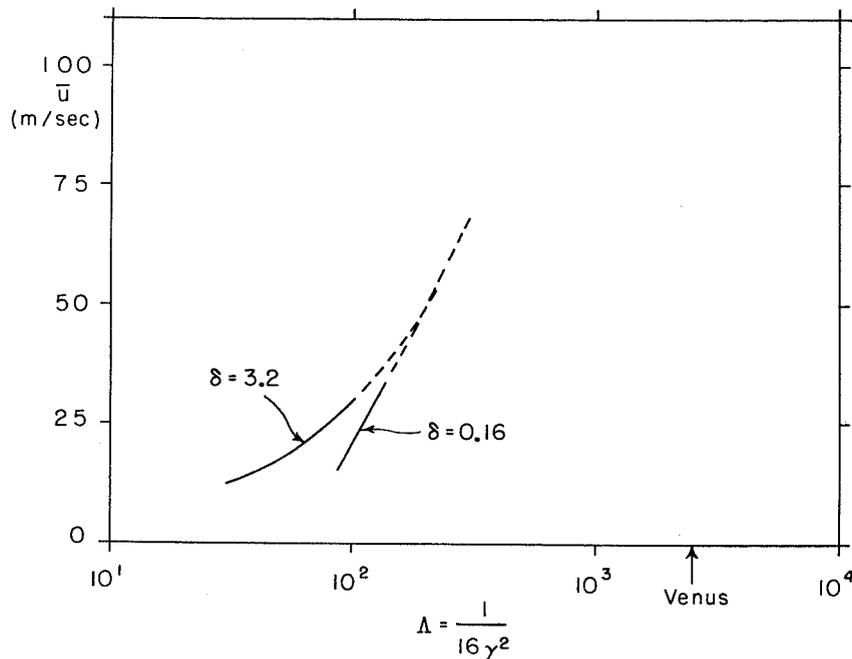
Fig. 7. Retrograde zonal velocities produced by the moving flame mechanism.

All the discussions of the dynamics of the upper atmosphere have concentrated on the apparent 100 m/s retrograde velocities, and have tried to explain these strong retrograde velocities.

In addition, the discussions have focused on the smallness of the parameter δ in the upper atmosphere, which indicates that diurnal heating is important. They have explored the consequences of diurnal heating in the upper atmosphere, even to the extent of making the discussions two-dimensional so that the meridional gradients and the motions in that direction are neglected. Quantitatively, that probably isn't accurate. But at least it makes sense, I think, to start off by looking at the simplest kinds of motions, namely the two-dimensional ones, to see if you can understand them and see how far you can get in explaining things like the 100 m/s velocities.

The first suggestion for the cause of the 100 m/s velocity was the suggestion by Schubert and Whitehead of the so-called moving flame mechanism.

Figure 6 illustrates the moving flame mechanism, at least in its simplest form. In its simplest form, there is an apparent motion of the sun in one direction, the solar absorption occurs in the lower parts of the upper atmosphere, the absorbed energy is then transmitted to higher levels by some sort of diffusion, and, because of the time lag for the diffusuion to higher levels, the isotherms that are produced have a slant in the direction opposite to which the sun is apparently moving.

This slant of the isotherms would tend to drive convection cells, which would also have a slant in the same direction. And with such a slant, you will get a correlation between the zonal and vertical motions that is positive, as they are diagrammed in Figure 6. So you get an eddy transport of retrograde momentum upwards, and the convection cells tend to produce a zonal velocity with a shear, with the strong retrograde motions in the upper part of the atmosphere.

The Venus atmosphere is not quite that simple. One problem that appeared early on was the fact that the local heating has the shortest

response time in the upper part of the atmosphere, rather than in the lower part. And that would tend to produce a tilt in the opposite direction.

However, Young and Schubert [Planet. Space Sci. 21, 1563, 1973] succeeded in demonstrating that you could still get the proper tilt for these cells, if you include the effects of stratification, which should be very important in the upper atmosphere because of the subadiabatic lapse rates. And when they included this effect, they were able to calculate the effect of the eddy transport and deduce the kind of zonal motions that would be produced, using the so-called mean field equations, neglecting the self-interactions of the convection cells.

The results of Young and Schubert's calculations are shown in Figure 7, which is adapted from their paper. In the figure, the retrograde zonal velocity produced by the moving flame mechanism is plotted. In general this depends on two parameters. Those two parameters can be taken to be any two of the three I discussed earlier.

δ is one of the parameters, and Young and Schubert did calculations for two values of δ. The smaller one in Figure 7 is the one that probably is correct for the upper Venus atmosphere.

For the other parameter you can take γ, or, as they did, you can take the parameter they called Λ, which is related to γ in the manner indicated in Figure 7.

Figure 7 shows the solutions as a function of Λ. In solving the equations, Young and Schubert had to use an iterative technique, which unfortunately turned out to be very slowly converging for the larger values of Λ. Therefore, they could not find solutions for the larger values of Λ.

That is unfortunate, since the value of Λ in the upper Venus atmosphere appears to be about an order of magnitude larger than the ones for which they found solutions.

What you can say is that the trend is encouraging. But it seems to me a bit risky to extrapolate, first of all because you are extrapolating over more than one order of magnitude, and secondly because in the solutions indicated by dotted lines, you cannot really neglect the self-interactions. And therefore, even for those values of Λ, their solutions may be inadequate.

So, although I think the results are encouraging, you have to take the position that it has not yet been demonstrated that this mechanism will produce the 100 m/s velocities under the conditions appropriate to Venus. It would be very interesting to see some calculations which extended this work to large values of Λ.

There have been other suggestions for the 100 m/s velocity in the upper atmosphere. None of them have been worked out as thoroughly as the moving flame mechanism. There is, for example, Thompson's [J. Atmos. Sci. 27, 1107, 1970] suggestion of an instability mechanism very closely related to the moving flame mechanism. To imagine this mechanism you can start with two diurnal convection cells that do not have a tilt, and then superimpose on them a perturbation which has a mean shear. That mean shear will tend to tilt the cells, and the Reynolds stress which results tends to reinforce the shear in perturbation, and if the instability is strong enough, then you can build up a strong velocity.

I don't think any of the calculations of this mechanism have been conclusive. Thompson's own analysis did not have adequate resolution in the circumstances where he found an instability. There have been more recent calculations by Busse [J. Atmos. Sci. 29, 1423, 1972] which do not

have this problem, and which, in fact, indicated that the mechanism would not work on Venus because he found that the instability did not occur unless the atmosphere were as deep, in order of magnitude, as the scale of the planet.

But even Busse's calculations are not conclusive, because he had to make assumptions, such as a zero Prandtl number, in order to solve the equations. So I think it is very difficult at this point to say whether such an instability will work, and I will leave it at that.

Another suggestion has been the one put forward by Gold and Soter [Icarus 14, 16, 1971], in which they point out that the diurnal heating in general will give rise to a semi-diurnal thermal tide. Associated with this semi-diurnal tide will be a second harmonic of the mass distribution of the atmosphere. This second harmonic will be acted on by the solar tidal forces, and this will provide a torque to accelerate the atmosphere in a zonal direction.

Well, again, it is very hard to evaluate this mechanism as to how strong a velocity will result. And the sign of the velocity, too, depends on the strength of the semi-diurnal thermal tide, and its phase, and on the effective viscosity of the atmosphere. We just don't know what these quantities are, so it is very hard to assess the plausibility of these suggestions.

I am running out of time, so I won't be able to say much about the small-scale motions. I think that is becoming a particularly interesting topic now that we have the Mariner 10 observations, which for the first time give us some observation of what is happening on a smaller scale.

There are a number of instability mechanisms that one could look at. And, in addition, the appearance of all those wave-like phenomena in the upper atmosphere suggests that it would be very interesting to look at the propagation characteristics of waves in the upper atmosphere. I believe that there will be some talks later in today's session which look at some of those questions. Perhaps at that time it would be appropriate to discuss some of these small-scale motions.

Let me just summarize briefly where I think we stand as far as the large-scale motions are concerned.

I think that for the deep atmosphere one can say that the attempts to explain what is happening are satisfactory, compared to the observations, although admittedly the observations are very few.

The Hadley cell hypothesis is capable of explaining the adiabatic non-turbulent structure of the deep atmosphere, the apparent lack of any horizontal temperature contrast, and the order of magnitude of the horizontal velocities.

The most crucial assumption in the Hadley cell hypothesis, I think, is that it assumes that there is sufficient solar absorption in the deep atmosphere to explain the high surface temperatures by the greenhouse effect. And that is the thing, I think, that would be most interesting to check. If we could get some observations, or somehow deduce how much solar radiation actually is being absorbed in the deep atmosphere, I think that would immediately tell us whether the Hadley cell hypothesis is completely viable or not.

It would also, of course, be valuable to get measurements of velocities in the deep atmosphere sufficient to define the general circulation. But that would probably require a large number of probes.

As far as the upper atmosphere is concerned, there are lots of suggestions for the cause of the motions. It is not clear that any of the suggested mechanisms will actually work under the actual conditions in the Venus upper atmosphere, although I think that the moving-flame mechanism looks promising and it would certainly be worthwhile to extend Young and Schubert's calculations to large values of Λ.

One thing that I think is going to have to be dealt with as far as the upper atmosphere is concerned, eventually, is the three-dimensional nature of the motions there. You are very likely to have meridional gradients as well as longitudinal gradients, and the meridional gradients may actually dominate. For this purpose you are likely to have to resort to numerical general circulation models, and I believe we will be hearing today about some attempts to look at the three-dimensional motions with numerical general circulation models.

DR. BELTON: Is the Goody-Robinson cell direct or indirect?

DR. STONE: They didn't say in their original discussion. I don't see how you can get a thermally driven cell that is anything but direct.

DR. GIERASCH: That is an interesting point which may be of only academic interest, because some sunlight does reach the surface. If the radiation and conduction are both very weak, then they don't filter much heat upwards along the adiabatic lapse rate. It is certainly energetically possible to pump energy downwards with the convection cell, sufficient to balance a small leakage upwards.

DR. STONE: With an indirect cell, yes.

DR. GIERASCH: They didn't actually solve the equation that way, but that would be the way it would work.

DR. STONE: This is then a third picture of the overturning, with the rising air being slightly colder than the descending air.

DR. GIERASCH: That is right.

I think Andy Ingersoll is right, that the question of subsolar versus antisolar is a subtle one, because once you have said that the radiative time is very long, you put that aside. And the question is then the dynamical response time versus whatever the time scale is in the forcing.

So the question is the dynamical response time versus the time for the sun to move. And I think your argument about the radiative time constant being important in that context may be wrong.

DR. STONE: I am not sure I follow what you are saying.

DR. GIERASCH: You said the circulation was equator to pole because the radiative time constant is much longer than a day.

DR. STONE: Right.

DR. GIERASCH: And I am saying that you might really need to compare the dynamical time constant to a day.

DR. STONE: Yes, I also did that. That was another part of the argument. The length of the day is not important in the dynamical equation.

Let me say it in a little different way. In the deep atmosphere, if you try to set up a radiative equilibrium state with just the thermal time

response due to the thermal inertia, with no dynamics at all, you will get
a radiative equilibrium state which has extremely small longitudinal temp-
erature gradients, and very large latitudinal temperature gradients, and
that is the basic drive for dynamics, although to be sure the dynamics
modifies it considerably.

DR. HESS: I would like, after all this time, to get off my chest one
of the things that bothered me about the Goody-Robinson model. Insofar as
it purports to be an explanation of the high surface temperature, it seems
to me to be a circular argument, because it invokes a large thermal dif-
fusivity. But you don't expect to get a large thermal diffusivity unless
you have a lapse rate near the adiabatic one.

So it seems to reduce to the statement that if you have an adiabatic
lapse rate you have an adiabatic lapse rate.

DR. ANDY YOUNG: I would like to throw another rock at Goody and
Robinson. This is a historical rock.

The model which has become known recently as the Goody-Robinson model
was proposed by Arthur Clayden in 1909 [Monthly Notices 69, 195], and he
discussed it in considerable qualitative detail, although not quantitatively.
And he even deduced from his discussion, quite correctly, that the rotation
speed of the planet had to be longer than several weeks, but shorter than
the planet's year.

Also, in the same paper, he gives what would be regarded today as
quite an accurate description of Tiros photographs, something like 50 or
60 years before there were Tiros photographs. I heartily commend people to
go back and read Clayden's 1909 paper. I think you will find it is quite
up to date.

DR. LIMAYE: Is it true that most of the deep circulation models assume
that the planet is not cooling off, that is, there is no internal heat
source?

DR. STONE: Yes. The assumption here was no internal heat source, or
at least a very weak internal heat source.

DR. LIMAYE: Do we know if that is a valid assumption?

DR. SAGAN: The Venera 8 gamma ray spectrometer deduced an abundance
of radioactive potassium comparable to that in the crust of the Earth, from
which one can deduce that the geothermal heat flux on Venus ought to be
about the same as on the Earth. And therefore negligible, right?

DR. STONE: Yes, right.

DR. INGERSOLL: But Hansen and Matsushima proposed that an internal
heat source, however weak, could keep the deep atmosphere hot if you have
a thick enough infrared blanket.

DR. STONE: Yes, okay. In the context of the model here, I think you
would need an internal heat source that was of the order of one percent of
the solar flux.

DR. SAGAN: I don't think the culprits are here. But they required an
internal energy source, which is at least an order of magnitude more than
on the Earth.

DR. BELTON: They took an internal heat source of a few times 10^{-6}
calories per square centimeter per second, which I think is comparable to

that on the Earth. And what they did was ask: what optical thickness would the dust have to have for thermal radiation? They came out with something between 10^4 and 10^5. Then you get into problems with the penetration of solar flux to the surface.

DR. POLLACK: Yes, Mike, but the point is that the Venera 8 measurements give us an estimate of the solar flux at the surface of Venus, which is roughly one percent. So it is that number that you should compare with an internal heat source. Carl's statement is that the internal heat source is negligible compared to the solar flux at the surface.

DR. JONES: I don't believe we know the surface albedo of Venus at all.

DR. POLLACK: I think it would be very surprising if the value was .99, which would be required to make the absorbed solar energy comparable to the internal heat source.

DR. BELTON: I will make one final remark about this. If you really want to believe the in situ measurements, which show almost a degree per kilometer super-adiabaticity in parts of the lower atmosphere, you could argue for a considerable internal heat source. We wouldn't be able to detect it from the albedo measurements we presently have, up to levels of a few percent of the absorbed solar flux.

NUMERICAL CIRCULATION MODELS

Eugenia Kalnay de Rivas, Massachusetts Institute of Technology

The presentation by Kalnay de Rivas is largely contained in her paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June, 1975]. The abstract of that paper follows:

The results of 2-dimensional simulations of the deep circulation of Venus are presented. They prove that the high surface temperature can only be explained by the greenhouse effect, and that Goody and Robinson's dynamical model is not valid. Very long time integrations, up to a time comparable with the radiative relaxation time, confirm these results. Analytical radiative equilibrium solutions for a semigrey atmosphere, both with and without an internal heat source, are presented. It is shown that the greenhouse effect is sufficient to produce the high surface temperature if $\tau_T^* >> 100$, and $S = \tau_S^*/\tau_T^* \lesssim 0.005$. This result is still valid in the presence of an internal heat source of intensity compatible with observations.

A 2-D version of a 3-D model is used to test the validity of the new mechanism proposed by Gierasch to explain the 4-day circulation. Numerical experiments with horizontal viscosities $\nu_H = 10^{11} - 10^{12}$ cm^2/sec^{-1} failed to show strong zonal velocities even for the case of large Prandtl numbers. It is observed that the dissipation of angular momentum introduced by the strong horizontal diffusion more than compensates for the upward transport of angular momentum due to the Hadley cell.

Preliminary 3-D calculations show a tendency to develop strong small scale circulations.

DR. FELS: What is the vertical resolution at the top of your model?

DR. RIVAS: Somewhat less than 1 km.

DR. FELS: If you spin the atmosphere up from rest the gravity waves will eventually be dominated by the main mode, the Venusian diurnal mode, which has an extremely short wavelength in the vertical, about 600 m, in fact.

You obviously may be having trouble resolving that. That can be important. That may be a very important mode.

DR. RIVAS: Yes.

DR. STONE: Your calculations so far are essentially for the lower atmosphere?

DR. RIVAS: I did make a calculation letting the top go up to about 13 mb for the two-dimensional model.

DR. STONE: You didn't show any results for that case?

DR. RIVAS: No.

DR. STONE: Okay. But it is in the upper atmosphere where you would look for gravity waves.

What were the typical meridional velocities you found for your case C?

DR. RIVAS: In all cases, in the region of the Hadley circulation, the meridional velocities were between 1 and 5 m/s.

DR. STONE: Well, that is nice, in light of Dr. Suomi's results.

DR. RIVAS: Yes, it agrees with both of you.

DR. SAGAN: In your model in which you succeeded in making the surface 750K what were the typical velocities in the lower few kilometers.

DR. RIVAS: About 50 cm/sec. So of the order of 1 m/sec.

DR. SAGAN: And what was the latitudinal variation of that velocity?

DR. RIVAS: Well, the maximum was in the middle latitudes.

DR. INGERSOLL: I think these are very impressive calculations that you have done. I want to make sure I understand what you mean when you say the Goody-Robinson model doesn't work. You started off with an initially adiabatic atmosphere?

DR. RIVAS: Yes.

DR. INGERSOLL: And even when you don't prejudice against circulation in the deep atmosphere, the circulation somehow doesn't penetrate?

DR. RIVAS: That is right. I should mention also that similar results were first obtained by Hess.

DR. HESS: But with a very different kind of model.

DR. JONES: Did the sign of the meridional component of the velocity change?

DR. RIVAS: No, except in the polar cell, the indirect cell.

I should also mention that the meridional temperature gradients are between 0.5K and 2K.

DR. SOMERVILLE: A lot of models like this have been constructed in simulated laboratory flows driven by horizontal temperature gradients, and they are all very sensitive to boundary conditions and viscosities. Have you tested the sensitivity of this model?

DR. RIVAS: Yes. For one thing I changed the height of the top. In one case it was 300 mb, in another it was 13 mb for the first model in which the cell doesn't penetrate. The results were essentially the same in these two cases, with the Hadley cell located at the same height, that is at the same pressure level.

NUMERICAL CIRCULATION MODELS

James Pollack and Richard Young, Ames Research Center

The presentation by Pollack and Young is largely contained in the paper by Young and Pollack which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

Modeling the atmosphere in accord with recent spacecraft and ground-based observations, we have carried out accurate, multiple scattering calculations to determine the solar energy deposition profile in the atmosphere of Venus. We find that most of the absorbed energy is deposited in the main cloud layer region, located at altitudes above 35 kilometers, and that the ground receives approximately 3% of the energy absorbed in toto by Venus. Using these results we have computed vertical temperature profiles under conditions of pure radiative equilibrium and radiative-convective equilibrium. Since the latter results satisfactorily match the temperature structure determined from various spacecraft observations, we infer that the greenhouse effect can account for the high surface temperature. Aerosols make an important contribution to the infrared opacity in these calculations. Finally, we discuss preliminary three-dimensional calculations of the general circulation of the atmosphere that incorporate the results of the radiative calculations.

DR. ROSSOW: Was there a particular part of the atmosphere where you were getting the unrealistically large amplitudes of the higher modes in the spherical harmonic expansion?

DR. RICHARD YOUNG: It seemed to be occurring around 60 km and above. In some of our initial calculations during the transitory flow there was an unstable lapse rate in that region, and I corrected that. That helped, but eventually the amplitudes still built up.

DR. ANDY YOUNG: Where do you get the thermal infrared CO_2 opacities? Louise [Gray Young] has been complaining that people who play this game don't use enough CO_2 opacity.

DR. POLLACK: Of course, the basic problem is that most of the laboratory data is for terrestrial-type conditions, while on Venus the atmosphere is almost 100 percent CO_2 and there are very high pressures. For that reason I used the tables published by Stull, Wyatt and Plass [Aeronutronic Report SSD-TDR-62-127 III] which do go to very high pressures.

DR. ANDY YOUNG: Do you know there are serious inaccuracies in those tables?

DR. POLLACK: In which sense?

DR. ANDY YOUNG: There are bands in the wrong places, and things like that. [see L. G. Young, Appl. Opt. 11, 202, 1972] Louise is now trying to do recomputation of opacities in the thermal infrared. She is having trouble putting in enough bands. The problem on Venus is that near the surface bands that are a thousand times too weak to be observed in the laboratory are black. And you must get into very hot bands; Louise is working in quadruply hot bands and stuff like that. One problem that she is having right now is that the computer program is running into too many double precision underflows in computing the transmission.

DR. POLLACK: First, I should point out that Bob Boese and Jake Miller at Ames are in the process of using very long pathlength cells for measurements at high pressures and high temperatures. They are attempting to provide a much better set of data than exists at the present time.

A second point is that, in a certain sense, the bands that come in at high pressures aren't very important, because they come in at the bottom part of the atmosphere. There is some overkill in the sense that the radiative gradients there are already very superadiabatic, and making them more superadiabatic won't make much difference.

DR. RIVAS: I don't understand why, when you have a superadiabatic lapse rate, you correct the mean value of your expansion spectrum.

DR. RICHARD YOUNG: It may be necessary to do more than just correct it in the mean. There may be local regions where the temperature becomes superadiabatic.

DR. RIVAS: What I am doing in that respect is going to grid space, correcting that, and coming back to spectral components.

DR. RICHARD YOUNG: We may have to do that.

DR. STONE: Am I correct in understanding that in the Boussinesq calculation you took parameter values appropriate to the upper atmosphere?

DR. RICHARD YOUNG: Actually they were parameter values more or less appropriate to the lower atmosphere, but in the Boussinesq case it turns out that even if you took different values for the upper atmosphere, it wouldn't make much difference. This can be shown by means of scaling arguments.

DR. STONE: Does this mean that you can't really take the magnitude of your mean zonal velocity as indicative of what you might get in a realistic calculation?

DR. RICHARD YOUNG: In the Boussinesq case, no.

DR. STONE: Were the high surface temperatures you found for radiative equilibrium primarily due to the water vapor absorption or to the CO_2 absorption?

DR. POLLACK: I would say it was about half of one and half of the other.

DR. STONE: What did you assume for the water vapor distribution?

DR. POLLACK: I assumed that it was more or less a sulfuric acid vapor pressure curve throughout the cloud region, which was the reason it was unimportant there. Below the cloud region, from about 35 km to the ground, I chose a uniform value of about 0.3 percent by volume.

DR. SCHUBERT: For your Boussinesq calculations you made a point of noting the phase of the maximum temperature region compared with the subsolar point, and, I believe, found the result 90 degrees. In Mariner 10 pictures, there is an indication of convection. How far is the region of convection separated from the subsolar point?

DR. RICHARD YOUNG: I think about 20 degrees.

DR. BELTON: It could be more than 20 degrees.

DR. RICHARD YOUNG: 90 degrees?

DR. BELTON: I don't see why not.

MERIDIONAL CIRCULATION

Peter Gierasch, Cornell University

The presentation by Dr. Gierasch is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

A meridional cell, with rising motion near the equator and sinking near the poles, transports angular momentum upward in an atmosphere whenever equatorial regions of the atmosphere have an angular momentum surplus relative to polar regions. This process may contribute to the maintenance of the Venus atmospheric super-rotation.

Super-rotation by the process is exhibited in a simple analytical model. The super-rotation ratio in the model is derived to be $exp[HD^2/v_v t_m]$, where H is depth in scale heights, D is the mean scale height, v_v is the vertical eddy diffusivity, and t_m is the meridional overturning time.

For the mechanism to work, some eddy process must maintain an angular momentum surplus in equatorial regions. Vorticity mixing is suggested. It is also demonstrated that if the Richardson number is large in a cyclostrophic atmosphere, the mean thermal structure is given by global radiative equilibrium, and local deviations from equilibrium are balanced by adiabatic cooling or warming associated with vertical motions.

DR. RIVAS: I read the preprint of your paper and I made some experimental calculations with large horizontal viscosity and diffusivity. It turns out that in all equations the main balance is between the horizontal viscosity or diffusivity term and the driving term. In the meridional equation the balance is between the pressure and the diffusion of meridional momentum, with extremely slow velocities of the order of 1 cm/sec.

DR. GIERASCH: When you did your calculation, did you have the Prandtl number equal to one?

DR. RIVAS: Yes.

DR. BELTON: Would you briefly restate Dr. Rivas' point?

DR. GIERASCH: Eugenia has run a calculation with a large horizontal diffusivity and it does not produce the motions I talked about.

But there is a problem because in order to have these strong zonal winds, the atmosphere must have a thermal wind balance, a cyclostrophic balance. Therefore horizontal temperature gradients must exist. So the horizontal diffusivity must permit horizontal temperature gradients, but not permit horizontal angular velocity gradients. The diffusivity must transport momentum effectively horizontally, but not heat. Because if it wipes out the temperature gradients, then there can't be any thermal wind balance.

DR. RIVAS: At most you should have Prandtl numbers of the order of 10^2?

DR. GIERASCH: That depends on other things also. That depends on Richardson's number and the total depth of the atmosphere. But yes, I think

you would want to have at least a stronger diffusivity for heat than for momentum.

DR. POLLACK: In the case of the Earth, you get very large effective horizontal eddy diffusion coefficients because of the baroclinic instabilities which produce large eddies. I don't think you will get baroclinic instability on Venus, because it is rotating so slowly. So, do you have something comparable to baroclinic instability to produce these large horizontal eddy coefficients?

DR. GIERASCH: I had baroclinic instability in mind, actually, until Peter Stone told me it probably won't work. Baroclinic instabilities might happen in the Venus atmosphere. You probably have to think of the local rotation rate of a given shell. But at the moment I don't have any specific instabilities in mind.

SPECTROSCOPIC WIND VELOCITIES

Wesley Traub, Harvard University

The presentation by Traub is largely contained in the paper by Traub and Carleton which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

We have measured the differential Doppler shift between various points on Venus using a high-resolution PEPSIOS interferometer (three Fabry-Perot etalons in series). Using both a CO_2 line and a Fraunhofer line we find a mean zonal wind velocity near the equator of $-83(\pm 10)$ ms^{-1} (retrograde); the velocity appears to vary from about -2 ms^{-1} to -125 ms^{-1}, with a time scale of greater than one week. Meridional velocities are measured to be weak (on the order of 30 ms^{-1} or less). The equatorial zonal velocity appears to be smaller (-73 ms^{-1}) in the "morning" than in the "afternoon" (-111 ms^{-1}) where the times of day are for retrograde rotation. A comparison with reported velocities of the ultraviolet dark markings reveals general agreement in that both find the motion to be retrograde, variable, and accelerated during the day. A new potential source of systematic error in all spectroscopic determinations of the differential Doppler shift of non-uniformly illuminated objects is pointed out.

DR. RICHARD YOUNG: When you observe zero or prograde flows, did you observe on an adjacent day retrograde flows of the order of 100 m/s?

DR. TRAUB: No. We never observed any great changes from one day to the next. On the days when we observed essentially zero velocities, we had observations several days in a row and they were all low velocity. We have never observed any great changes in velocity within a period of one week.

DR. STONE: The variation of velocities that you find might also explain the Venera measurements, and it would be interesting if you could correlate the results. Were any of your measurements made at the same time as the Venera probe measurements?

DR. TRAUB: I don't think so.

DR. O'LEARY: Do you align your interferometer before each observation?

DR. TRAUB: We do not make daily checks on the tilt of each etalon, but we do regularly inspect the parallelness of each pair of plates. There are other systematic effects that may occur, and that would be bad. We have to insert an interference filter in the beam, and it may have a small wedge angle, in addition to a slight waviness in the surface. So there are things that are somewhat beyond our control. But the instrument is stable from day to day. It is our experience that the etalons will remain in essentially perfect alignment for periods of up to at least one year.

DR. JONES: Were there any noticeable trends for when you get the 100 m/s velocity. For example, in the morning or evening terminator, or a function of elongation?

DR. TRAUB: In the measurements we made there was a correlation between the velocity and the elongation of the planet in the sense that we do find a higher wind velocity in the Venus afternoon, and lower in the morning.

DR. BELTON: In spite of the Andy Young effect, a comparison of the shifts obtained for Fraunhofer and CO_2 lines is potentially very important. The Fraunhofer lines, in effect, probe the atmosphere far deeper than the CO_2 lines do, though we don't know how far. If we just take your result at face value, it implies that there is very little vertical shear.

WIND-BLOWN DUST

Carl Sagan, Cornell University

The presentation by Sagan is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The threshold frictional velocity, u_{*_O}, necessary to initiate grain movement on the Venus surface is 1 to 2 $cm\ s^{-1}$. Particles smaller than 30 or 40 μm in effective diameter will be so moved and suspended at the threshold of movement. A small diameter turnup in u_{*_O} is expected if there is surface cohesion. These values of u_{*_O} require velocities > 0.3 m s^{-1} above the surface boundary layer for grain motion on the surface. Theoretical and Venera 8 doppler measurements suggest marginally that dust should not be raised at the Venera 8 landing site (10°S), but should be raised at higher latitudes. Dust carried to tens of kms altitude will be transported laterally over the entire planet and may make important contributions to the solar energy deposition, general circulation, and cloud chromophore problems. However, the Venera 8 photometer measurements and the low albedo of reasonable surface materials imply a clear lower atmosphere at 10°S, despite the fact that dust raised at high latitudes should contribute to the aerosol content at 10°S. Dust raising on Venus may be inhibited by limited vertical turbulent diffusion or by thermal sintering of particles on the planetary surface.

DR. BELTON: We are short of time, so Seymour Hess will give his paper and then we will open the discussion.

WIND-BLOWN DUST

Seymour Hess, Florida State University

The presentation by Hess is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

A calculation is performed of the friction velocity needed to lift dust from the surface of Venus. It is found that the most easily lifted grains are 16 - 17 μm in radius, and a friction velocity of about 1.3 cm s^{-1} will suffice. These are much smaller values than on earth and Mars. Very light free-stream winds will raise dust on Venus. Dust of this size cannot remain suspended in the constant-stress layer because gravitational settling is more efficient there than diffusion. The situation reverses at heights above 1 - 2 km where diffusion can keep fine dust suspended for long periods. A mechanism for production of find dust is suggested.

DR. HESS: There are eight ways to get a slide of that size in, only one of which is correct. Therefore, we always have that difficulty.

DR. SUOMI: In your consideration of the friction velocity, did you take into account the possibility of obstacles like stones, which would change the profile drastically? Although the dust might blow from the top of the stone, it would then settle between the stones and be less available for a long period of time.

DR. SAGAN: I did scale with various roughness parameters. The results are not very sensitive to what is, on the Earth, a reasonable range of roughness.

DR. POLLACK: What roughness heights did you use, Carl?

DR. SAGAN: It is the log of the roughness height that enters in the logarithmic velocity profile, so the result is not extremely sensitive to it. I used a range of something like from 2 times the particle size to 20 times the particle size.

DR. POLLACK: If you used a larger roughness height than about 30 times the particle diameter, then the threshold friction velocity is given by a different formula.

DR. HESS: I would argue with that. If you use a mean wind profile, you are dealing with an average over quite a distance. It seems to me it isn't so much the particle size that is important in determining the roughness length, it is the obstacles that may be around. I see Suomi nodding his head, and he is a very wise man, so that makes me feel much better.

DR. POLLACK: That is right. But, in laboratory experiments that Iverson, Greeley and I have done, the particle size does in fact change the roughness height.

DR. HESS: Yes, if you lay out a nice smooth sand surface, then I agree with you, the particle size determines the roughness parameter. But nature doesn't usually do things like that.

DR. POLLACK: The point is that if you do choose a large roughness height, then in general you need larger friction velocities to cause grain

motion. Also, even with neutral stability, logarithmic profiles are applic-
able to only the very lowest part of the boundary layer, and you have to use
something else in most of the boundary layer.

Carl pointed out, and Iverson, Greeley and I also find from our wind
tunnel experiments, that cohesion is chiefly responsible for the increase in
the threshold velocity for small particles. So a very important thing and a
very difficult thing to get at is how much this cohesion effect changes in
different environments.

DR. SAGAN: It is not intuitively obvious what the cohesion is between
30 micron grains on the surface of Venus.

DR. HESS: Just to show you how parallel our thinking is: I discuss in
my paper the possibility that there might be a sink of small particles, while
Carl has been discussing recementing of particles at high temperatures.

DR. SAGAN: All these ideas are obvious.

DR. O'LEARY: Carl can you elaborate on the present radar results, such
as the Arecibo maps? I think that the craters on Venus are more subdued
than those on the Moon and Mercury.

DR. SAGAN: The main thing is that on the Moon and Mercury, and to a
lesser extent, on Mars, the crater diameter to depth ratio is about 10 to 1.
In the case of Venus, according to the Goldstone results, it is more like
100 to 1. So they are very shallow craters. The question is, what has made
them shallow? Filling by wind-blown dust is an obvious possibility. In the
case of Mars, where there is some filling, that seems to be a reasonable
explanation.

But in a case where the dust can't be moved around, there has to be
some other mechanism. The question is: What is it?

DR. ANDY YOUNG: Don't the radar results also tell you that there is
roughness on Venus on the scale of 10's of centimeters? And that says there
are some places where the roughness is large compared to the particles?

DR. SAGAN: Surely. I did want to say something about Seymour's idea
that the big particles raise up little particles. Those particles are
moving very slowly, 1 or 2 cm/sec. They move with the entrainment speed of
the wind.

DR. HESS: That is right. But in saltation that depends on how high
they rise.

DR. SAGAN: If you do the calculations, you will find that it is a few
centimeters a second. And it is not enough to displace the dust.

DR. HESS: I considered the case in which the wind is fast enough that
the friction velocity can raise millimeter size particles to a speed of the
order of a meter per second.

DR. SAGAN: Meters per second?

DR. HESS: A meter per second at the height of the saltation.

DR. SAGAN: You would have to get it out of the boundary layer.

ATMOSPHERIC STRUCTURE

Michael Belton, Kitt Peak National Observatory

When I was asked to give a review of the structure of the lower atmosphere on Venus I was rather anxious to do it, because I was involved in the interpretation of the Mariner 10 TV pictures. These contain considerable evidence for global wave propagation in the atmosphere and it is clear that before such phenomena can be discussed, at least as far as perturbation theory is concerned, we must have a good idea of the basic state of the Venus atmosphere. So I was very interested in trying to make up my own basic state in the context of this review.

Then I discovered that in the last three years there have actually been, for this part of the subject, more review papers written than research papers. I figured one more would be of little credit, so my review will be very short -- about five minutes. Then I will go on to take what I consider a rather extreme position: I would like to look at the current foundation stones of our understanding of the structure of the Venus atmosphere, pick them up, so to speak, and try to see what horrors lurk beneath. In particular I will be looking out for possible bandwagon effects.

This latter part of the talk will be divided into four parts; one on the CO_2 abundance; the second concerns mixing ratios of minor constituents; a third called "winds or illusions"; and a fourth: "Is Venus a greenhouse?"

Let's go to my review, which is very short. Figure 1 defines the Venus lower atmosphere, as portrayed by Michael Marov in his 1972 review article [*Icarus* 16, 415]. That paper plus his subsequent paper with co-authors on the Venera 8 measurements [*Icarus* 20, 407, 1973] are, in my opinion, probably the best review available, as of the beginning of this conference, for the lower atmospheric structure.

The one thing I would point
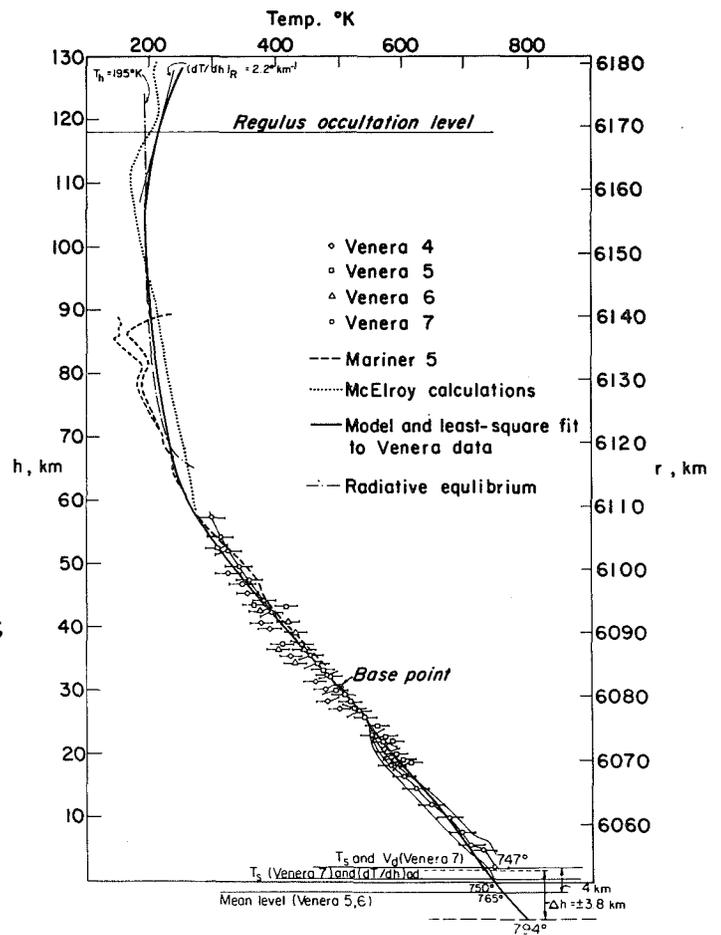
Fig. 1. Temperature profile of Venus atmosphere from a range of observations and calculations. Individual Venera temperature measurements are shown. [Marov, 1972: Icarus, 16, 415]
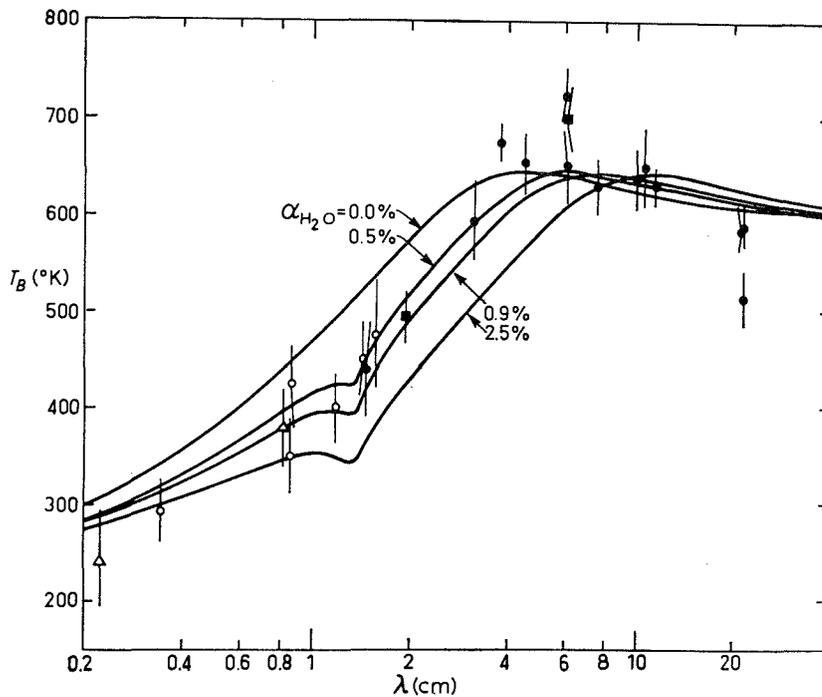
out in Figure 1, for later reference, is the modest bulge in the Mariner 5 data compared to the various Venera P-T points near 50 km. This feature, as you will see later, is also present in the Mariner 10 occultation result. It amounts to something like 15°K.

This concludes my review on structure and I would now like to consider my first major topic: The CO_2 abundance. At the start of this conference Andy Young went to a corner of the blackboard and wrote down the CO_2 mixing ratio as greater than 90 percent. I don't think anybody in this room challenged that, so I classify it as a potential bandwagon. The question is: how secure are we in our knowledge that CO_2 is actually in the Venus atmosphere in such a high proportion? The best measurements are from Venera 5, which yielded 97 ± 4 percent. Veneras 7 and 8 didn't have any chemistry sets on them. [Although Veneras 7 and 8 did not measure CO_2, Venera 8 did have an ammonia chemistry set aboard.]

But first, let's go into some of the reasons why the CO_2 mixing ratio is very important in this subject. One of the main reasons concerns the interpretation of the microwave spectrum, which is the basic observation that limits our estimates of the water content of the atmosphere. Figure 2, from Pollack and Morrison [*Icarus, 12, 376, 1970*] shows a model spectrum for essentially a pure CO_2 atmosphere. You can see there is room to put some additional opacity in the atmosphere, but not very much. Thus water estimates are limited to mixing ratios of the order of a small fraction of one percent. However, if there is less CO_2 present, it being replaced by a gas that is not so active in the microwave region, then there can be room for more water; perhaps amounts as large as that indicated by the Venera 4 and Venera 5 in situ measurements.

Other reasons why precise knowledge of the CO_2 abundance is important are: its significance with regard to the evolution of the atmosphere; for precise interpretation of spectral measurements of Venus; and finally, for
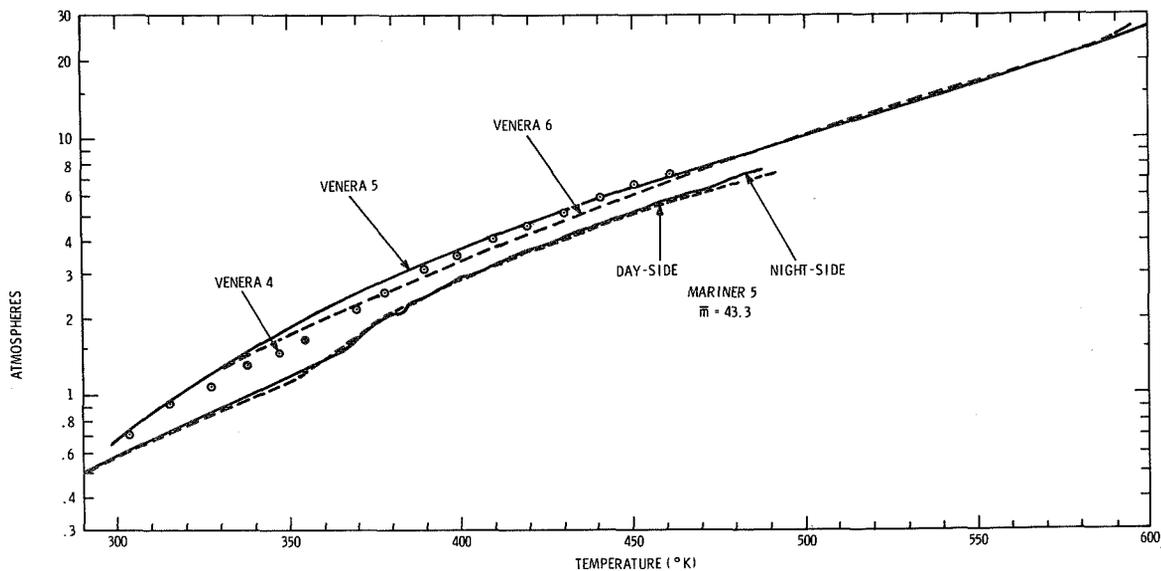
Fig. 3. A comparison of early Venera in situ measurements of pressure and temperature in the Venus atmosphere with the results of the radio occultation experiment on Mariner 5. The latter assumes 97% CO_2 - 3% N_2 mixture. [Ainsworth and Herman, An Analysis of the Venus Measurement, GSFC, X625-72-187, 34 pp., 1972]

meaningful discussions of the basic radiative state of the atmosphere, and its dynamic stability.

Now, what are the ways of obtaining the CO_2 abundance? The old way was spectroscopy, but if you look at the latest review of Venus spectroscopy by Louise Young [*Icarus*, 17, 632, 1972] you will find no mention of a spectroscopic determination of the CO_2 mixing ratios, only an assumption of its value. This is because spectroscopists have discovered, much to their embarrassment, that they couldn't determine the abundance of the only thing they could easily observe.

The only attempt that I recall to solve this problem was by Goody, Hunten and I a few years ago - perhaps the Young's have some similar discussions - anyway, we published [Belton, Hunten and Goody, in *The Atmospheres of Venus and Mars* 1968, p. 69] a result that the CO_2 is greater than about 10 percent. We couldn't really say much more, the basic problem being that the CO_2 reflection spectrum only contains definite information on the product of pressure times abundance.

A second way of getting the CO_2 abundance is through comparison of radio occultation measurements and the P-T points as measured in situ. Put together this information allows an estimate of the mean molecular weight of the atmospheric gases. When this was first done with Mariner 5 and Venera 4 data, it was my impression that a mixing ratio of 95 percent CO_2 gave a very good fit, and, providing the other constituents were not too light, their nature did not matter too much. So it seemed to me that the occultation data confirmed the high CO_2 amount measured directly by the chemistry experiments on Venera 4. However, Figure 3, which is from a paper by Ainsworth and Herman [An Analysis of the Venus Measurements, GSFC, X-625-72-187, 34 pp., 1972] shows the lapse rate computed from Mariner 5 results assuming a 97 percent mixing ratio of CO_2 with N_2 and also the lapse rate from various early Venera profiles. The latter were taken at slightly different latitudes on the planet, but I don't think that that factor is too significant.
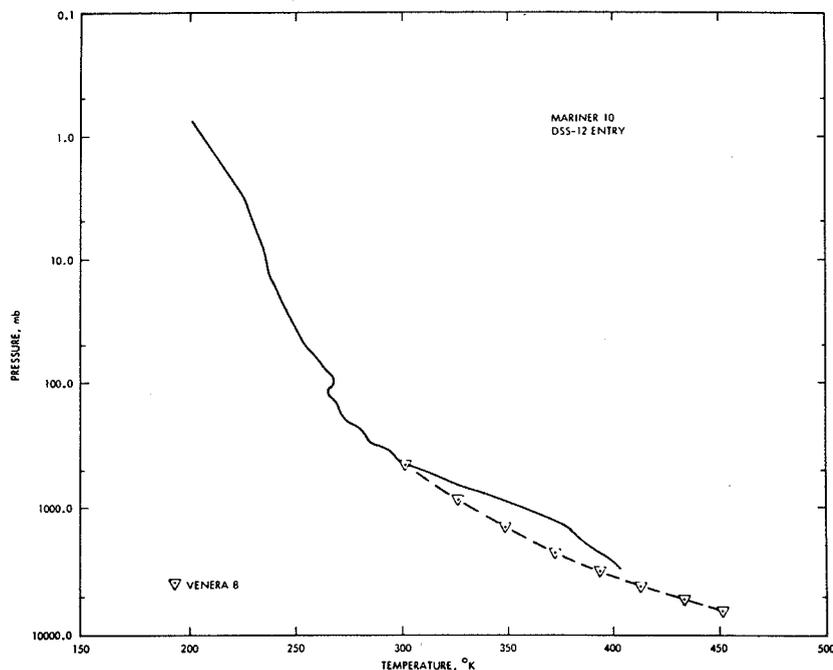
Fig. 4. Comparison of the Mariner 10 radio occultation results on atmospheric structure with the results from Venera 8. [*Howard, et al., 1974: Science, 183, 1300*]

You can see there is a clear discrepancy in the two results when you assume 97 percent CO_2, as I pointed out in the first slide. The discrepancy corresponds to something like a 20°K displacement in temperature. The simpler way to reduce the discrepancy is to assume a smaller mean molecular weight -- that is, to first order, bring the mean molecular weight down by about 20 percent.

For CO_2 and nitrogen, that would require the CO_2 mixing ratio to be a bit less than 70 percent. For argon and CO_2, the CO_2 fraction would go down to only 25 percent!

Figure 4 shows the Mariner 10 results of Howard et al. [*Science, 183, 1300, 1974*] compared with the Venera 8 profile. Again there is the same kind of problem. I would have preferred a comparison with Venera 7 since it and the Mariner 10 occultation path are much closer together in latitude. Nonetheless the same problem exists.

Now I may be stretching the data a little too far here, so that is as far as I want to go with the occultation business and move on to the main reason why I am a bit worried about a CO_2 abundance as large as 95 percent. In case after case, and Figure 5 is just one illustration, the run of pressure-temperature which was measured shows a superadiabatic gradient in certain parts of the atmosphere of the order of 1°K/km. Now Carl Sagan showed many years ago, as have Gierasch and Goody in their paper about dynamical support of the Venus clouds [*J. Atmos. Sci., 27, 224, 1970*], that it would be very hard on the average to support even 10^{-6}°K/km superadiabatic lapse rate in the Venus atmosphere. The superadiabatic region in the Venera 8 data, shown in Figure 5, is recognized by Marov, et al., but they minimize the problem. They suggest that the problem may be that the altimeter was misinterpreted and that the probe was passing over a 7 degree slope on the surface. That, of course, may be possible.
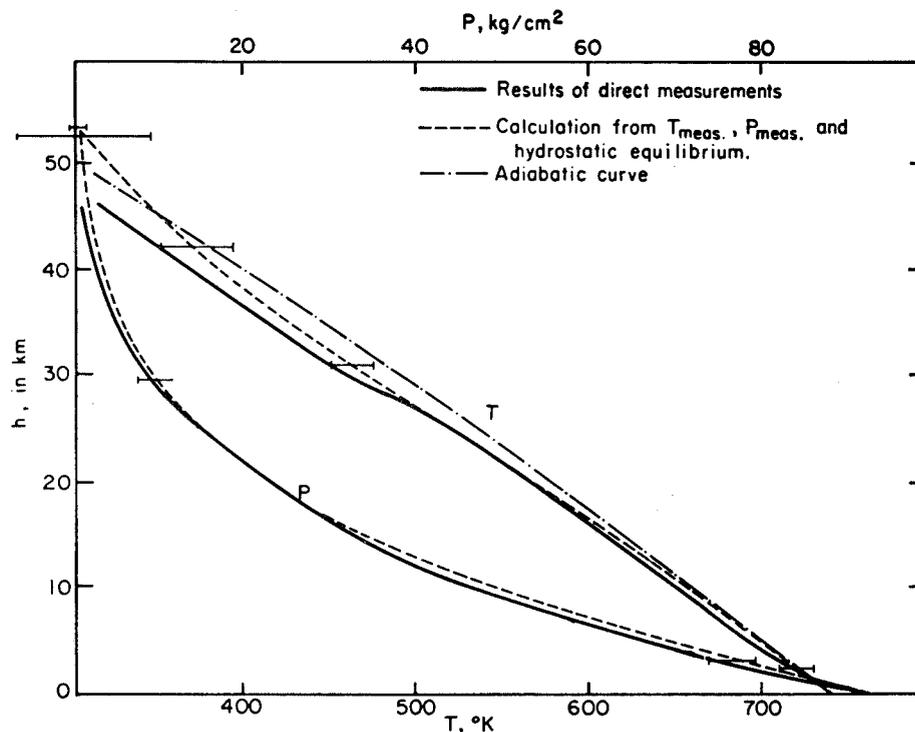
Fig. 5. Venera 8 measurements of pressure and temperature in the Venus atmosphere. The lapse rate near 30 km is superadiabatic. [Marov, et al., 1973: Icarus, 20, 407]

There are similar things that have kind of been swept under the rug, in my opinion, e.g., the occultation data from Mariner 5 between 35 and 40 km, to be sure, deep down where the data is uncertain, implies a slightly higher lapse rate than that for dry CO_2. Also, as Anderson [Nature, 217, 627, 1968] pointed out the Venera 4 data show a superadiabatic gradient at an altitude of about 28 km. Finally, Marov in his 1972 review notes a superadiabatic regime between 3 and 16 km in the Venera 7 data. Well, we may be stretching the numbers too far. After all, the curves in Figure 5 are only a polynomial fit to some data points, for which you saw the error bars on Figure 1. But there seems to be mounting evidence that a dry, nearly total CO_2 atmosphere would imply superadiabatic lapse rates, and that doesn't seem very reasonable to me. The way to get rid of this problem is to assume that some other gas is present in the atmosphere that will lower its effective specific heat. With nitrogen as the other gas you would have to lower the CO_2 mixing ratio to about 70 percent.

Now, let's look at how the CO_2 abundance measurements were made. With this we are getting to one of the exciting points of this presentation. As far as I am concerned I have always had a Michael Farraday complex. He was the great British laboratory technician who became a recognized first class scientist, partly on his ability to perform experiments in front of people. He was able to perform them flawlessly; they always worked, and they didn't explode. I always admired him, particularly since when I was in high school chemistry, my papers always came back with either "bang" on them, or "rats" across my experimental setups.

These chemical measurements were made only on Veneras 4, 5, and 6, and they were done with certain instruments called gas analyzers. Each spacecraft carried two sets of these which were operated at different levels in

Venera 4			Venera 5			Venera 6		
P(atm)	T(°C)	CO_2(%)	P(atm)	T(°C)	CO_2(%)	P(atm)	T(°C)	CO_2(%)
0.7	~25	90 ±10	0.6	25	97 ±4	2.0	85	>56
2.0	85	-	5.0	150	>60	10.0	220	>30

Table 1.   Venera Measurements of the CO_2 Mixing Ratio

the atmosphere. Basically, a single valve was opened allowing the Venus atmosphere to be sucked in and fill a bank of eight gas analyzers. When each bank of gas analyzers was filled it was hermetically sealed and allowed to stabilize. Then various chemical tests were performed in the different cells. The CO_2 cell consisted of two chambers separated by a pressure sensitive membrane. Stress on this membrane was measured electronically. In one chamber was a pellet of potassium hydroxide that would absorb CO_2. The efficiency of this process is affected by the amount of moisture, so the chamber also included a dessicant, some lithium salt, to get rid of the moisture. In the second chamber there was a pellet of calcium chloride whose function again was as a dessicant. Thus the pressure differential buildup in the cell would only be due to the absorption of CO_2 on the KOH. The results of the various measurements are shown in Table 1.

The Venera 4 measurements of CO_2 at the 0.7 atmosphere pressure level yielded 90 ±10 percent. A measurement was also tried at the 2 atmosphere level, but no result was reported.

The Venera 5 measurement, high in the atmosphere, gave the number that everybody quotes, 97 ±4 percent. A measurement was also made at the five atmosphere level, but because of saturation of the KOH it only gave a lower limit. Venera 6 similarly gave only lower limits so our knowledge of the CO_2 mixing ratio, the number that we use, is basically a single measurement from Venera 5, aided and abetted by a measurement of lesser precision on Venera 4 at one point in the atmosphere. And these measurements were made right in the middle of Andy Young's sulfuric acid cloud. And that is the reason for my chemistry experiment, which I shall now perform:

What happens when you get sulfuric acid on the chemicals in the CO_2 gas analyzer? I am going to show you, then I will add to the discussion to show that I am again stretching a point here. This is the sulfuric acid and here is some calcium chloride. Let's see what happens when we put sulfuric acid on calcium chloride.

Fizzle.

The gas that came off was HCl, and the material that has sedimented out, is calcium sulfate. This is a rather exothermic reaction; it gives out something like 2 kilocalories per mole. The second experiment is even more interesting. I don't think this has ever been done in the annals of planetary astronomy. Let's see what happens when we put potassium hydroxide in the sulfuric acid.

Fizzle. Fume. Fizzle.

Isn't that fantastic? What came off was water, basically steam, and

almost 162 kilocalories per mole, a tremendous amount of heat. Now the
question is: could the sulfuric acid clouds possibly cause this to happen
in the Venera gas analyzers? For the sake of comparison let's consider
cloud droplets such as exist in fair weather cumulus clouds, droplets of
about 10 μm in diameter with number densities between 100 and 1000 per cubic
centimeter. That is already somewhat more than what Andy was talking about
at higher altitudes.

    DR. ANDY YOUNG: A lot more.

    With such cloud particles you would produce very little heat and of
the order of 10^{16} HCl molecules. At the 0.7 atmosphere level, where the
measurement was made, there were about 10^{19} molecules per cm^3, so the num-
ber of HCl's produced is effectively insignificant. However, 0.7 atmos-
pheres is a long way down from the 50 mb level, so the cloud properties
could be substantially different from anything that was spoken of yesterday.
We could imagine a half millimeter diameter droplet; or the Venera probe
may have collected sulfuric acid on it as it fell, and then sucked in the
sulfuric acid when the valve opened. For definiteness let's consider a
single half millimeter drop getting in. It would produce 8×10^{18} HCl
molecules, comparable to the amount of CO_2, and so could substantially
increase the apparent amount of CO_2. It would also produce heat, enough
to raise the temperature about 12° K. Maybe that is a problem, maybe it
is not.

    Of course this is an extreme example, but it is an extreme atmosphere.
As of now I still believe there is 90 to 100 percent CO_2. But I think we
had better be cautious about this particular subject.

    Now let's turn to mixing ratios of minor constituents. Andy Young in
his talk yesterday, together with others, stated what the mixing ratios of
condensibles probably were in the Venus atmosphere, at least so far as we
know from spectroscopic data. Now one of the more interesting of these
ratios is the value for HCl. One of the main reasons is because there are
some nice models coming out now that seem to remove previous difficulties
of explaining the observed rapid recombination of CO and O back into CO_2.
These are the papers by McElroy, Sze and Yung; one that is published [*J.*
Atmos. Sci., 30, 1437, 1973] and one that is about to come out. Basically
they use HCl at about the 10 mb level to provide hydrogeneous materials
that can catalyze the recombination of CO and O back together into CO_2.

    However, we will first of all consider the case of water. As I men-
tioned before, the Veneras may have had problems with sulfuric acid in
their water vapor measurements, but I have not analyzed that. They claim
mixing ratios of the order of several tenths of one percent, enough to make
a substantial water cloud. On the other hand Earth-based spectroscopic
observations yield mixing ratios that are anything from 0 to 10^{-4}. There
is an observed variation by a factor of at least two orders of magnitude in
the amount of this sometimes observable material. This is really quite
startling, I think, when you consider Figure 6 which is from some unpub-
lished work of mine. It represents a measure of the short-term variability
in the carbon dioxide absorption spectrum on Venus and is based on the
published work up to about 1971 on near-infrared bands of CO_2 by Schorn,
the Youngs, Barker and various other authors. They have published for an
observed band a quantity which they call W_0. W_0 is an extrapolated equiva-
lent width for the R(0) line, extrapolated from the measured equivalent
widths of the other observed lines in the band. So it represents a good
measure of the observed band intensity on Venus for that particular
measurement.

In the three or four years of publications that I looked at, there were 162 such values of W_0. I put the results from the six different bands together, normalized the mean to unity and Figure 6 is the result. It is slightly skewed, but the important point is that the dispersion, the basic, short-term variability, is of the order of 15, or perhaps 20 percent. This is in contradistinction to observations of water which varies as much as two orders of magnitude. I only have one explanation of this: water is not homogeneously mixed in the stratosphere of Venus. In fact the distribution of water must be very inhomogeneous to give an order of magnitude variability when there is only 15 percent variability in the major gas.
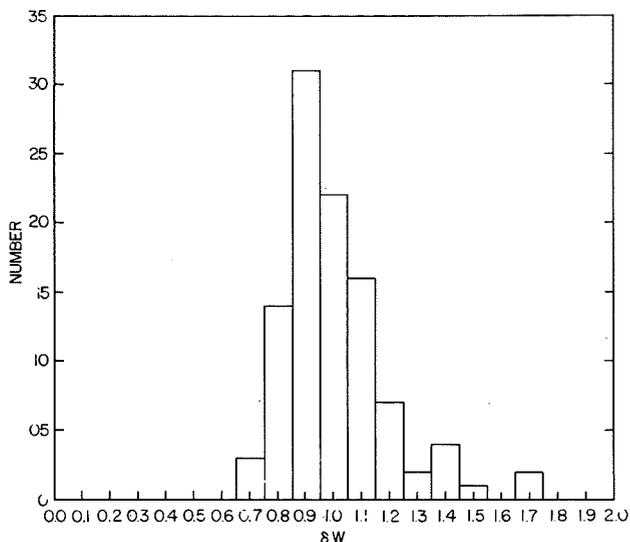
Fig. 6. Normalized distribution of observed band absorption in CO_2 on Venus; 162 observations of six near IR bands are the basis for this figure. The data was collected by various authors (see text) over several apparitions of the planet.

I assume that this may be due to variability in the transmission of the clouds: occasionally there must be holes in the clouds, so we can see down where there is a lot more water. Kuiper's airplane measurements gave a mixing ratio average over the planet's disc of something like 10^{-6}, so presumably the mixing ratio must be much larger below the visible clouds, surely as high as 10^{-4} and perhaps as high as 10^{-3} as suggested by the Venera experiments.

Now let's consider HCl. HCl is particularly interesting to the spectroscopist, because it apparently provides direct spectroscopic data about the degree to which it is unmixed. It therefore provides a more quantitative argument than the variability argument just used for water vapor. This was pointed out by Louise Young [*Icarus* 17, 632, 1972] in her review and, as far as I know, she is the only person to recognize this application.

Figure 7 shows the data. You should remember that the mixing ratio that is generally used for HCl, 5×10^{-7}, is the result of one report [*Astrophys. J.* 147, 1230, 1967] in which four or five spectra were averaged together. So far as I know, only one other person has measured HCl to date, Uwe Fink at Lunar and Planetary Lab. He tells me that HCl is probably variable, but a number is not yet available.

Now, let's look at what the Connes group did to compute a mixing ratio. Their intention was to make an estimate of the mixing ratio that was independent of the radiative transfer problem. So they took a CO_2 line near the HCl lines with essentially the same equivalent width. Thus the lines should be basically the same as far as photons are concerned. Then by comparing the equivalent widths and knowing the relative intrinsic strengths of the lines, a mixing ratio is obtained that should be independent of the radiative transfer problem to the first order. Now why is this argument demonstrably wrong for Venus? By looking at all the HCl lines in Figure 7 you can see that they fall on a curve of growth which has a different slope than that for CO_2. So if the lines have essentially the same pressure half-width, which is roughly the case, that means that the effective pressure is
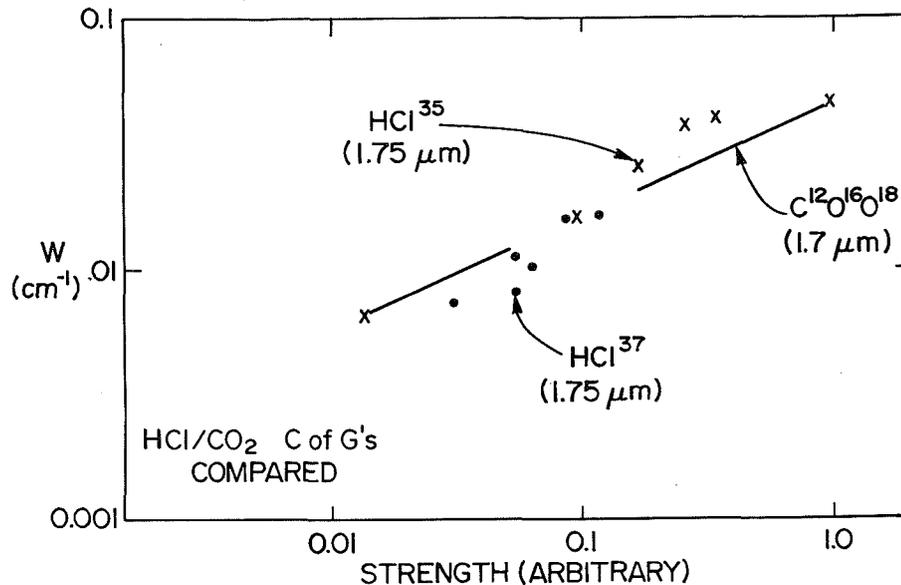
Fig. 7. A comparison of the observed curves of growth of HCl and CO_2 lines of similar intrinsic strength. The slope of the HCl points is steeper, indicating less saturation in the lines.

higher for the HCl than it is for the CO_2. The pressure is higher because the HCl lines must be broader, i.e., less saturated, to be closer to the linear part of the curve of growth. What are the effective pressures? For CO_2, Figure 8 shows some data I worked up some while ago and is based on the same observations used in Figure 6. As indicated in the figure I get an effective pressure of 15 mb, plus or minus 5 or 10 mb. Other spectroscopists prefer 50 mb. In the past even higher numbers, say 200 mb, were preferred.

Now, what is the range of effective pressures that have been derived for HCl? The smallest that I know of is about 200 mb, and the number I prefer from my analysis [*J. Atmos. Sci.*, 25, 596, 1968] is almost 300 mb. Thus we have direct evidence that there could be a tremendous separation in the levels of formation of HCl and CO_2 lines and that HCl is far from being homogeneously mixed. The HCl mixing ratio, down in the clouds, could be much greater, perhaps two orders of magnitude greater, than the number we have been using to date. Whether the mixing ratio is higher or lower than 5×10^{-7} at the 10 mb level, where I understand it is crucially needed for the CO/O recombination problem, I don't know. But it seems to me that this is a problem that has been pointed out in the literature, and the people concerned with the aeronomy should recognize Mrs. Young's valuable contribution.

Now let's go to the subject: winds or illusions? I planned to give a paper on wave propagation in this morning's session, but I discovered that I couldn't solve the problem. However, I do have some ideas that I want to tell you about and I would like to discuss them as an extreme position. There is plenty of evidence of waves in the Mariner pictures although I think the balance of the evidence is clearly in favor of most of the apparent motion of UV markings being a true mass motion, but let's, for the moment, take the posture that they are not.

The first evidence for waves can be seen in Figure 9 which shows global views of the planet on 1-day centers over a period of 8 days. As we were
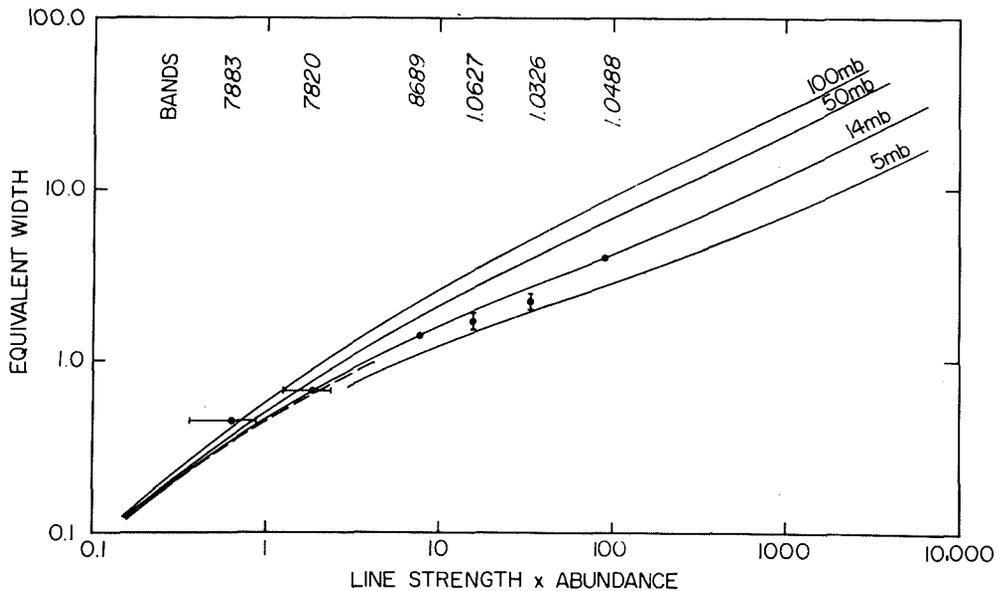
Fig. 8. Scattering model curves of growth based on a Voigt line profile compared to observations of CO_2 absorption on Venus. The individual curves are marked with the assumed effective pressure.

watching the pictures coming in in real time with rather poor definition on regular TV monitors I noticed something that puzzled me. Nothing seemed to change. The same general pattern was always present. As the data came in, day after day, and with little hard copy to compare things on short time scales I was under the impression that maybe there wasn't any four-day rotation. We know now that this of course is not true and that apparent motions of ∿100 m/s exist. But the fact remains that the basic overall symmetry in the markings seems to maintain itself. I say that that is a wave, and in particular it is a manifestation of the diurnal thermal tide propagating at ∿4 m/s through the Venus stratosphere.

A second piece of evidence for waves in the picture is what are now termed circumequatorial belts. We point these out (Figure 10) to everybody hoping that they will say, "Gee, that's obvious, I know what they are!" But nobody has done that yet.

DR. JONES: Contrails.

I have made some preliminary measurements on these features. They appear to be truly latitudinal in the following sense: they are within two degrees of a line of latitude as defined by the IAU convention, and they also appear to be lines of latitude in a coordinate system which is based on the symmetry of the bow-like features that stretch from the subsolar region.

I think the circumequatorial belts are waves because they propagate rapidly toward the equator from high latitudes and it is hard to imagine a source of such large meridional mass motions. Still additional evidence for waves can be seen, for example, the tremendous amount of coordination in the bow-like waves on a global scale. These features seem to progress around the planet, and very possibly be fundamental to the origin of the spiral streaks (cf., Figure 10) as we call them. Some things in these pictures may not be waves, but I think the bow-like "waves" probably are.
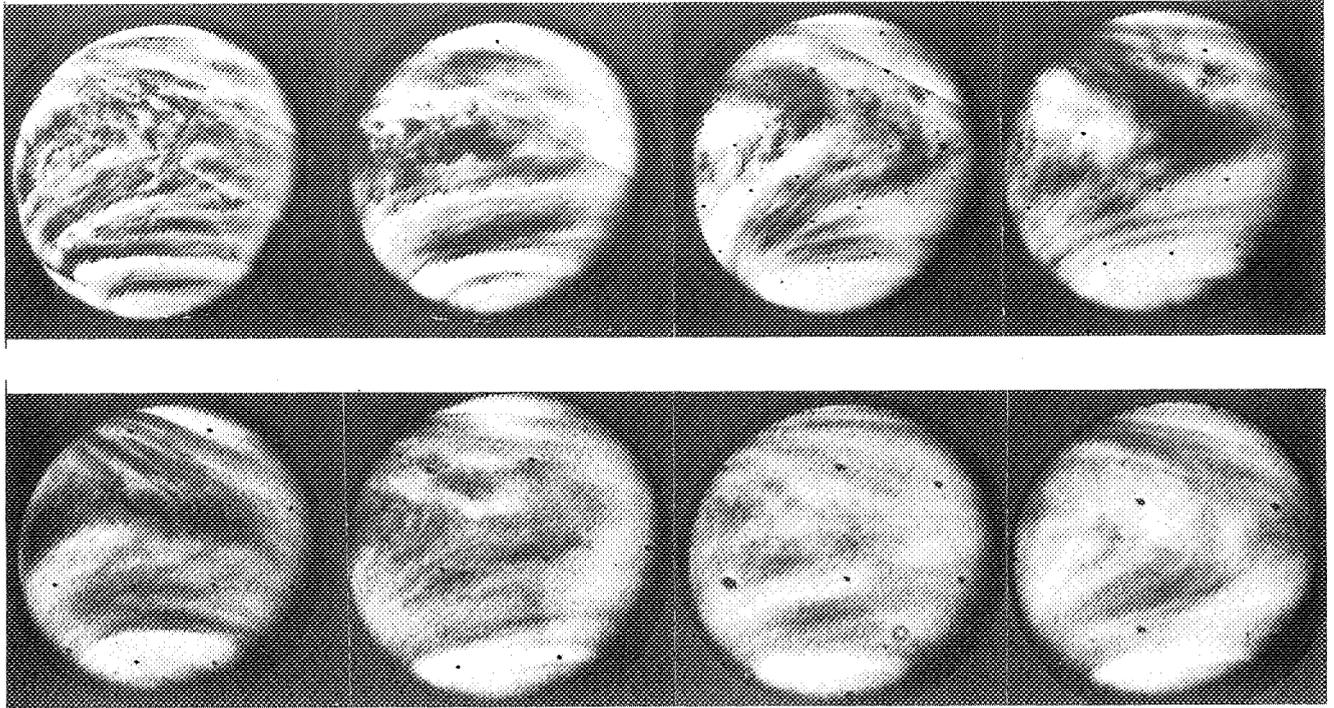
Fig. 9. *Montage of global views of Venus on 1 day centers. Time progresses from top left to top right for four pictures, then bottom left to bottom right.* [*Murray et al., 1974: Science, 183, 1307*]

Then the question is, what kind of waves are they? So I started looking for a wave that was nondispersive, and a mechanism that would explain the shape of the observed waves. An obvious candidate is the gravity wave of extremely long wavelength (relative to the scale height of the atmosphere). They are nondispersive and they can propagate rapidly with a velocity $\sim(gH)^{\frac{1}{2}}$.

They propagate at that velocity, as deduced by Lamb in 1910, in a compressable atmosphere only if it is neutrally stable. If it is not neutrally stable then there is a whole spectrum of propagation velocities for these waves. Taylor, in England, worked out the general problem for a constant lapse rate in 1936 and showed how to calculate the spectrum of propagation velocities. I took the conditions in the Venus stratosphere, which yield a lapse rate of $\sim 5°K/km$, and put that into the eigenvalue problem, and found the spectrum of propagation velocities. The first one is 270 m/s and the succeeding ones 117 m/s, 92 m/s, 75 m/s, 56 m/s, etc. These numbers kind of look familiar. For a neutrally stable atmosphere the allowed propagation velocity is ~ 188 m/sec, almost twice that which is observed.

On the other hand - I told you I was going to take an extreme position - if the mean winds are 100 m/s, then we would have to look for a propagation velocity of gravity waves way down in the list of eigenvalues. Or if the winds are something in between, say on the order of 30 m/s, then these waves must be propagating according to one of the intermediate eigenvalues.

Now the perturbation theory doesn't tell us which one of these modes the planet prefers to propagate its waves at. But we can consider one
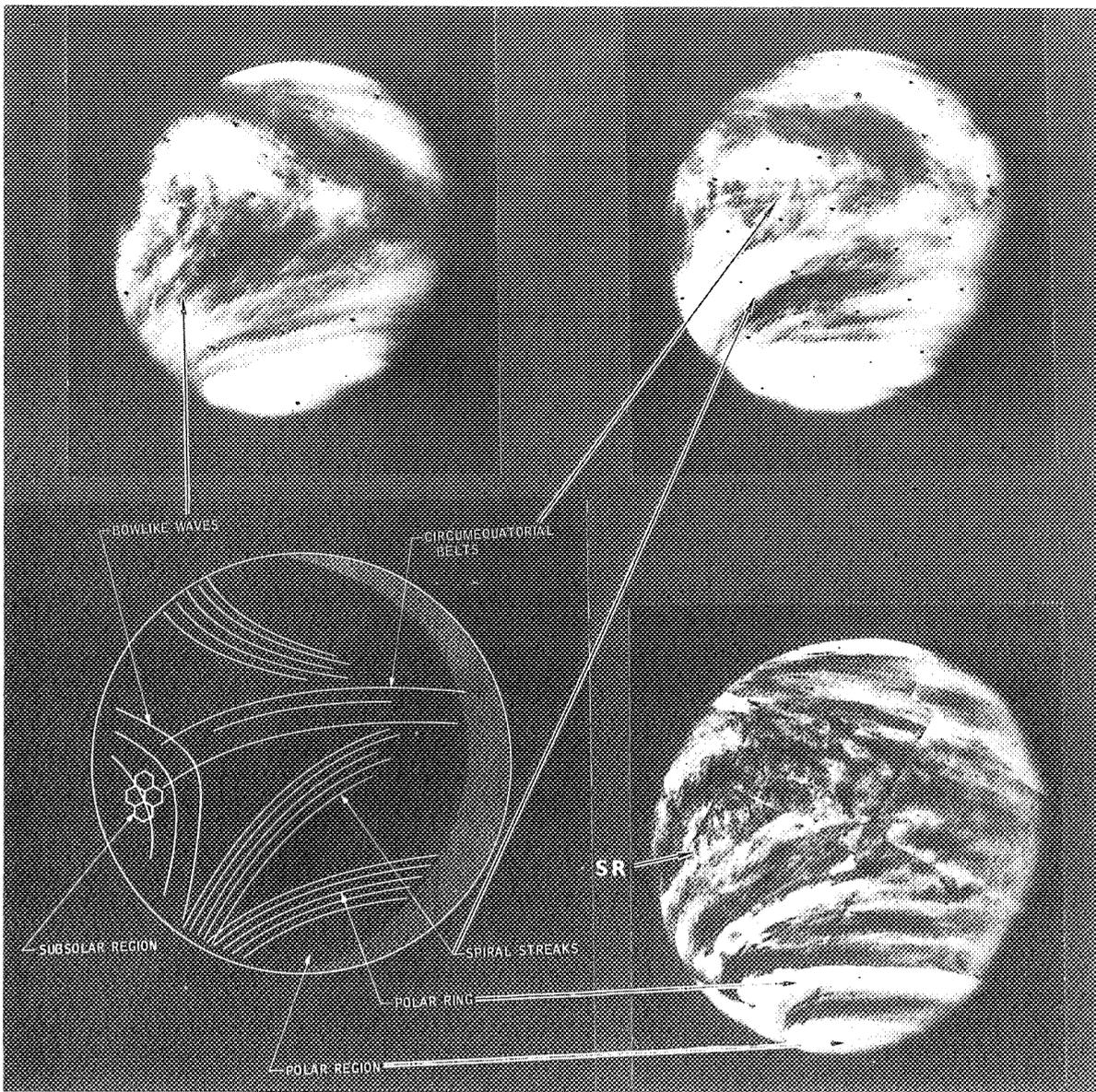
Fig. 10.   Circumequatorial belts and other features in the Venus pictures
[*Murray et al., 1974:   Science, 183, 1307*]

definite wave, a circumequatorial belt. That, I suspect, must be a wave.
There is just not going to be a meridional wind blowing at 15 to 30 m/s
across the equator on Venus. So I contend that the rough measures I made on
the circumequatorial belts may well be the preferred velocity of propagation
in the stratosphere of Venus. Granted they are going in the direction per-
pendicular to the equator, but it is possible.

There is one other thought I would like to try out on you, and that
concerns why the bow-like waves have their characteristic shape. The thought
that I have is a very simple one: that the shape is merely a reflection of
zonal motion on a sphere. What we presumably should look for is a source of
excitation for these waves that has the same morphology as the waves that it

is producing. Is there some kind of field on Venus that might have a bow-like shape? I say, yes, there probably is. Let's assume that there are zonal velocities, and let's neglect what Vern Suomi was saying about some slight acceleration towards higher latitudes. Let's just imagine a constant zonal velocity on a sphere. Now imagine a line of balloons, released along a N-S meridian. Because of the motion on a sphere the balloon at the equator will advance the least in longitude, and a bow-like shape results.

So, consider the stratospheric temperature field in the presence of such a constant velocity mass motion. The line of maximum temperatures should have that same bow-like shape. This is because the maximum temperature at some latitude is delayed behind the longitude of the subsolar point. How far it is delayed is determined by the thermal radiative time and rate of solar heating. Simple models show that the locus of local temperature maxima will have the required bow-like shape.

Once excited, how can these waves be maintained? Perhaps a resonance is involved. Unfortunately it would have to be a very high overtone, but it is easy to see that some kind of resonance is possible. It would take an overtone of about 25. Perhaps this is not inconceivable.

That will have to be the end of my talk. Unfortunately I don't have time to get to the greenhouse problems as I intended.

DR. ROSSOW: I want to put a couple of limits on the CO_2 and water abundances. Calculations show that it takes very little liquid water to make the clouds optically thick in the microwave region. Any acid at all is enough to make a percent of water vapor condense into a liquid, and the microwave spectrum is not consistent with that amount of water. So water is limited to less than a percent, just on that basis. Also, since massive clouds are ruled out, the upper limit on the mass mixing ratio, cloud to atmosphere is something like 10^{-3} or perhaps 10^{-2}. So your suggestion that part of the CO_2 measured by Venera 5 might have been gas produced by a chemical reaction with the cloud drop, is limited to that small amount.

Another way to reconcile the Venera measurements of temperature with the occultation measurements of temperature is to put in some extra opacity. What is really being measured in the occultation is the density of atmosphere. By adding an absorber a different temperature is obtained for that part of the atmosphere.

DR. BELTON: The scale height is obtained from the refractivity, not from absorption.

DR. STONE: With regard to the contrails, the circumequatorial belts, there is one thing that immediately comes to my mind. When you have a very stable atmosphere, like the upper atmosphere appears to be, the kind of instability you get is Hart's "finger" instability which would give rolls parallel to the equator, similar in appearance to the circumequatorial belts.

In that connection a question which comes to mind is: what is the scale of the contrails? In Hart's analysis, [*J. Atmos. Sci.*, 29, 687, 1972] the physical scale does depend on things like the stratification and the boundary conditions. If you really stretch things you might get a scale as large as 50 km.

DR. BELTON: The scale of these features is of the order of 50 km, provided that they are resolved in the picture.

DR. STONE: It would be easier to explain if they were smaller scale.

DR. SAGAN: Can you briefly tell us what you would have said on the greenhouse effect if you had had the time?

DR. BELTON: In my opinion, there are only two reasonably definitive studies of the greenhouse effect in the literature. Of those two studies, one is extremely well known, and that is the one by Jim Pollack in two papers [*Icarus*, 10, 301 and 314, 1969]. The other one, which I suspect very few people here know about, is by a gentleman called Eric Roeckner and has the title "Temperaturberechnung der Venusatmosphäre bis 80 km Höhe aufgrund solarer und Thermischer Strahlungsströme sowie konvektiver und turbulenter Wärmetransporte" [Mitte. a.d. Max Plank-Institut fur Aeronomie. Nr. 46, 1972].

Roeckner, in a very detailed calculation, comes up with a stable radiative atmosphere for Venus. The lapse rate is subadiabatic for a CO_2 atmosphere. Jim Pollack on the other hand, gets a totally adiabatic, radiative-convective atmosphere. Every model that he computed had a radiative temperature gradient which was superadiabatic, so his models all have adiabatic lapse rates.

Basically the discussion I was going to engage in was why I think Jim's calculations gave that result. I think the reasons are quite simple. One is that every approximation he made in the radiative calculation is only valid for large opacities and the approach is, therefore, only good deep in the atmosphere and therefore begs the question of the radiative greenhouse where the regions of low opacity are of primary importance.

The second point concerns the boundary conditions that Pollack applies high in the atmosphere at the bottom of his assumed cloud layer. He starts each calculation at the bottom of the cloud with the assumption that the local lapse rate must be equal to the adiabatic gradient. He has very good physical reasons for doing that, but not ones that are consistent with this particular calculation. What must be done is a proper radiative-convective calculation without second guessing the result. Pollack has imposed on his solution a condition that the atmosphere be convective high up; this is exactly where it may not be convective in a pure radiative calculation.

DR. ANDY YOUNG: Your argument about the spectroscopic determination of CO_2 abundance comes out of various half-baked arguments as to the effects of multiple scattering on the spectral lines. All of the models that you have used are exceedingly simplified. I don't know of any that is wholly realistic. I think perhaps you ignored some of the work that Louise [Gray Young] has done in which she shows that you get quite a satisfactory interpretation of the observations by just using a reflecting layer. The only catch is that it doesn't explain the phase effect.

As far as the spectral reflectivity at any one time is concerned, it is fairly consistent with a large amount of CO_2. She was arguing this at a time when the scattering people were saying CO_2 was less than one percent of the atmosphere.

DR. BELTON: My reply to that is that my half-baked model does agree with the phase effect.

DR. ANDY YOUNG: But your half-baked model requires that the slope of the curve of growth of CO_2 be exactly half, as I understand it.

DR. BELTON: Absolutely not. I will show you a slide privately [cf.
Fig. 8] where I calculate the effective pressure and put in the Voigt pro-
file, and it gives the best fit to observations that is available.

DR. ANDY YOUNG: Well, we are now getting slopes of the curve of growth
up near 0.7 in some cases.

I also want to point out that sulfuric acid clouds were first suggested
by Godfrey Sill, and Louise [Gray Young] suggested them to me. All I did
was popularize them, so don't call them my sulfuric acid clouds.

DR. JONES: The bow-like waves are very strongly reminiscent of super-
sonic flow at MACH 2. So what you should look for is a wave that propa-
gates at a velocity of perhaps half of the flow velocity. Some of the
propagation velocities you listed certainly meet that criterion. If 100
m/s is assumed to be the flow velocity, then the bow-like waves might cor-
respond, for example, to a propagation velocity approximately half that
value, about a fixed object. That fixed object could, perhaps, be the sub-
solar point, which appears in some of the images to constitute an obstacle
to the flow.

The other thing I wanted to say is that there is almost a perfect
analogy to supersonic flow in ripples that occur in water waves. In the
1950's this was used by some people who couldn't afford wind tunnels, as a
way of viewing supersonic flow. You can direct a stream of water against
an object of the correct shape and excite a bow wave, just as they are
excited around ships, which is a total analog to supersonic flow. I am
surprised that no one has previously suggested that.

DR. INGERSOLL: I think it has been suggested. I think that is why
they are called bow waves. And certain problems, namely the fact that they
weren't stationary relative to the subsolar point, caused the modification
to bow-like waves. We see a further erosion to bō-like waves, suggesting
an archer's bow rather than waves from a ship.

DR. JONES: That may just be a problem of the proper frame of reference.
The object which is causing the disturbance may itself be in motion.

DR. BELTON: The first time I gave a talk on this I called them shock
waves, which I associate with supersonic flow. This caused considerable
disturbance in the audience, so that is when the terminology bow-waves was
born.

DR. JONES: I think your earlier intuition was very sound.

STRATOSPHERIC HAZES FROM MARINER 10 LIMB PICTURES

Brian O'Leary, Hampshire College

The presentation by O'Leary is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

High resolution pictures of the limb of Venus taken by the Mariner 10 television camera indicate the presence of tenuous haze layers high in the stratosphere. At least two distinct layers separated by a few kilometers in altitude appear in pictures taken in both orange and ultraviolet light and extend laterally for several thousand kilometers from the equator to high latitudes. Photometric profiles of these hazes have been analyzed to determine their vertical distribution. An "optical barometer" technique for determining the altitudes of the hazes is presented wherein the Rayleigh-scattering component is derived by comparing orange and ultraviolet (UV) brightness profiles for nearby picture pairs. This technique appears to work very well for the orange/UV pairs which were studied. The derived scale height for CO_2 gas is 4.2 km, corresponding to a temperature of $200°K$, in good agreement with radio occultation data. The optical barometer yields a pressure of 4 mb for the level at which the slant path optical depth, τ_{slant}, at the limb is unity. This level corresponds to a distance from the center of Venus $R = 6131$ km which is accurate to within 1 km provided that there is no appreciable contribution to the brightness by Rayleigh-scattering aerosols which mimic CO_2 gas.

It is possible that the limb haze layering observed between $R = 6130$ and 6140 km could be correlated with temperature inversions detected by the Mariner 5 radio occultation experiment. A model is proposed wherein the concentration of particles increases rapidly with an effective scale height of about 2 km as we descend about 10 kilometers from the limb haze ($\tau_{slant} = 1$) to the main polarization cloud deck ($\tau_{vert} = 1$).

DR. JONES: Your calculation for the mean scale height resembles very closely the procedure used by Goody [Planet. Space Sci. 15, 1817, 1967] to get the haze scale height at about the 7 mb level. Doesn't he get a very different result?

DR. O'LEARY: He gets a larger haze scale height, 3.5 or 4 km.

DR. FJELDBO: I would like to comment on the haze layer you see at 6,131 km. The Mariner 5 radio occultation data show an inversion layer at that altitude. The temperature was about 200K. I have a slide [Figure 1] illustrating this.

DR. O'LEARY: A temperature of around 200K does correspond to about a 4.2 km gas scale height which is what I'm getting from the optical barometer technique.

DR. FJELDBO: I'm convinced that we saw an inversion layer at 6131 km. However, I can't determine the exact temperature because it depends on the choice of boundary conditions near the top of the detectable atmosphere, that is, near 90 km altitude. It is interesting that the inversion layer was located at about the same altitude as your haze layer. Perhaps there is some haze in the inversion layer.
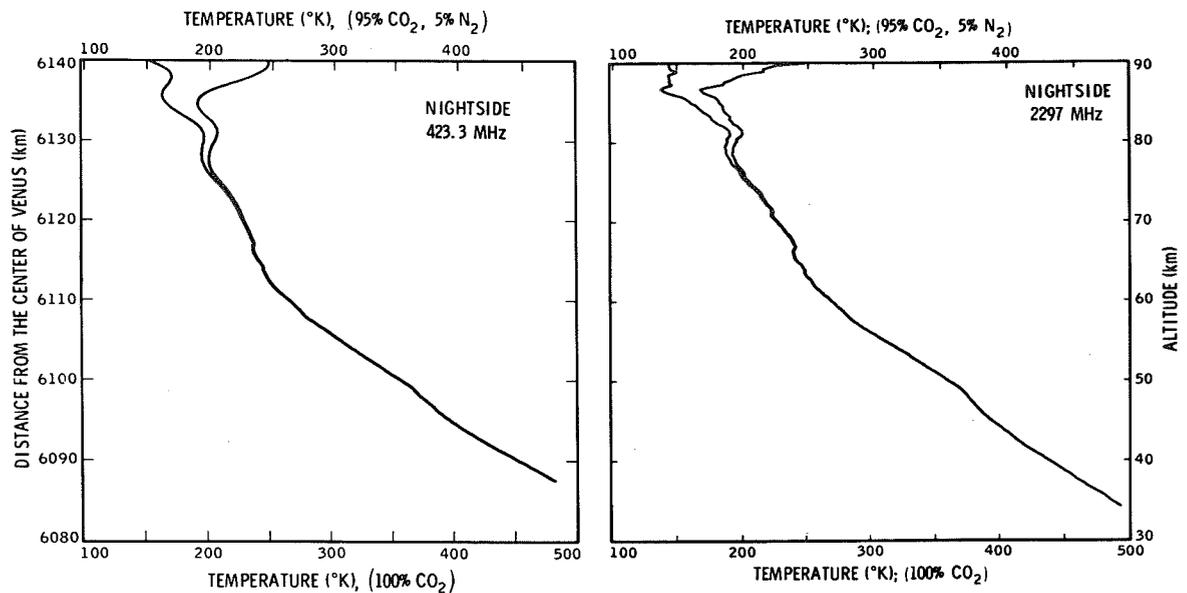
Fig. 1. Two temperature profiles deduced from the radio occultation measurements conducted on the northside of Venus during the immersion of Mariner 5. [Fjeldbo, Kliore and Eshleman, Astron. J. 76, 123, 1971]. The profile on the left side was deduced from an uplink experiment where the amplitude of a 423·3 MHz signal was measured in the spacecraft. The temperature profile on the right side was obtained with a downlink experiment where the frequency of an S-band signal from the spacecraft was measured on the Earth. The S-band link reached down to within a fraction of a km of the super-refractive portion of the atmosphere.

DR. JONES: What is the altitude?

DR. FJELDBO: 81 km, if you assume a radius of 6150 km.

DR. JONES: What is the latitude?

DR. FJELDBO: The latitude was about 37°N. I should point out that these measurements were made on the nightside. The dayside measurements also showed a layer at approximately the same altitude. However, the inter-pretation is not as clear in the latter case, because we only got useable data from the S-band link on the dayside. Thus, one could argue that the dayside layer may have been created in our calculated profiles by either instabilities in the oscillator on board the spacecraft or by phase scintillations in the ionosphere of Venus, the interplanetary medium, or the earth's atmosphere.

DR. POLLACK: As I pointed out in some earlier remarks, the earth's stratosphere is an extremely useful example. It is very interesting that rather than having uniformly mixed aerosols in the earth's stratosphere, the more typical situation is in fact to have a layered situation. I suspect that will be a common feature any time there is a relatively stable lapse rate.

Also I don't think we should assume that there is only one haze layer. It is possible that there are several layers; there is no way the Mariner pictures can disprove that. In fact the pictures suggest that the layering is the result of the same sort of processes that occur in the earth's stratosphere. And in the earth's stratosphere there typically is not just one layer, but many layers. The existence of separate layers does not necessarily imply a change in cloud composition.

DR. O'LEARY: The atmospheric thickness that we are dealing with in this single scattering problem, before we get into the noise, is about 8 km. So we're talking about one layer, which we see at all latitudes, about 5 km thick and another layer above it.   There could be more layers in the 10 to 15 km down to the level at which the vertical optical depth is unity.

DR. TAYLOR: Did you say that all the pictures are the same?

DR. O'LEARY: I didn't mean to say that.   But all of them that we have examined so far show at least the two layers.   At higher latitudes the two-layer structure tends to disappear, but otherwise, at least from the equator to 30°N, they are almost identical.   They have the distinct gap between layers.

DR. TAYLOR: Is that irrespective of whether they are over dark or light ultraviolet features?

DR. O'LEARY: We can't really tell because we are looking at the limb, and it is several hours later before we are able to photograph the area from above.

DR. TAYLOR: Have you tried to figure it out?

DR. O'LEARY: No.

DR. TAYLOR: Correlation with the images from above might provide a strong handle on the origin of the UV markings.

DR. JONES: The contrast between light and dark regions increases as you go toward the limb.

DR. O'LEARY: It's hard to tell whether the hazes are in a dark region or a light region because of the rapid rotation of UV features.

DR. ANDERSON: These haze layers are higher than the level which you see in an image from above.

DR. O'LEARY: In the limb observations there are air mass factors of the order of 100 to 200.

DR. SAGAN: I would like to point out that in the 1899 Astrophysical Journal [9, 284] there is a paper by a young planetary astronomer who had not yet received his Ph.D., Henry Norris Russell, who left the field to do something else -- I think it was his first published paper.   In the paper he argued that the then-current belief that the extension of the cusps of Venus is due to refraction was erroneous, and that it is due to scattering. And to do this there is a detached limb haze separated by at least a kilometer from what we would say is the main cloud deck, and what he called the surface.   I think that's doing pretty good for 1899.   He did his observations with the 5-inch finder of the Princeton great equatorial telescope.

DR. DOLLFUS: I would like to show a slide [Figure 2].   This is to compare the model given by Dr. O'Leary with a model deduced from our ground-based measurements at the limb of the planet [Icarus 17, 104, 1972].   The optical limb is at a height of 63 km (R = 6115 km), which refers to the upper part of the main cloud layer observed with grazing incidence at the poles.   This should be compared with the Mariner 10 value of 67 km (R = 6119 km) for the model at 50 mb, or 77 km (R = 6129 km) for the curvature of the limb.   In the measurements of the elongation of the cusp during superior conjunction the upper haze region is observed, and the value is 88 km (R = 6140 km).   This relates to the top of the thin upper haze layer above the poles.   The model of Dr. O'Leary with p = 4 mb gives 80 km (R = 6131 km).
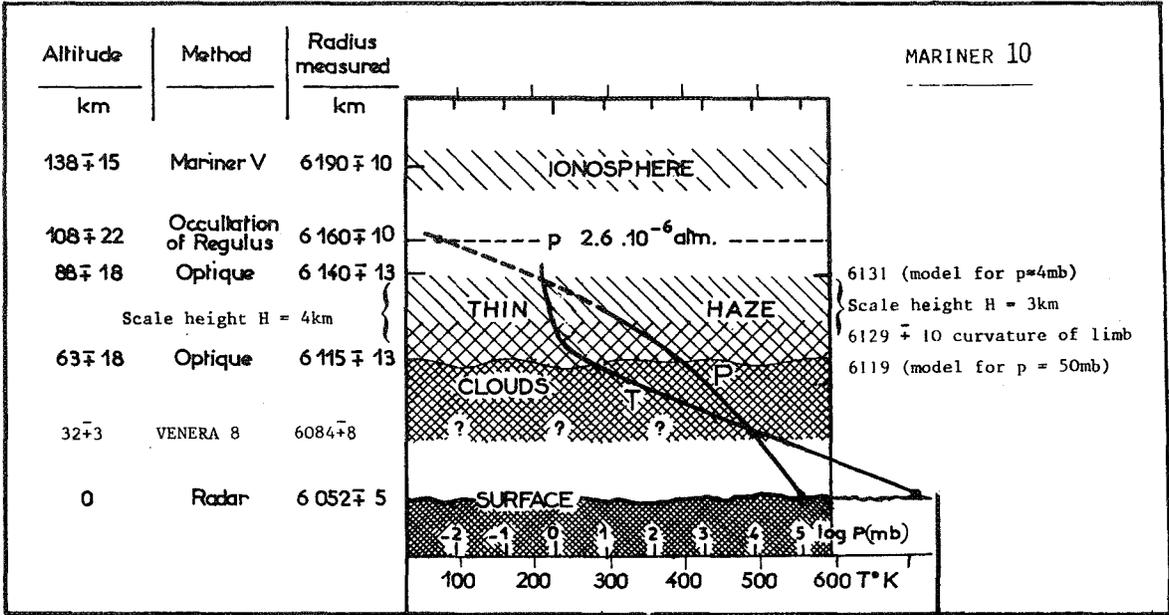
Fig. 2. Diagram shown by Dollfus to compare different aerosol layers deduced from ground-based observations and Mariner 10 measurements.

MARINER 10 OCCULTATION MEASUREMENTS OF THE LOWER ATMOSPHERE

Arvydas Kliore, Jet Propulsion Laboratory

The presentation of Kliore is largely contained in the paper by Howard et al. in Science [183, 1297, 1974].

DR. INGERSOLL: The next paper is related to this one, so we will defer the discussion until after Woo's paper.

MARINER 10 OBSERVATIONS OF SMALL-SCALE TURBULENCE

Richard Woo, Jet Propulsion Laboratory

The presentation of Woo is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

In this paper we develop a technique for estimating the outer scale of turbulence in a planetary atmosphere using dual frequency radio occultation measurements. This technique is based on the frequency dependence of the temporal frequency spectra for the log-amplitude fluctuations, and is particularly useful when probing the upper atmosphere where the transverse velocity of the line-of-sight path is decelerating very rapidly.

We apply this technique to the region of strong turbulence located in the vicinity of 60 km (~180 mb) on the dayside of the atmosphere of Venus using the Mariner 10 S/X radio occultation measurements. We find that, contrary to earlier findings from Mariner 5, the outer scale of turbulence is at least 5 km. It appears, therefore, that the outer scale of turbulence is as large as the vertical extent of the region of strong turbulence. Estimates of the structure constants for refractive index and temperature fluctuations indicate that the turbulence is stronger than that measured near the earth's tropopause.

DR. ANDY YOUNG: I want to ask why you are so concerned about little discrepancies between Mariner 5 and Mariner 10 results. We see in many other types of data that the atmosphere of Venus changes with time. For example, from our observations of the amount of CO_2 we found that the cloud top height is varying, yet the cloud top temperature is essentially independent of time. That means that at a given pressure level, there are fluctuations in the temperature. And if you look at the long-term variations in the amount of CO_2 above the clouds this corresponds to temperature variations at a given pressure level of the order of 10K.

So I shouldn't be at all surprised that when you look at Venus one time and a couple years later you find a 10K or 15K difference in temperature profile.

DR. KLIORE: That's true. And the uncertainties at those levels are really so high that we really shouldn't defend the differences.

DR. JONES: You mentioned the dropout of your signal at 40 km or a little higher than that. Could that be another region of turbulence so intense that you can't see through it?

DR. WOO: We still see turbulence effects there.

DR. KLIORE: That is not the reason for loss of signal. The reason we can't go any lower is because the refraction angle cannot go higher than about 13°. At that point the signal stays within the atmosphere at a constant altitude.

DR. JONES: But you said you lost the signal before you expected to.

DR. KLIORE: Yes, and that might be due to another region of turbulence.

DR. STONE: Doesn't your result for the outer scale imply that you are getting energy generated to create this turbulence on scales as small as 5 km and not below that?

DR. WOO: Yes.

DR. STONE: The implication is that turbulence is being generated with scales of 4 or 5 km, which I think is very interesting because any kind of small-scale instability you can think of would have a scale of the order of the scale height which is about 5 km. So immediately you have all sorts of possibilities.

DR. POLLACK: I think that the bumps and the bowing out of the transmission profile could be due to layering within the clouds. And that type of layering would also lead to significant fluctuations in the divergence of the solar flux, which would produce fluctuations in temperatures. One of my slides showed that you can get a lot of solar energy deposition where there are strong gradients in the cloud properties. In fact your curve may be telling us a lot of interesting things about the solar deposition pattern.

DR. O'LEARY: Dr. Fjeldbo, I have a question about the 200K temperature which you find for the region of the limb haze. What kind of error would you put on that, in light of what Arv was saying about the great uncertainties?

DR. FJELDBO: The temperature we compute for the inversion layer observed at 81 km altitude during Mariner 5's immersion depends on the choice of boundary condition near the top of the atmosphere and on the composition. Assuming a boundary temperature in the range 150 to 250K at 90 km altitude and a composition ranging from 100% CO_2 to 95% CO_2 and 5% N_2 yields an inversion layer temperature of 190 to 210K.

DR. O'LEARY: The scale heights that I'm getting are very consistent with 200K.

DR. AINSWORTH: In the region of turbulence at 45 km the Venera 7 and 8 retrograde horizontal winds decrease rapidly from over 100 m/s to 15 to 40 m/s.

DR. WOO: Right. It is apparently a region with a lot of wind shear.

DR. KRAUSS: Rasool made some suggestion about cloud layers of mercury compounds.

DR. WOO: That was based on fluctuations in the Mariner 5 amplitude data. I think the fact that we see phase as well as amplitude fluctuations in the case of Mariner 10 means that most of the Mariner 5 amplitude fluctuations were indeed due to turbulence. However, absorption layers could still be present.

MARINER 10 INFRARED OBSERVATIONS

Fred Taylor, Jet Propulsion Laboratory

The presentation by Taylor is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The Infrared Radiometer experiment on Mariner 10 measured limb darkening curves for Venus in two spectral intervals, one near 11 μm and the other near 45 μm wavelength. In this paper, these are analyzed in terms of the vertical opacity profile at each wavelength over a limited altitude range, approximately 60 to 80 km above the surface of the planet. Accurate multiple scattering calculations are used to show that both opacity profiles are consistent with a model containing a cloud of 1.1 μm radius sulfuric acid droplets, and a small amount of water vapor. Profiles of particle number density and humidity versus height are presented.

DR. CLARKE: Will the infrared experiment lead to an independent verification of the sulfuric acid cloud composition?

DR. TAYLOR: I don't know yet. But it will be very interesting to see if the relative opacities at these long wavelengths are consistent with the sizes and composition deduced at much shorter wavelengths. [cf. more recent abstract above]

DR. CLARKE: I thought that the residuals you mentioned were below the resolution of the instrument, which I understood to be 3°K.

DR. TAYLOR: Oh, no. It's much better than that. It was about .05°K in the long wavelength channel. It was poorer in the 12 μm channel, which is why I didn't show residuals in the 12 μm channel.

DR. POLLACK: Could you explain how you interpret the break in your 12 μm channel results.

DR. TAYLOR: I suspect it will require two cloud layers. As the zenith angle varies the relative weight of the two layers changes. [cf. Taylor's paper in J. Atmos. Sci. 32, June 1975]

DR. POLLACK: One thing that worries me a bit is the fact that the bottom cloud layer is much more transparent than the top one. That's the reverse of what I might intuitively think, since you would expect smaller particles in the higher cloud layer.

DR. TAYLOR: That fascinates me, too.

DR. ANDY YOUNG: That's easy to do. If the sulfuric acid is made high up and percolates down through the stratosphere, then at the tropopause it is being mixed into the hotter atmosphere and being destroyed, so it has a lower mixing ratio. So it's easy to do. You can wave your arms and make anything.

ATMOSPHERIC STRUCTURE AND HEATING RATES

Andrew Lacis, Goddard Institute for Space Studies

The presentation by Lacis is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

Ground-based observations and Venera 8 entry probe measurements are used to infer the vertical distribution of cloud particles in the atmosphere of Venus. In the cloud-top region, from a few mb to a few hundred mb pressure, the mixing ratio of cloud particles to gas increases with depth. The visible clouds are diffuse with a scale height of about one-half of the gaseous atmosphere. Although the presence of significant vertical structure could escape detection by available observations, the diffuse haze appears to extend over at least 20 km in altitude. The Venera 8 measurements suggest considerable vertical structure in the deep atmosphere. A unique solution for the cloud structure is not possible, but if it is assumed that the cloud optical properties are independent of height then some characteristics of the relative cloud structure can be deduced. Under this assumption the results show a maximum cloud density near 40 km, a nearly homogeneous particle mixing in the region from ~40 to 50 km, and a fairly sharp cloud bottom near 30 km. Relative maxima in the cloud density are also implied near ~55 and 10 km, but with much greater uncertainty.

From ground-based observations we find that Venus absorbs approximately 22.5% of the incident solar flux; nearly 4% of the incident flux is absorbed in the UV ($\lambda < 0.4$ µm), 5% in the visible ($0.4 < \lambda < 0.7$ µm), and 13.5% in the IR ($\lambda > 0.7$ µm). Only ~1% of the incident flux (~5% of the absorbed flux) is associated with the UV contrast differences. Most of the solar energy is absorbed above 55 km, with the maximum heating probably near the $\tau = 1$ level. The heating rate has a strong dependence on the cloud particle distribution, and can exhibit considerable vertical structure. The solar heating at the ground is in the range ~0.1 to 1% of the incident solar flux, unless the ground albedo is near unity.

DR. ANDY YOUNG: I would just like to say that the ultraviolet absorber, whatever it is, certainly plays an important part in the absorption of solar energy. And one thing that should be kept in mind is that, if it behaves like any reasonable substance, as you go down further into the cloud and the temperatures are higher, that absorption must shift out to longer wavelengths. So there continues to be light available that wasn't absorbed higher up.

DR. JONES: How sensitive are these models to the location of the cloud boundaries and to the assumed albedo of the atmosphere?

DR. LACIS: The exact choice of boundaries doesn't matter much. But the uncertainties in the solar zenith angle and the spherical albedo cause serious problems. The uncertainty of 2.5° in the zenith angle causes roughly a factor of two uncertainty in the derived optical thickness.

DR. JONES: How about the albedo of the ground?

DR. LACIS: That is a derived number, not an input parameter. The high ground albedo obtained arises from the fact that the transmission near the ground is relatively high compared to the transmission in the middle of the atmosphere. So the high ground albedo is required to yield the observed transmission near the ground.

DR. HAPKE: The lowest albedo you got for the ground was about 60 percent, and that's a very high albedo for any natural rock, even a highly pulverized rock. That's about appropriate for very pure natural quartz.

Most silicate rocks -- and I assume that the surface of Venus is silicate rock -- contain some iron, and as soon as iron is added to silicate it drops the albedo way down.

DR. LACIS: It is a high surface albedo. If you assume a lower transmission than reported by the Russians for the last data points, then you can make it lower.

DR. JONES: I think it could mean ground fog.

DR. SAGAN: If one believes that you're actually looking at a surface albedo and if one also believes that laboratory experience on the albedo of rocks is relevant, then you would like to bias your results toward the lowest possible surface albedo. If I remember your results right, the lowest possible surface albedo comes in the case of the clear lower atmosphere. Is that right?

DR. LACIS: Right.

DR. SAGAN: So would you not think this provides some evidence, a slight tilt in the direction of a clear lower atmosphere?

DR. LACIS: Sure.

DR. BELTON: I was very intrigued by the number you mentioned for the thermal optical depth required for the greenhouse effect to yield the observed temperature at the surface. Isn't it true that you couldn't conceivably get an optical depth of 1000 with 95 atmospheres of CO_2?

DR. POLLACK: In greenhouse discussions it is inevitable that people will always look at the wrong part of the atmosphere. They always look at the bottom of the atmosphere, because that's where the big numbers are.

The real problem is to achieve the correct radiative balance toward the top of the atmosphere. I think aerosols play a very key role in the upper half of the atmosphere. In fact, a very useful constraint on aerosol properties, aside from the Venera 8 data, is obtained by demanding that they achieve this sort of thermal balance.

DR. ANDY YOUNG: In fact sulfuric acid aerosol is the perfect greenhouse material because the stuff is transparent in the visible and blacker than hell in the infrared.

DR. SILL: In reference to the surface albedo which Hapke and Sagan were talking about, if you believe some of the properties of carbon dioxide that have been proposed, then the main constituent of the surface could possibly be things like calcite. And even if this is diluted with some iron-bearing clays, the albedos are still around 70, 80, 90 percent.

DR. SAGAN: You're not even right to the order of magnitude.

DR. SILL: I've measured them.

DR. HAPKE: Pure calcium carbonate, yes. Try some natural rock.

DR. JONES: That will depend upon grain sizes.

DR. HAPKE: Yes. But, put a little iron in there and the albedo falls way down.

DR. ROSSOW: I notice you have rather wide error bars on the Venera 8 profile. If you derived the minimum ground albedo that would still allow the transmission to be within those error bars, what would you get?

DR. LACIS: I haven't done that yet. It would have a significant effect. Through most of the atmosphere the error limits claimed for the transmission are not as important as other uncertainties, such as those for the zenith angle and spherical albedo.

But the ground albedo is practically determined by the value at the last measurement point. If that value, instead of being one percent, is half a percent, or zero, that would certainly reduce the ground albedo.

GROUND-BASED CO_2 AND H_2O OBSERVATIONS

Edwin Barker, McDonald Observatory

The presentation of Barker is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

During the 1972-74 period, 115 pairs of CO_2 and H_2O abundance determinations have been made with the coude scanner of the 2.7 m reflector at McDonald Observatory. These observations were made on 35 days usually within one to three hours of each other. The pairs of observations were made over the same area of the illuminated disk of Venus with the guiding, seeing and slit placement errors less than 15% of the disk diameter.

A correlation analysis of the pairs of observations grouping them into eight periods of time which corresponded to telescope observing runs or periods of similar phase angle shows a lack of correlation in all except one period. For this set, H_2O abundances were positively correlated with the relative CO_2 line strengths for measurements of the 8689 Å CO_2 and 7820 Å CO_2 bands made on the same day. Comparison of abundances on some 25 individual days shows a positive correlation on one day and a marginal negative correlation on two days with no correlation on the remaining 22 days.

On the basis of the lack of correlations, one has to conclude that either the H_2O level of line formation does not fluctuate in phase with the observed CO_2 absorption fluctuations or the horizontal distribution of the H_2O vapor must be inhomogeneous.

DR. INGERSOLL: Since we are way behind schedule, I will have to stifle discussion until after the next paper.

LONG-TERM VARIATIONS OF THE CLOUDS

Audouin Dollfus, Observatoire de Paris

The presentation by Dollfus is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The large number of UV photographic pictures taken by different observatories throughout the world are grouped for analysis at the IAU Planetary Photographs Center of Meudon Observatory.

Three horizontal "V", "Y", or "psi"-shaped dark-hued cloud features are usually aligned along the Equator and move 110 m/sec westward in a planetary-wide rotation. The more intense and distinctly "V"-shaped features last several weeks. The smaller-scale cloud-structures usually show significant changes after each successive rotation in 4 days around the planet. The average UV contrast is 23% but can fluctuate from being undetectable to 37%.

For periods of several years, the polar areas can be intermittently covered by a white cloud. This never occurred between 1962 and 1966 but happened 25% of the time for at least one pole since 1967, and only when the planet was in a particular half of its orbit. These polar clouds are ephemeral and usually last several weeks or months; they evolve independently for the two poles.

The Mariner 10 configurations are typical of the 3 equatorial dark "V"-shaped features of similar intensities, with two white poles.

DR. SMITH: The only two cases of contrast in the yellow which you showed were taken in 1942 and 1943 when it must have been very difficult to obtain good film. Is it possible that film defects caused apparent contrast?

DR. DOLLFUS: No. We checked that.

DR. JONES: I think it should also be emphasized that not only did Mariner 10 take photographs at one time but also essentially at one phase angle. All the pictures that have been shown were taken between phase angles of 20 and 30 degrees.

Do you have UV photos from the 1943 series when you had high contrast in the yellow?

DR. DOLLFUS: No. There were no opportunities. The telescope available was a refractor, not transparent in the UV.

AERONOMY OF VENUS

Michael McElroy, Harvard University

I'll first give an update of the present status of the Mariner 10 UV results, since Lyle Broadfoot couldn't be here today. I should say from the outset that the analysis is still in a highly preliminary stage. The reduction of the data is taking quite a bit of time for a number of reasons, not the least of which is the care and attention that Broadfoot has been giving to make absolutely sure that there are no spurious effects.

The great problem with this instrument relates to the fact that it isn't really a spectrometer; one doesn't really know with absolute confidence what one is looking at. Broadfoot has been working very hard to make sure that some of the things that are seen are not due to scattered light and so on.

But I'll give you an account of the data as they now stack up, and I'll point to some of the more interesting features of the observations which seem to be real and which seem to be surviving as the analysis of the data continues.

The instrument has a series of channels or detectors centered at various wavelengths, chosen mostly as a compromise between targets at Venus and Mercury (cf. Figure 1). Each channel has an effective width of about 10 Å.

We have a channel at 304 Å designed to detect the possible presence of He$^+$ which has a resonance line at that wavelength.

The 430 Å channel was chosen primarily to provide a measure of background, rather than to look for any specific airglow emission. In fact the experiment basically is to look at the exosphere and try to detect its luminosity, either the nightside atmosphere or the dayside atmosphere. It's an airglow experiment.

584 is the resonance line of helium. This channel was designed to look for helium in the atmosphere of Venus and look for helium in the atmosphere of Mercury.

740, 867 and 1048 are channels which were on this experiment primarily to look for inert gases at Mercury, although these channels also have some sensitivity to things like O$^+$. But, it would be very hard to see O$^+$
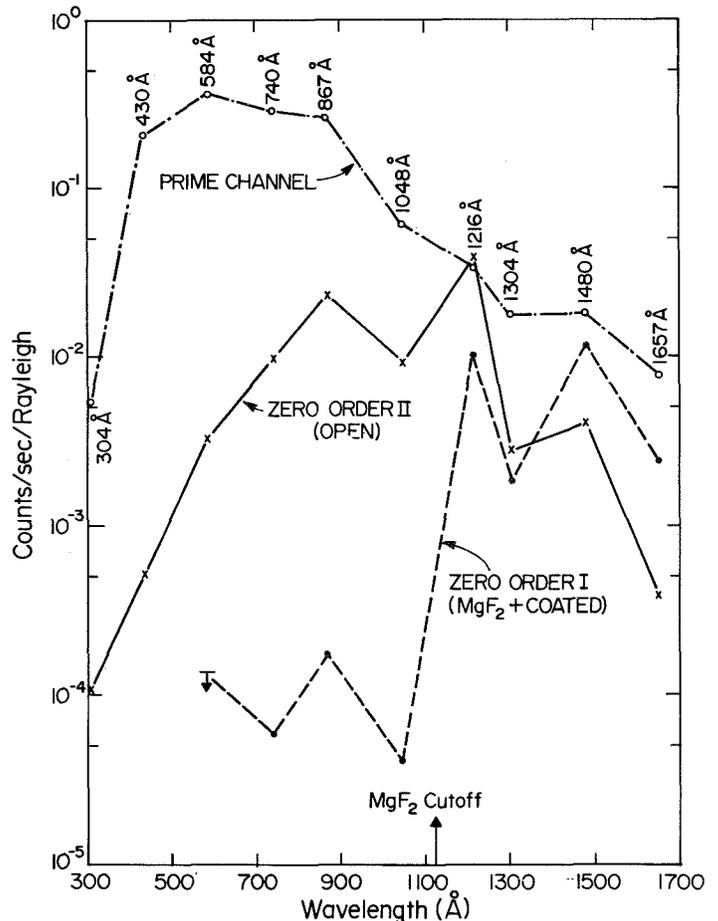

Fig. 1. *Spectral sensitivity of Mariner 10 ultraviolet grating spectrometer.*

unless there's a very large amount present. I will not make any comments about the possible abundance of O^+ on the basis of these data, although eventually it will be possible to at least set limits on the amount.

1048 was primarily included in order to look for the resonance line of argon. In fact, the 1048 channel turned out to be very useful because, although it perhaps did not see argon, it did see hydrogen at Lyman-beta, 1025.7 Å, which slips into the 1048 Å channel with about 1 percent efficiency. It gives us, therefore, a better handle on the hydrogen distribution. Eventually we will be able to carry the hydrogen analysis into close distances from the planet with some confidence. So there's some redundancy from the pair of channels, 1048 and 1216, the resonance line for Lyman-alpha.

1304 is the resonance line of atomic oxygen and provides a simple way to look for CO_2 in the atmosphere of Mercury as well as giving us a direct method of measuring the abundance of oxygen in the upper atmosphere of Venus. We have some nice data from 1304 which raises, however, many more questions than it answers at this point.

1480 was also chosen as a background channel. We see some of the more intense emissions from Venus in the 1480 channel. This would have been a surprise at the time the experiment was conceived, but it was not really a surprise when the data were acquired because Warren Moos at Johns Hopkins had by that time taken some very nice UV spectra of Venus which showed that the 1480 channel was located in a spectral region at which Venus showed bright emission.

1657 is the resonance line of atomic carbon. We found 1657 to be very bright, and, again, there is a difficulty in providing adequate excitation processes for that emission.

There were also two zero order channels with the response functions shown in Figure 1. Grossly speaking, we can think of zero order 2 as a channel sensitive to the shorter part of the wavelength spectrum, and zero order 1 as a channel sensitive primarily at the longer wavelengths.

So this gives an additional handle on the nature of the emissions. We can see not only the specific emissions in 10 Å bands, but we also have an integrating device with some spectral discrimination because of the presence of the two filters, one a magnesium-flouride filter with copper iodide coating and the other an open detector.

Table 1 is a summary of some of the emissions seen at Venus, and a comparison between the number of counts observed in different channels. This gives the counting rate seen by the instrument. The instrument also looked at the earth on the way out, so we have some terrestrial data for comparison purposes. Table 1 shows the counts observed by the instrument at 282,000 km from the earth, and counts from Venus at two different distances, 194,000 and 113,000 km.

One of the big surprises in these data is the extraordinarily high signal in the zero order channel, which has greater sensitivity at the longer wavelengths. On the basis of the counts in the two zero order channels, it appears that this very high signal comes from somewhere longward of about 1300 Å and shortward of about 1700 Å.

How bright is it? It's about a factor of 30 or 40 brighter than the earth as seen by the same instrument. We're talking about exceedingly bright airglow intensities, if that's what it is, on the order of a mega-Rayleigh or perhaps a little bit more than that. So we're talking about emission rates of the order of 10^{12} photons cm^{-2} sec^{-1}, if the observation is indeed due to airglow.

Probable emitting species	Channel (Å)	Count rate (sec^{-1})		
		Earth 282,000 km	Venus 194,600 km	Venus 13,000 km
Zero order	1150-1700	880	15,800	26,200 (4,000)
Zero order	200-1500	640	8,480	12,160
He$^+$	340	5	22	100
Background	430	4	15	67
He	584	100	187	233 (0.61)
Ne	740	7	21	87
A	867	17	31	100
A	1048	21	39	147
H	1216	350	506	693 (19)
O	1304	120	127	267 (17)
CO, fourth positive	1480	4	173	987 (55)
C	1657	~1	53	260 (30)

*Table 1. Comparison of dayglow observed at Venus and the earth by Mariner
10. The data for the earth were obtained at 2130 G.M.T., 3 November 1973,
at 282,000 km, with a cone angle of 85.3°. Data for Venus were obtained on
5 February 1974 at two distances; the conditions were: 194,000 km, 2315
G.M.T., cone angle 151.8°; and 13,000 km, 1710 G.M.T., cone angle 131.0°.
The numbers in parentheses above give the approximate intensities, in units
of 10³ Rayleighs, for the Venus observations at 13,000 km.*

Now, of course, the continuing concern is that we may be seeing long
wavelength white light, or spurious light, if you like, from the planet. I
don't think the problem is by any means solved at this point. But I can
simply report that Broadfoot feels reasonably confident at this point that
the behavior of the two channels is strongly indicative of a real emission
in the spectral region noted above, rather than long wavelength transmission
and spurious counting of scattered light from the planet.

The first surprise in the data was detected in the Lyman-alpha channel
(cf. Figure 2). Hydrogen is the lightest gas, and we'd expect to find a
hydrogen cloud around Venus and therefore to see Lyman-alpha first on
approaching the planet.

Well, that wasn't the case. We saw some of the zero order channels
creep up almost as soon, or perhaps sooner than the Lyman-alpha. The geo-
metry of the approach is such that we scan first toward and across the dark
limb. Approaching the limb, we see Lyman-alpha. But we also see an
increase in the zero order channels not all of which may be attributed to
Lyman-alpha.

Figure 3 shows some of the other channels, with Lyman-alpha included
for reference. The drop in Lyman-alpha occurs on crossing the dark limb of

the planet, because (1) the astronomical sky background is blocked off, and, (2) any true airglow from the other side of the planet is also blocked off. When these data are analyzed a fairly consistent picture emerges for Lyman-alpha.

You can see that the channels 1657, 1480 and 1304 rise at very large distances from the planet. We're not talking about very large intensities here. We're talking about emissions for the 1304 channel at a level in the few tens of Rayleighs. Likewise, for 1480, we're talking about something like 50 or 100 Rayleighs at the half intensity point. The emissions are thus relatively faint. The surprising feature however concerns the extended nature of the luminosity. The emissions appear to have a scale height larger than that of atomic hydrogen.

Fig. 2. Hydrogen Lyman-alpha emission rate versus minimum distance of the line of sight from the center of the planet. For comparison, the data obtained by Mariner 5 scaled down by factors of 0.25 and 0.7 are also shown. [After Broadfoot et al. Science 183, 1315, 1974.]

That's not the only surprise about this. Charlie Barth would have seen this behavior had it been there for Mariner 5, and it is my understanding that it wasn't there. One has now to worry that we may be dealing with a time-dependent phenomenon. What could have changed on Venus between Mariner 5 and Mariner 10? I'll come back to that. There are some serious indications that Venus looks quite a bit different, which raises questions about the role of the solar wind in particular in the physical processes which occur in the upper region of the atmosphere.

Another point that might be raised at this point is related to possible confusion in the instrument due to scattered light from the bright planet or from the sun. The instrument has a sun shade which shields the detectors from light which might enter directly from the sun. Any light which gets in would have to be multiply scattered off various surfaces outside. There are similar precautions to protect the instrument from spurious signals due to scattering of planetary radiation.

We have data from the bright side of the planet taken after encounter, when there might have been a relatively large chance for stray light to enter the instrument. Intensities at 300 km on the day side were lower than at 50,000 km above the dark limb. So the instrument itself seems to be telling us that it worked pretty well.

DR. CLARKE: It seems to me you may get more scattered light above the dark limb, because as you get to the planet you're looking down towards the spacecraft, but when you're on the bright side you're looking up, away from the spacecraft. So if there is any reflected light off parts of the spacecraft you may see it more on the incoming path.

VENUS: DARK LIMB DRIFT

ALTITUDE OF LINE OF SIGHT (km)
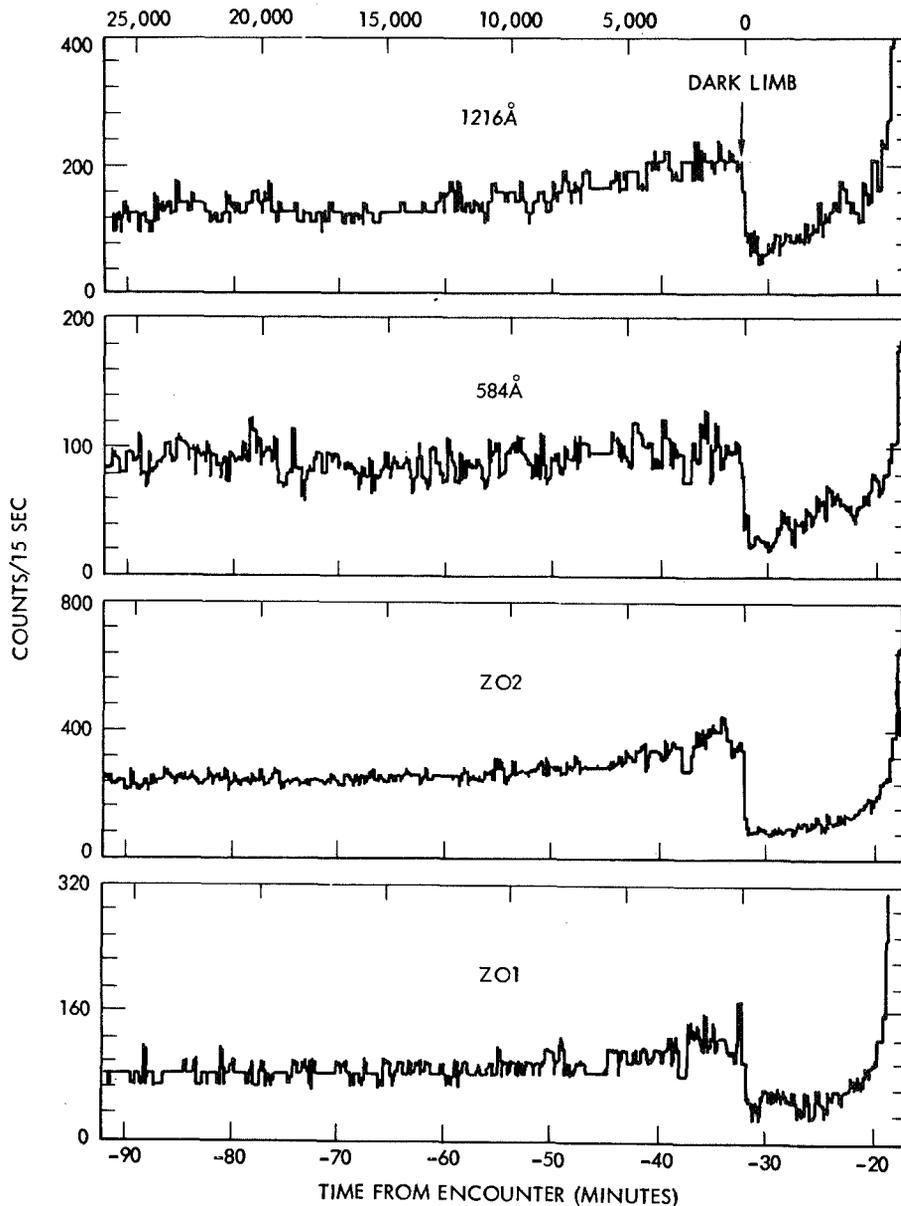
Fig. 3. The data obtained at 1216 Å, 584 Å, and from the two zero-order channels are plotted against time from Venus encounter. These data were obtained during the dark limb drift experiment. [After Broadfoot et al., Science 183, 1315, 1974.]

DR. MC ELROY: Well, the shield during the approach is on the sunlit side of the instrument. On the way out the planet is shining essentially from the other side. I don't really think there is a much larger chance of having scattered light on the approach.

DR. CLARKE: The cone angles are low, about 60 degrees, when you're coming in on the dark side. That means you're looking toward the bottom of the spacecraft. And on the way out you're looking up.

DR. MC ELROY: I'm not sure we're going to resolve it here. So let's take it up later.

These Lyman-alpha data are in generally excellent agreement with the Mariner 5 results. There are perhaps some differences in the intensity, but the spatial variation is in excellent agreement.

The 1304, 1481 and 1657 data change by a very small amount as you cross the dark limb. If you believe that the data are real, the only way to make much sense out of that is to say that one is in an optically thick emitting atmosphere, so that you can't range far enough to see the bright limb. And that's rather strange and curious. So also is the very bright emission that one sees at 1481, not only here but also in the Moos experiment.

The only simple explanation for the emission one sees at 1481 is, I think, the proposal which Warren Moos made, namely, resonance radiation emitted by CO.

Let me remind you that one of the major surprises in the Mariner 5 data was the fact that there appeared to be two components to the Lyman-alpha emission. In most interpretations of these components, one is credited to resonance scattering by atomic hydrogen and the other to resonance scattering by non-thermal hydrogen, e.g., resonance scattering by deuterium - there are a number of ideas in the literature.

Now the first look at the Mariner 10 data confirms very nicely the reality of the inner component, which I would have thought was the more suspect of the Mariner 5 results. We essentially get agreement, apart from slight differences in the intensity. But there's no doubt that it is real.

And the Mariner 10 results tell it to you in two ways, because the Lyman-beta result confirms that we are indeed seeing atomic hydrogen, and that we're seeing the sort of saturation one would expect with the estimate for optical thickness with the transition to optically thick behavior taking place at about the right location. So there's little doubt that we're seeing atomic hydrogen here, and the deuterium model is simply not correct.

Continued careful examination of the Mariner 10 data may reveal the second component. It's perhaps premature to announce it, but I think there are strong indications that the second component is in fact there, and that we are indeed seeing a hot - quote/unquote - component with a temperature of perhaps 1100, 1200 K, as a very crude estimate.

I should say that the best interpretation of the hydrogen results would now say that the exospheric temperature of the planet is about 400 K. From the helium results it looks as though it might be a little hotter; but, I think that's not terribly significant, or at least it should not be given too much weight at this time.

The 1304 results indicate that one percent is an absolute upper limit for atomic oxygen in the upper atmosphere near the ionosphere peak and you can force better agreement at large distances from the planet with about a tenth of 1 percent. The problem now is that you don't have enough intensity to account for the data close to the planet. Venus is a lot brighter than it has any right to be, and there are excitation processes which are not clearly identified by anybody.

I do not know of any way to explain in a simple way the very high intensities seen by Moos, not to mention the Mariner 10 results in the - quote/unquote - fourth positive bands of CO. If you try to do it with resonance scattering by CO you would have to assume that the upper atmosphere was chock full of CO, that CO was the major constituent. There are other reasons to believe that that's simply not reasonable, as I'll try to show you a little later.

Let me make some remarks about what we see in the helium channel, 584 Å. The intensity is about 600 Rayleighs. It's two or three times brighter than the earth at 584, and we have looked at the earth with the same instrument at 584.

We are in the process of analyzing the helium data to try to see what we can say about the helium concentration in the planet as a whole. And if I can anticipate some of the conclusions that we'll draw in the next part of this review, about the degree of mixing in the upper atmosphere, it appears as though Venus has quite a lot of helium. I'm hesistant to quote numbers, but it's not likely to be less than 10^{-5} according to our present analysis.

The reason I'm hesistant is that in trying to analyze the data with an optically thick spherical scattering program which Yuk Ling Yung and Nien Dak Sze and I have been doing, and just allowing for resonance scattering as the excitation process, that is leaving out esoteric excitation mechanisms, we derive a mixing ratio which is of the order of 3×10^{-5} for the bulk mixing ratio in the planet as a whole. But it could be higher, primarily because there is serious uncertainty as to the precise value for the scattering efficiency of helium. In the analysis to date we used a rather high value for the solar flux at 584 Å, three times higher than Hinterregger. If we were to adopt a g value lower than the value derived by Donahue, the amount of helium derived from our analysis would go up accordingly. So the helium abundance may actually turn out to be significantly higher.

It's worth pointing out, of course, that if Venus outgassed helium at the same rate as the earth, and if you simply ignored escape of helium, you'd expect the planet to have a mixing ratio of helium something like 10^{-3} or 10^{-4}. That would be the value appropriate for a production rate of 10^{6} atoms cm^{-2} sec^{-1}, ignoring escape. And at 400 K the thermal escape of helium is negligible. So in the absence of significant non-thermal mechanisms helium could indeed accumulate in the atmosphere. It'll be interesting to see how this analysis continues.

Now let me shift gears and go on to the other subject I am to talk on, which consists of some comments on the work that's going on in various aspects of Venus' aeronomy at the moment.

I'll talk a little bit about the hydrogen escape problem, and about the upper atmosphere and what I think you can say about the degree of mixing in the upper atmosphere of Venus. I'll then say something about the chemical processes which are taking place in the middle atmosphere, and how we now see the question of the CO_2 stability. I'll try to make the point that it's not just simple aeronomy, but, in light of what we've been hearing the last few days here, there is a real possibility that the clouds of Venus are limited and controlled in some sense by the supply of oxygen required to oxidize the sulfur so that the entire upper atmosphere is coupled to the cloud and indirectly then to the surface in a really staggering way.

In my opinion one of the most serious problems in trying to make the sulfuric acid cloud model work is to find an adequate supply of oxygen to turn the sulfur into H_2SO_4. If I believe John Lewis' discussion of the thermal chemistry, then COS should be the most abundant form of sulfur in the lower atmosphere. The problem is to make H_2SO_4, and in particular the problem is to find an adequate source of free oxygen to oxidize sulfur carried up to cloud level presumably as COS.

The obvious place to look for the required source of O_2 is in the photochemistry of CO_2, and to invoke downward transfer of oxygen to oxidize sulfur. So let me talk a little bit about that.

At this point we omit a portion (∼one-third) of McElroy's talk. This discussion, primarily of the photochemistry of CO_2 and the relevant transport processes, closely followed the paper by Sze and McElroy which will appear in Planetary and Space Science.

I don't think anybody is going to get very much out of Figure 4. Don Hunten assures me that Venus Pioneer is going to solve all the problems of Venus. I just want to point out that this is what he has to measure to solve all the problems. For example, the key constituent is OH. Its mixing ratio is 10^{-12}. And that's not all, because you also have to worry about the chlorine compounds, for example CLOO and ClO, and Cl and Cl_2.

Fig. 4. Model of number densities in the Venus atmosphere.

I understand that Belton is arguing that HCl is not mixed in the atmosphere of Venus, and he has some definitive observational data to show that the mixing ratio falls off at altitude. Of course that's no big surprise. If you look at any of these figures you see that the HCl mixing ratio is a function of altitude. If you reduce the abundance of molecular hydrogen in the lower atmosphere, you can make it fall off faster than in Figure 4. If you change the eddy coefficient you can make it fall off faster earlier.

There is plenty of freedom in the aeronomy to adjust to even Belton's strange HCl profile. I confess I don't understand how he can get it; but, there are ways in which you can do some interesting aeronomy on the basis of that observation.

Incidentally, I think it important to note that the mixing ratio of chlorine should remain constant with height. So if HCl drops off, one is essentially forced, I think, to say that atomic chlorine comes in as a major constituent. Something must replace the HCl, in the absence of significant condensation processes to remove the chlorine from the system. With present aeronomical models, that replacement is by chlorine. Atomic chlorine becomes fairly important.

The question I want to address in the remaining minutes here is: How do you supply molecular oxygen to the sulfur to make H_2SO_4?

First of all, there is a maximum rate at which the chemistry can supply molecular oxygen to make H_2SO_4. That's determined by the total photolysis

rate of CO_2. So if you take all the oxygen liberated by photolysis of CO_2 and tie it up as sulfuric acid, you still have a source which is limited to about 2 or 3 × 10^{12} molecules cm^{-2} sec^{-1}. That in turn says something about the time constant for a photochemical smog, such as the sulfuric acid would be in this case. I think Wofsy, and perhaps others here, have probably talked about that.

The time constants are very long, and, of course, how long depends exactly on how thick you believe the cloud to be. But we're talking about rather long time constants - time constants which are almost certainly larger than 10^7 seconds for the formation of any significant optical thickness of a sulfuric acid cloud by chemistry.

The amount of oxygen that you get down to the cloud is rather critically a function of the mixing processes occurring in the stratosphere. And the faster you mix it the more readily you can get the oxygen down. You can get a few times 10^{12} molecules cm^{-2} sec^{-1} down to the cloud if you have eddy co-efficients in the cloud region or immediately above the cloud, which are very high indeed; I mean 10^7 cm^2 sec^{-1} or so. I'm not sure that this really makes much sense.

If, on the other hand, the effective eddy coefficient is down in a more reasonable range, around several times 10^5 cm^2 sec^{-1} or so, then the supply rate of oxygen is very small, and the time constant for the cloud goes up accordingly. Instead of 10^7, say 10^8 seconds for an optical depth of 1 in the cloud.

So I think to the extent that a sulfuric acid cloud requires oxygen supplied by photolysis of CO_2, it becomes an exceedingly interesting and very complicated coupled dynamical-chemical problem. It's a very unusual kind of cloud by terrestrial standards, a cloud which is essentially deter-mined by short wavelength ultraviolet radiation, below 1700 Å, by and large. We're talking about 10^{-4} of the energy absorbed by the planet being respon-sible for the cloud, which, if you believe some models, is in turn respon-sible for the rather high surface temperatures by indirect processes, which in turn are responsible for the supply of HCl to the atmosphere, which in turn is required in order to regulate the supply of oxygen to make the cloud.

So it's a very, very complicated process, and you can also easily get involved in the chicken-and-egg question of whether the planet had to be hot in the beginning before you could get the system to really go. There is, I think, a whole series of very curious stability questions that are raised by this process.

One last remark, switching back up to the top of the planet. The ques-tion is: Where does the hydrogen come from? John Lewis would argue that Venus should form without water, without hydrogen. Jim Walker, I understand, is going to take issue with that later this afternoon.

DR. PRINN: I don't think John Lewis has said it is without water.

DR. MC ELROY: Well he has said quite strongly in his recent paper, that the hydrogen and the sulfur almost certainly are later additions due to fall-in from extraplanetary sources. His equilibrium models certainly do not have any significant vapor pressures of hydrogen.

Now there's another way in which you can get hydrogen, and the numbers turn out to be very good. I refer to accretion of hydrogen from the solar wind.

The total amount of hydrogen which is flowing toward Venus in the solar wind is about 10^8 atoms cm^{-2} sec^{-1}. So obviously if you had 100 percent capture efficiency you would - remember that thermal escape is exceedingly low - build up an enormous amount of hydrogen in the lower atmosphere in geologic time, a mixing ratio of 10^{-2} or 10^{-3}. These are very large source rates.

If the mixing ratio of water in the lower atmosphere is a few times 10^{-4}, which I tend to believe for other reasons, then the required hydrogen could be supplied with capture rates equal to about 10 percent of the supply.

I think it's almost impossible to escape having sources of that magnitude, irrespective of the details of the interaction of Venus with the solar wind. Because at very least there is a source of hydrogen due to charge transfer of solar wind protons with neutral atmospheric gases - oxygen, for example - which then turn the kilovolt proton into a kilovolt hydrogen atom which simply smashes into the planet.

Now, a theory that doesn't propose an observational test is not very interesting. This theory does propose an observational test, because the abundance of deuterium in the solar wind is less than 10^{-7} that of H due to nuclear burning in the sun. On the other hand, Cameron would argue that deuterium in cometary nuclei is very high, 10^{-3} or so. So measurement of the D/H ratio becomes, I think, an exceedingly important constraint on any study of the hydrogen evolution of the Venus atmosphere.

DR. HUNTEN: Perhaps it is worth mentioning another test that has already been applied to the earth [Junge et al., J. Geophys. Res. 67, 1027, 1962], and that is to look for neon which should come along with hydrogen.

DR. ANDY YOUNG: I'd like to know at what level in the atmosphere the H_2SO_4 is manufactured.

DR. MC ELROY: I really don't know. This is a calculation of what is going on somewhere above the clouds. What we're trying to do is see how fast oxygen can be produced and sent down to mate with the sulfur chemistry below.

Let me make a related point. There is a question as to how H_2SO_4 is made. It's easy to make it heterogeneously if you get to the stage of SO_2 or SO_3. It's very difficult to do it otherwise. From SO_3 it's easy: just add water heterogeneously and you make H_2SO_4. That apparently is the way people believe it happens in the earth's atmosphere.

But, I think it's very difficult to get to SO_3 on Venus. A more likely process on Venus is that you get to SO_2 and add peroxide, H_2O_2, which is quite abundant at these levels. In fact, one of the reasons the oxygen mixing ratio goes down - that the oxygen stops being mixed - is because it starts turning into H_2O_2. And to the extent that you can shield the H_2O_2 from ultraviolet dissociation you can build up quite significant amounts of it.

So if the H_2SO_4 is built with H_2O_2 it is a rather different beast. For example, Bruce Hapke and I talked on the way in this morning about the possibility of H_2O_2 in the solid phase being responsible for the UV absorption.

DR. HUNTEN: One way to build H_2O_2 is to wait for dark, for night.

DR. ROSSOW: Do you see any problem with keeping SO_2 if it is made deep in the atmosphere? I think Ron Prinn's model had COS made at the expense of SO_3 from the destruction of the H_2SO_4, but then SO_2 was left over, and that's what percolated back up.

DR. MC ELROY: I'm not sure that I understand, or have any real feeling for what the complete cycle is. I'll tell you the way I think it goes in simple terms.

I believe that sulfur is carried up as COS. The COS has to get into some hard ultraviolet sunlight before it begins to break up and release some sulfur. Then it goes through some complicated chemistry to make H_2SO_4 in the particulate phase. Then there is an equilibrium in which those particles are settling out. Now as they go down they're evaporating. I presume the first step is formation of things like SO_3. Now what happens to the SO_3? I would guess there's some oxidation of CO associated with that, SO_2 is formed, and that the whole process is not complete until it gets down to very high temperature levels near the surface.

So it's really a cycle which involves the atmosphere from the photo-chemical region above the clouds right down to the surface that you have to consider.

DR. ROSSOW: It seems to me crucial to determine if SO_2 can make it back from the bottom. If it can, then your proposal of hydrogen peroxide in the atmosphere might suggest that H_2SO_4 is easier to make than some people have suggested.

Another comment is that all your curves for densities of various things were flirting with the cloud tops. The maxima in most of the curves were almost within 10 km of where I think the cloud top is.

DR. MC ELROY: Well the peak oxygen production is about 25 km above where we think the cloud tops are. But you're absolutely right; it's close to a situation where you do have to worry about the clouds.

DR. ROSSOW: If you're within a couple of scale heights you're essen-tially right at the clouds.

DR. MC ELROY: I'm not sure what point you want to make about that. So what?

DR. ROSSOW: Well the first "so what?" is that I'm worried about any curve you draw that goes down into the cloud, because all those chemicals are going into solution in the droplets, there is chemistry going on in the droplets. But mostly I'm saying that anything that comes percolating up out of the clouds is right there were all the action is.

DR. MC ELROY: I think the second point is the serious one. I mean, there may be very good reasons to worry about a significant coupling of the sulfur chemistry and the oxygen chemistry at low levels. That is going to be rough to handle.

DR. POLLACK: I was a little bit confused by your ruling out the high flux cases for escape of hydrogen based on your argument that, as I under-stood it, the present content of water vapor had to last the entire history of the solar system. I would think that as long as the present content could be replenished, either through outgassing or cometary impact or some other mechanism, that whatever the escape time is, there would be no problem.

DR. MC ELROY: Let me try to go over the argument again. The first point is: What do the escape fluxes that appear on that slide mean? They are the net fluxes at the top of the atmosphere which are driving the chemical analysis all the way down. So they really must include the cometary source if it is deposited at high levels in the atmosphere, as well as the solar wind source, and take account of the thermal escape rate and any non-thermal escape. It's the net flow through some mythical boundary at

the top of the planet. Those curves are labeled by the net flux of hydrogen from the planet.

Now if there is also a supply at the bottom, then you're absolutely right, all bets are off, and I cannot make any statement. If the planet itself is outgassing at a rate of several times 10^7 or 10^8, then that particular constraint on the eddy coefficient disappears. So I am assuming that the source at the surface is not very large.

DR. POLLACK: Where is the boundary that you're talking about?

DR. MC ELROY: The critical level is at a height of about 210 km.

DR. POLLACK: Even in the case of cometary impact you can well imagine that most of the evaporation would occur lower in the atmosphere.

DR. MC ELROY: Yes, that evaporation may be at about 120 km, but the conclusion is the same and we are still stuck with the constraint. The key question is where the source is relative to the chemistry that's going on in the atmosphere, and the latter is lower down.

DR. HUNTEN: The other argument which I find even more convincing is that the densities of hydrogen atoms in the atmosphere are so small that you need mean velocities of thousands of centimeters per second to support limiting fluxes. And there is an embarrassingly small amount of H there. You need a horribly efficient mechanism per H atom to get such large fluxes.

DR. MC ELROY: Let me make a point which I forgot to make along the way: What is this extra component, the hot component, of hydrogen?

I think deuterium is not a viable option any more. We think that the most likely possibility is that hydrogen is produced by reaction of protons with oxygen, which then turns hot protons into hot hydrogen. And we have then an exospheric source of hydrogen atoms. So the reason you see a temperature of 1100 or 1200 K is that you're effectively seeing some mimic of the ion temperature.

Another comment: I think that a way of perhaps accounting for the extended envelope, which perhaps Joyce Penner could talk about, might be found by considering the non-thermal sources of oxygen and CO and their distribution around Venus.

If you have fast ion flows in the planet driven, for example, by the solar wind, you may expect to generate some fast neutrals, and the fast neutrals can then scatter sunlight. And that's one way perhaps of getting some luminosity outside which may account for this extended component, and it's also a way of having it depend on solar activity. And I should mention that the solar wind was blowing much stronger during Mariner 10 than it was during Mariner 5.

DR. WALKER: I don't understand why you feel the sulfur has to get reduced in the lower atmosphere. Why can't it just go down as H_2SO_4, evaporate, and come back up as H_2SO_4?

DR. PRINN: You would have to have an SO_3 mixing ratio in the lower atmosphere near 10^{-5}.

DR. WALKER: Is there any problem with that?

DR. PRINN: Yes, I will cover that in my talk.

DR. HARLAN SMITH: I'm a little surprised at the helium abundance, but

if you're going to make 10^{-4} mixing ratio for hydrogen from the solar wind is there any way you can avoid getting 10^{-5} for helium from the same source?

DR. MC ELROY: It's a good question. I think that's a real possibility. Again it depends on what the escape processes are, what kind of escape rate of helium you can get with the solar wind acting as a sink, the ionization of helium and stuff blowing around and then back into space. It's a real possibility that the helium is delivered by the sun also.

DR. ANDERSON: Just a comment on the comparison of Mariner 5 and Mariner 10. We [Mariner 5] didn't see any extended emission in essentially zero order channels. But the threshold of our sensitivity was between 60 and 100 Rayleighs in those two channels, and you [Mariner 10] probably had more sensitivity than that. We didn't see anything off the dark limb, and we didn't see anything on the dark disk.

We did see an emission off the dark limb amounting to about 300 Rayleighs between 1350 and 1700 Å. But that's the only thing we saw on the dark side from those essentially zero order channels. But I don't think that's necessarily in conflict with what you saw.

DR. MC ELROY: I'm not sure. I think we need careful comparisons to see whether there is or is not a difference in the intensities. My impression was that Mariner 5 would have seen this. We're talking about intensities of some of these things that are getting up, on the dark disk, above the 100 Rayleigh figure. We're talking about a total intensity in that bandpass which is probably several hundreds of Rayleighs. I think you would have seen it.

DR. STEWART: I'd like you to comment again on that megaRayleigh airglow. You mentioned that the Mariner 10 instrument was not a spectrometer. You mentioned also Moos's flights and Rottman's flights with a spectrometer to look at the full disk of Venus. Their spectrum covers that region to which you attribute the very large signal, 1300 to 1800 Å. And yet their total signal was maybe 40 kiloRayleighs, which is very much less than you're talking about. Is it not much more likely that there is indeed a scattered light problem causing the megaRayleigh signal?

DR. MC ELROY: Scattered light would make me feel a lot more comfortable. But, on the other hand, Broadfoot is certainly not prepared to buy that at the moment. He's convinced on instrumental grounds that it's not a likely explanation.

Let me point out that the comparison with Moos is a dangerous one to carry too far, because the intensities in the various channels do seem to be variable. For example, I think there's no way of escaping the conclusion that Lyman-alpha was brighter when Mariner 5 went there than it was for Mariner 10. I believe that's reasonably well established. The 1304 signal is also significantly different here as compared to Moos, about a factor of 2. And the 1480 channel is significantly brighter than that of Moos.

I should also say that the signal from the 1480 channel is stronger than that from the 1657 channel. I caution that this is highly preliminary stuff. But this would not be consistent with scattered light. That, I think, is where the real key will come. If that holds up then it looks as though it's not scattered light.

DR. STEWART: I grant you that. But you're talking about factors of 2 and 3 and not factors of 100.

DR. MC ELROY: Obviously it's going to have to be a factor of 100 or so brighter than it was when Moos made his observations. On energetic grounds it's not out of the question.

DR. STEWART: I understand that also. But also if you take your measured signals in the first order channels the contribution is nowhere near the average contribution which would be required over the whole spectral range by the 1 megaRayleigh result. That is to say, the emission spectrum would have to have holes in it at the first order channels. If you spread the megaRayleigh between 1300 and 1800 Å you get intensities larger than you see in the first order channels by factors of about 3.

DR. MC ELROY: It's a very serious problem. I think if we can find an excitation process that accounts for Moos's data I'll be willing to draw a fast conclusion about whether these data are spurious or not.

DR. BAUER: I'd just like to make a comment regarding the influx of protons and helium. I think it's rather unlikely that the solar wind would be a source for the atmospheric helium, simply because the abundance of helium in the solar wind is low. And, secondly, I think you don't have really any good processes for creating hot neutral helium which would come into the atmosphere. Isn't that correct?

DR. MC ELROY: Well, no. The solar wind proton flux at Venus is about 10^8. I was talking about something like 10 percent capture efficiency for hydrogen. The helium concentration is about 10 percent.

DR. JONES: 1 percent.

DR. MC ELROY: Okay. It's marginal.

DR. BAUER: Also I think the creation of neutral helium is doubtful. There are charge exchange processes. But I think it's not the same as a resonance process with O.

DR. MC ELROY: Resonance doesn't really make much difference at these energies. The cross-section is pretty well gas kinetic.

DR. JONES: Your helium abundance is based on a measurement over one particular region. You may have the problem of a non-uniform distribution of helium.

DR. MC ELROY: I should say that the helium number I'm quoting you is really based on a disk intensity and not yet on a detailed analysis of limb profile. If there is a spatial inhomogeneity we should be able to say something about that. We do have profiles for a fairly significant range of geographic coverage, because the UV instrument was aligned with the imaging system. So we do have data over quite a range of time.

SOME ASPECTS OF THE CHEMISTRY AND DYNAMICS OF THE UPPER ATMOSPHERE

Ronald Prinn, Massachusetts Institute of Technology

The presentation by Prinn is largely contained in his paper which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

Photochemical models for the Venus clouds are presented and discussed. We illustrate models for sulfuric acid density as a function of altitude based on a proposed photochemical scheme. Emphasis is placed on two competing removal mechanisms for sulfur atoms above the visible clouds:

$$S + O_2 \rightarrow SO + O,$$

$$S + COS \rightarrow S_2 + CO.$$
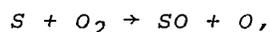
The first reaction (which forms the major oxygen sink in the visible cloud region) requires reasonable O_2 concentrations and leads to sulfuric acid production. The second reaction occurs in regions where O_2 is severely depleted and leads to elemental sulfur production. Quantitative estimates of the balance between these two competing processes are presented together with a discussion of the complete sulfur and oxygen cycles on the planet. We propose that the dark regions in the ultraviolet on Venus are oxygen-depleted regions where a significant amount of ultraviolet-absorbing sulfur is being produced. We also discuss observations of particle densities on Venus and their implications for vertical mixing rates. Transient internal gravity waves are a likely process for vertical mixing above the altitude $z \simeq 80$ km and we suggest that the vertical eddy-mixing coefficient is given by

$$K_z < 7 \times 10^4 \, exp[(z - 80)/2H]; \quad z \gtrsim 80 \text{ km.}$$

where H is the atmospheric density scale height. This suggests the turbopause should lie near or below 136 km. The dispersion relation for internal gravity waves in regions of wind shear suggests vertical mixing can be accomplished by transient thermally or mechanically-forced waves with horizontal wavelengths $\gtrsim 25$ km.

DR. HAPKE: At the last DPS meeting we showed some McDonald spectra and pointed out that elemental sulfur is a pretty good fit to the spectrum of Venus.

But, although it's a very good fit at the bottom part of the UV curve around 4000 Å, the spectrum of Venus begins to keel over too fast toward longer wavelengths for elemental sulfur to be a good match to it. So an additional absorber is still needed.

DR. PRINN: Well, I would like to see a multiple scattering model done, instead of just comparisons to a laboratory sample. In multiple scattering if the absorption coefficient is small you get more scatterings which may flatten out the curve.

DR. HAPKE: I've done a two-stream multiple scattering calculation, and it doesn't help. Maybe a more detailed calculation would change it, but I doubt it. Sulfur is just too bright at long wavelengths. [In a post-conference paper to appear in the special issue of the Journal of the Atmospheric Sciences (32, June 1975) Hapke and Nelson present evidence that incompletely polymerized sulfur can match the spectrum of Venus.]

DR. ROSSOW: For your upper limits on the eddy diffusivity, how did you derive the value for the cloud scale height?

DR. PRINN: This was determined before Mariner 10. I had three levels from which I determined the particle scale height. I used Goody's critical refraction level which corresponds to an optical depth of unity looking tangentially across the planet, I used Jim Hansen's 50 mb level, and then the line formation level below that.

DR. ANDY YOUNG: There are some chemical problems with elemental sulfur. The fluosulfonic acid I mentioned earlier, which is necessarily present in the droplets if they're made of sulfuric acid, because we know HF is present, attacks elemental sulfur and produces SO_2. I don't know the rate of this reaction, but presumably it's fairly rapid. And I think there are difficulties in maintaining elemental sulfur up to the level where we can see it.

Furthermore HSO_3F vapor is stable right down to the surface of the planet as far as breaking up is concerned. It is stable to something like 900 C. And that should be put into the equilibrium chemistry. I think there's a whole spectrum of sulfur/oxygen/halogen compounds that ought to be put into that equilibrium chemistry to make it believable at the surface, even if equilibrium is the situation.

DR. PRINN: I have a feeling HSO_3F will find some more thermodynamically stable form to be in at the surface than HSO_3F.

DR. YOUNG: But it ought to be included in the chemistry, it seems to me.

DR. PRINN: You mean included in John Lewis' chemistry of the surface?

DR. YOUNG: Yes.

DR. PRINN: Well, he took the most stable forms of those various compounds. And, of course, that's the thing they're going to end up in in thermochemical equilibrium. So there's no point in taking less stable substances, and putting them in the computation.

DR. MC ELROY: I do think Andy is raising a very important point. The atmosphere near the cloud level is clearly not in equilibrium. It is disturbed by sunlight.

DR. PRINN: No; I'm saying equilibrium at the surface.

DR. MC ELROY: But the question then is, what is the time constant for the thermodynamic equilibrium compared to the vertical transport time. If the time is short the entire atmosphere may be in equilibrium with the sun rather than the surface. The sun has an effective temperature of 5000 K at the relevant UV wavelengths.

DR. GREYBER: Why can't the elemental sulfur result from outgassing on the surface, instead of from cometary influx.

DR. PRINN: I only said that if Venus did not have any primordial sulfur, in other words if it was accreted without any sulfur, it would be very easy to saturate the atmosphere with sulfur from cometary material. These are very small mixing ratios for sulfur compounds in the atmosphere.

MARINER 10 RADIO OCCULTATION MEASUREMENTS OF THE IONOSPHERE

Gunnar Fjeldbo, Jet Propulsion Laboratory

The presentation by Fjeldbo is largely contained in the paper by Fjeldbo et al. which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

Data from the Mariner 10 radio occultation experiment have been utilized to determine the vertical electron density distribution in the ionosphere of Venus. The ingress measurements, which were made at 1.3° North latitude on the nightside of the planet, show two distinct layers. The main layer was located at 142 km altitude and had a peak density of 9×10^3 el/cm^3. A secondary layer with a peak density of 7×10^3 el/cm^3 was detected at 124 km altitude. During egress, the ionosphere was probed at 56.0° South latitude on the dayside of Venus. The solar zenith angle in this region was 67.0°. The dayside ionosphere consisted of a main layer with a peak density of 2.9×10^5 el/cm^3 at 142 km altitude and several minor layers. At the top of the dayside ionosphere, the measurements showed an abrupt drop in the density from 2000 el/cm^3 at 335 km altitude to below the level of detectability, i.e., less than 200 el/cm^3, at 360 km altitude. This abrupt density change may be the ionopause where the solar wind plasma interacts with the ionized components of the atmosphere.

DR. BAUER: In your figure in the altitude range between 250 and 350 km there is quite wide variation in the density. If the solar wind actually scavenges some of the ions, and since you measured the time variation of the integrated content, could this not be an indication of flowing plasma?

DR. FJELDBO: Yes, it could be an indication of turbulence in that region.

DR. CLARKE: Did you make a comparison with the Mariner 5 S-band data and the Mariner 10 data?

DR. FJELDBO: We didn't see the ionopause boundary in the Mariner 5 S-band data because of the limited oscillator stability. So we used the Mariner 5 differential dispersive doppler data when we compared the ionopause measurements from the two missions. In the lower portion of the dayside ionosphere the Mariner 10 electron density profile was compared with the Mariner 5 S-band data.

DR. STEWART: In obtaining the number density profiles from your line of sight data do you assume spherical symmetry?

DR. FJELDBO: Yes, in this case we assumed spherical symmetry.

MARINER 5 UV OBSERVATIONS

Donald Anderson, Naval Research Laboratory

The presentation by Anderson is largely contained in two papers by Anderson. The abstract of one of the papers, which has been accepted for publication in the Journal of Geophysical Research, follows:

Airglow measurements of the disk of Venus, made by the ultraviolet photometer on Mariner 5 on 19 October 1967, are analyzed to determine the sources of the observed emission. Rayleigh scattering models for a semi-infinite atmosphere are used to determine the scale height and single scattering albedo of the scatterer, and to determine the 1304 Å emission rate. It is found that: (1) the scale height of the Rayleigh scattered radiation is 4.5 ± 0.5 km; (2) the single scattering albedo near 2000 Å is 0.8 ± 0.1; and (3) 500 R of 1304 Å radiation is detected at solar zenith angles between 90 and 95°.

The abstract of the other paper, which has been submitted to the same journal, is:

Lyman-α measurements of the exosphere of Venus, made by the ultraviolet photometer on Mariner 5 on 19 October 1967, are analyzed. Radiative transfer models for a spherical isothermal hydrogen atmosphere, with carbon dioxide present as a pure absorber, are used to determine the exospheric temperature and density at the bright limb, and on the dark disk. It is found that: (1) the bright limb data have two components with exospheric temperatures of 275 ± 50°K and 1020 ± 100°K and densities 2 ± 1 × 10^5 cm^{-3} and 1.3 × 10^3 cm^{-3}, respectively; (2) the dark disk data are best fit by a two-component density model with exospheric temperatures of 150 ± 50°K and 1500 ± 200°K and densities 2 ± 1 × 10^5 cm^{-3} and 10^3 cm^{-3}, respectively; (3) the dark limb exhibits only a hot component because of the very low temperature of the cold component.

A MODEL OF THE VENUS IONOSPHERE

Thomas Donahue, University of Michigan

The presentation by Donahue is largely contained in the paper by Nagy et al. which appeared in Geophysical Research Letters [2, March 1975]. The abstract of that paper follows:

Results of model calculations of the Venus ionosphere covering the altitude range from 120 km to 300 km are presented. The chemical scheme and the reaction rates adopted for the model are the same as given in a recent paper by Kumar and Hunten [1974], except that the electron temperature dependence of the dissociative recombination rates is taken into account. The calculations were carried out for 'low' and 'high' atomic oxygen models, corresponding to an $[O]/[CO_2]$ ratio of 0.4 percent and 4 percent respectively at 140 km. The results of the calculations agree well in both shape and magnitude with the Mariner 5 and 10 occultation results in the chemically controlled region; at the higher altitudes reasonable agreement with the observation is obtained if diffusive equilibrium and high vertical flow velocities (10 km/sec) are assumed as upper boundaries for the Mariner 5 and 10 conditions, respectively, although solar wind-ionosphere interactions are the likely controlling mechanism for the Mariner 10 case.

DR. BAUER: I'd certainly agree that sweeping away by the solar wind can play some role, and that the velocities you get at the top are about 15 km sec^{-1}. It is easy to show in a very simplistic kind of model that from momentum transfer of the solar wind to the ionosphere plasma you could get velocities of the order of 10 km sec^{-1}.

What bothers me about this mechanism is that you have to invoke a very large upward flux, which is really the maximum diffusive flux.

DR. DONAHUE: It isn't quite that large. The upward flux for the low atomic oxygen density case is 2×10^9 cm^{-2} sec^{-1}. The limiting flux is 2.5 $\times 10^9$. It's close.

DR. BAUER: This implies a tremendous loss of oxygen by solar wind scavenging.

DR. HUNTEN: But the flux is a couple of orders of magnitude larger than the Michel limit [Michel, F. C., Planet. Space Sci. 19, 1580, 1971] which is a fraction of the solar wind number flux.

DR. DONAHUE: That's for a planet-wide loss. If you were to interpret this in terms of planet-wide loss you would exceed the Michel limit. This has to be a local flux that comes back someplace else.

DR. JONES: I notice that while you can get slopes which roughly agree with Mariner data you haven't really succeeded in explaining any of the ledges in the profile.

DR. DONAHUE: The top ledge is in agreement.

DR. JONES: In a way, yes.

There is some indication from Dr. Fjeldbo's data for the uppermost

part which we had interpreted as possibly due to helium ions. That would cause the same kind of problem, that you would really sweep away all the helium.

DR. DONAHUE: Obviously one difficulty is the one-dimensional calculation. The two-dimensional calculation certainly has to be done. This calculation is only representative of a suggestion to stimulate a look at another possible solution. And, in fact, we haven't even looked at the possibility that solar-wind pressure on this sort of ionosphere would produce the observed profile.

DR. MC ELROY: I don't believe your model, Tom. Because I think you can take your own numbers and show that you run into an impossible contradiction. And it surprises me that you didn't find it. You're doing a one-dimensional treatment of a problem which is basically two-dimensional, as you pointed out yourself. Let's consider what really happens in the two-dimensional case.

In the two-dimensional case you would have ionization over some horizontal scale. In the one-dimensional case you're doing it over the vertical scale. So as every dynamic meteorologist knows, to first order you increase the velocity for the horizontal flow, and the aspect ratio is the ratio of scale height to planetary radius. And this can't be very far off actually. That number is 3000 km sec^{-1}, which is higher than the velocity of the solar wind. So I don't think your model would work.

Also I'm surprised that Bauer didn't defend himself a little better than he did. Because I think his model is still basically right, or has a good chance of being right. As I understand his model, it is not crucial that there be large amounts of atomic oxygen. There could equally well be faster velocities, downward transport.

DR. BAUER: There is one problem, though, with the larger downward velocities. I think it's quite easy to show that it could be one order of magnitude higher, that is of the order of kilometers per second. But I believe that you then run into some difficulty, if you consider that the lower part of the ionosphere observed is photochemically controlled. If you're willing to invoke compression down to the F_1 maximum, then I think that that would be an alternative.

But the real problem we found is that in fact we need an O/CO_2 ratio at the F_2 ledge, as we call it, which is somewhat higher. And that is not allowed currently by any of the eddy diffusion coefficients which I think are considered to be likely today. On the other hand I feel there is definitely a good chance that downward momentum from the solar wind will affect the distribution one way or another.

So I really see the only difficulty in our model as being the atomic oxygen concentration. And despite what I've heard today I'm not so certain that the last results are in yet. Because when Dr. McElroy argued about the CO and O composition it sounded to me almost like a circular argument. Can you assume that the oxygen has to be low, when you also have to question some of the end results about the CO?

DR. HUNTEN: The basic evidence comes from the hydrogen.

DR. DONAHUE: In response to McElroy: I thought I covered the second point, namely that one really must look at what pressure will do.

But in response to the first point, of course we recognize that problem, and I thought I mentioned it. It is not necessarily a consequence that when you do the two-dimensional calculation, including flow, that you

won't get the same kind of slope that we found in the one-dimensional case. You may end up with a vertical velocity at the top which is reasonable, and a horizontal velocity which is reasonable. That's what we're hoping turns out when we do the two-dimensional calculation. It's possible that the two-dimensional calculation will still allow us to have the big slope without implying such large velocities.

DR. MC ELROY: Your model, it seems to me, is very promising at localities. The idea that you can get structure by blaming it on the electron temperature is interesting. I think that a combination of that with Bauer's model on top can work.

I agree also that it's not at all clear that the flow is not hot at high altitudes. You may well be right on that, at 70 degrees solar zenith angle. It's marginal. That's exactly where Curt Michel was putting his division points. It's really the divergence of velocity that is important, not the velocity itself. And it isn't clear how that would go.

Let me ask a question about the electron temperature. It seems to me you have a temperature which is variable, from 3000° to 300° on a scale of 10 km.

DR. DONAHUE: Thirty km.

DR. MC ELROY: The conductivity goes roughly as $T_e^{5/2}$. It looks like I have enough energy to make my MegaRayleigh airglow if I can find the mechanism. It seems to me there's a very large energy flux associated with that great a magnitude.

DR. DONAHUE: Well clearly we haven't done the electron temperature calculation. We took the quick way out, we took the old Bauer and Hartle [Geophys. Res. Lett. 1, 7, 1974] calculation and just got a rough slope from their figure. That didn't seem unreasonable on this one-inch-by-one-inch type of figure.

The spirit of this paper is to say: This is a suggested way out of the problem when you try to cope with a low atomic oxygen density. The problems you point out certainly exist and need to be handled.

SESSION 6: THE EVOLUTION OF THE ATMOSPHERE

DR. RASOOL: This last session is very close to my heart because the last scientific paper I did before I went to Washington was one on the evolution of the atmosphere of Venus. Actually, I still don't know what the answer is, and that's why I'm very excited about listening to what Jim Walker has to say.

One must realize that the atmosphere of a planet has only two ways to go, into the crust and into space. In between the upper boundaries which McElroy was talking about earlier today, and the lower boundaries, which must have been discussed earlier this week, many things are happening. But knowing the processes and rates at the upper and the lower boundaries and knowing something about meteorology and transport processes, and by looking at a number of planets, we can really make a good story of the evolution, not only of Venus, Mars and the other planets, but of earth itself. And that's what is so important about this discussion.

EVOLUTION OF THE ATMOSPHERE

James Walker, Arecibo Observatory

I doubt that the story is going to be as complete as Ichtiaque would like. At any rate the main question about the evolution of the atmosphere of Venus arises from the fact that Venus is similar to the earth in size, density and location in the solar system. And so the question is what went wrong on Venus, and the problems are the high temperature on Venus and the high surface pressure compared with that of the earth.

Now the high temperature is, I think, clearly a result of the high surface pressure. Whether the high temperature on Venus is the result of the general circulation of the atmosphere or the result of the greenhouse effect is not essential. The atmosphere, in order to preserve that high surface temperature, must have a very large infrared opacity, and thus a very massive atmosphere.

So the question is: what causes the massive atmosphere? There seem to be two rather contradictory theories.

One theory is that the surface pressure on Venus is high because gases react very rapidly with surface rock and equilibrium has been achieved. With the high temperatures on the surface of Venus, this equilibrium leads to high surface pressures. That's the chemical equilibrium theory associated mostly with the names of Mueller and Lewis.

The other possibility is that Venus has a very massive atmosphere because the gases don't react with the rocks, and everything that has ever been released from Venus is still in the atmosphere. I'll come to the question of why the gases don't react with the rocks in due course.

Under the assumption that everything that is degassed is still there, there is an interesting comparison that one can make with the amount of CO_2 in the atmosphere of Venus, which is something like 5.3×10^{23} gm for a surface pressure of 100 bars, and the terrestrial number, which is about 5.1×10^{23} gm. I believe this comparison was first made by Carl Sagan in about 1962 when the numbers were very different from what they are now. Oddly enough, Sagan found agreement between the two numbers, although they were both smaller by an order of magnitude. Both the numbers have since gone up, but they still agree. Venus' CO_2 is in the atmosphere while on the earth CO_2 is in the crust, mostly in the form of carbonate minerals.

I don't think that this is evidence that Venus and earth have the same amount of carbon or the same amount of carbon dioxide. One reason why this is probably no more than an interesting comparison, and not a particularly informative one, is that if the chemical equilibrium theory for the Venus atmosphere is correct, there is carbon dioxide in the rocks on Venus as well as in the atmosphere, so the first number should be larger. In addition, there is evidence for carbon dioxide or carbon in the mantle of the earth, so the second number should be larger, too. Both of the budgets are incomplete.

Let me discuss the chemical equilibrium theory briefly. The idea here is: the surface temperature on Venus is high; the partial pressures of the

volatiles are high; and the atmosphere is in chemical equilibrium at the high temperatures. On earth, where the surface temperature is low, CO_2 tends to reside in the solid phase as carbonate minerals, and on Venus, where the surface temperature is high, CO_2 tends to reside in the atmosphere.

Now, the terrestrial atmosphere is very far from chemical equilibrium. We don't have anything approaching chemical equilibrium in the earth's atmosphere for several reasons. One is that the rates at which gases react with rock are low at the low surface temperatures of the earth. A second reason is that there are very rapid disequilibrating processes, most of them associated with biological activity on the earth. And a third reason why some of the constituents of the earth's atmosphere are not in chemical equilibrium is that photochemistry drives them away from chemical equilibrium.

On Venus the kinetics of the reactions between rocks, minerals, and atmospheric gases, are much faster; the idea of the chemical equilibrium theory is that these rates are so fast that perhaps the atmosphere has come into equilibrium with the rocks. The point of describing some of the disequilibrating processes on the earth is that before we can decide whether chemical equilibrium can exist on Venus, we have to consider the rates of some of the disequilibrating processes. It is not enough to say that gases react rapidly with rocks; you also have to say that the gases react more rapidly with the rocks than with anything else.

Let's consider now some of the disequilibrating processes. Biology presumably is not a factor. Photochemistry is plainly a significant disequilibrating process. McElroy and his co-workers have, over the years, presented a number of theories to explain the carbon monoxide and oxygen concentrations of the Venus atmosphere photochemically, and they've been successful.

Now either the carbon monoxide and the oxygen concentrations of Venus are controlled by photochemical processes or they are controlled by chemical equilibrium with the rocks. The chances that they are controlled by both are small. I think the chances are good that the carbon monoxide and the oxygen content in the Venus atmosphere are controlled by photochemical equilibrium and not by chemical equilibrium between the atmosphere and the rocks.

DR. RASOOL: Will you explain why you think these gas amounts are not controlled by the chemistry of the surface?

DR. WALKER: Because McElroy has been so successful in explaining their abundance photochemically.

DR. PRINN: Well, that's the visible atmosphere. That doesn't tell you what it is down at the bottom.

DR. WALKER: I agree it may well vary with altitude.

DR. PRINN: I think it is pretty obvious that it does.

DR. WALKER: Below the clouds, yes. Since Lewis also succeeds in explaining the observed carbon monoxide and oxygen concentrations in the atmosphere, that suggests either that those mixing ratios do not vary with altitude or else that there's a fortuitous coincidence.

DR. PRINN: Lewis essentially doesn't have any oxygen on the surface while McElroy has a mixing ratio of 10^{-7}. Until oxygen is actually measured

neither Lewis' nor McElroy's concepts can be said to explain oxygen.

DR. WALKER: The point I want to make is that everything we know about carbon monoxide and oxygen in the atmosphere of Venus is explained by photochemical processes. It is possible that the lower atmosphere is insulated from the upper atmosphere, and reactions with the rocks control those concentrations in the lower atmosphere. It is also possible that both photochemistry and reactions with the rocks lead to the same abundances. That last possibility, I think, is unlikely.

DR. JONES: What rock reactions would produce carbon monoxide?

DR. WALKER: I don't think any would. I think the carbon monoxide is controlled photochemically. What I'm saying is that I think we can eliminate those two gases as candidates for chemical equilibrium with the surface.

Let me continue on the subject of disequilibrating processes. There is a question of whether the water vapor amount in the atmosphere is disequilibrated as a result of the photolysis of water vapor and the escape of hydrogen. That process appears to be negligible. The upper limit on the loss of hydrogen from Venus may be 10^6 cm^{-2} sec^{-1}; or it may be 10^7 cm^{-2} sec^{-1}, depending on how many non-thermal escape mechanisms you want to consider. At those rates, it would take something like 400 billion years to dissipate the amount of water presently in the atmosphere of Venus. So photolysis of water and hydrogen escape is not a significant disequilibrating process at present in the atmosphere of Venus.

What about volcanism, the release of gases by volcanoes, as a possible disequilibrating process? One point to note is that volcanic gases are disequilibrating. They are in equilibrium with the rocks, but that equilibrium is achieved at some depth within the planet where pressures and temperatures are higher than they are at the surface. When the gases get to the surface they are not in equilibrium even with the rocks from which they are derived.

Volcanic gases on the earth are part of what we might call the rock cycle, and briefly, the rock cycle goes like this: Igneous rocks react with atmospheric gases to give weathered sedimentary rocks. Sedimentary rocks are buried beneath other sedimentary rocks, carried down to depths within the earth, and heated up, whereupon the volatiles are driven off to return to the atmosphere and new igneous rocks are made. That's a brief summary of a complicated set of processes.

The question is whether there is a similar kind of rock cycle on Venus. If there isn't some such process, it is quite possible that there are no disequilibrating volcanic gases being released on Venus.

Now a key element of the rock cycle on the earth is weathering and erosion. It is not enough to have the gases react with the igneous rock; the debris have to be removed to some other place like the floor of the ocean so that fresh rock is exposed at the surface to react with the gases. So this question of transport is the key one.

On Venus there undoubtedly is weathering, at least in principle. Atmospheric gases will react with fresh rock that is exposed at the surface. But there is a real question as to whether there is any transport, any way of moving the weathered rocks away from the place where they are formed and repeatedly renewing the surface so that gases and rocks can continue to react.

Wind velocities seem to be too low at the surface to result in a significant transport of surface materials. Yesterday we were talking about raising grains of dust with radii of perhaps 10 μm, and that is just not going to be significant in the transport process. It's not a significant way of freshening up the rocks.

If the rocks aren't freshened up, they will become buried. Even if weathering occurs, the rocks after a while will become buried with debris, with weathered, old, tired rocks, and the reaction between gases and rocks will stop.

DR. MC ELROY: Is rock transport by plates, by large tectonic plates, significant?

DR. WALKER: That's part of what I call the rock cycle, but it is not part of what I'm talking about right now. What I'm talking about right now is what is done by running water on earth. The rocks are weathered and then scraped clean. On earth, wind plays a role, but running water is more important.

DR. RASOOL: You said that the reaction would stop after a certain amount of rock had been weathered. What is that depth, a centimeter, a millimeter?

DR. WALKER: Some finite depth, but I don't know what it is.

DR. MC ELROY: I think tectonic transport is certainly also important.

DR. WALKER: There's a difference. Tectonic transport does not scrape the surface clean. It will take the sediments back down and then metamorphose those and yield new volcanic rock.

DR. MC ELROY: That's exactly what I'm saying; that's exposing new stuff in the volcanic rock.

DR. WALKER: I will get to that.

My suggestion for a model of the rock cycle on Venus is just that. Rocks are weathered, there are volcanoes with lava flows which cover the old weathered stuff; and this provides fresh igneous rocks to attack and weather. So it is possible that there is a kind of a rock cycle on Venus, even in the absence of erosion and transport of weathered material. That's hypothetical. We don't know whether it occurs, but it is a possibility.

Another very remote possibility is that you could have a process of gravitational wasting, in which the rocks weather and break down, and then the debris just rolls down the side of the mountain, leaving fresh rocks exposed on the mountain top to renewed attack by atmospheric gases. I think that's a bit of a long shot, but it is the only alternative that has occurred to me.

Now the fact that Venus has fairly marked topography indicates either that there is no erosion occurring on Venus or that there is tectonic activity on Venus. If you have this process of gravitational wasting, in the absence of tectonic activity the mountains would in due course be worn down and would have disappeared.

DR. JONES: What about meteoritic impact as a means of exposing new rocks to weathering?

DR. MC ELROY: The atmosphere is a pretty good shield.

DR. WALKER: I understand that there are things that look like craters on the surface of Venus.

DR. RASOOL: They are kilometer-sized objects.

DR. POLLACK: The atmosphere of Venus is very effective in slowing down meteoroids whose size is less than about a kilometer. Since there are relatively few meteoroids above this limit, meteoritic erosion may not be very important for Venus.

Let's consider a range of possibilities concerning the level of tectonic activity on Venus.

One possibility is that there is no tectonic activity. That's a good thing for the chemical equilibrium theory, because volcanic gases are disequilibrating. However, because there is topography on Venus, if there is no tectonic activity there must also be no erosion, and having no erosion is bad for the chemical equilibrium theory because in that case fresh rocks are not exposed at the surface. Weathering can only reach a finite depth and the gases can react with the rocks only down to a finite depth in this situation. That means that there is a limited supply of reactive minerals for atmospheric gases to react with. So the initial supply of some gases will have exceeded the initial supply of reactive minerals. Those gases will have reacted with the minerals as much as they can, and the gas left in the atmosphere will be what we see today. For those gases, chemical equilibrium will not exist. They are just a remnant after the weathering has gone as deeply as it can. For some other gases, there might have been a larger supply of reactive minerals than of initial gas, and those gases may have come to chemical equilibrium. It is a situation in which there can be some gases in chemical equilibrium and some not, depending on the supply of reactive minerals at the surface of Venus.

Another possibility is that the tectonic activity provides enough fresh rock, by means of lava flows or some such mechanism, that it provides a substantial amount of fresh unweathered rock at the surface. It also provides volcanic gases, but the supply of fresh rock may be sufficiently large and the supply of volcanic gases sufficiently small that the gases can come to equilibrium. Or just some of the gases might come to equilibrium.

A third possibility is that Venus is tectonically active but the rate of supply of volcanic gases is large and the rate of supply of fresh unweathered rock is small, so that the volcanoes supply gases to the atmosphere too rapidly to permit the gases to react with the rather limited supply of unweathered rock. In that case there is not chemical equilibrium.

The point of all of this is to show that the fact that reactions between gases and rocks are probably rapid at Venus' temperatures does not imply that the atmosphere of Venus is in chemical equilibrium with the surface. It is essential to consider these questions of supply, to consider the disequilibrating processes such as the release of volcanic gases, and to consider the supply of fresh unweathered rocks that the atmosphere can react with.

The next point to consider about the chemical equilibrium theory is that for the gases that do achieve chemical equilibrium, if any, the partial pressure will depend on the mineral assemblage for which the chemical equilibrium is achieved and on the temperature. The temperature will vary on the surface of Venus, not much with latitude nor with time of day, but with

height of the mountains. And I would hypothesize that if chemical equilibrium is achieved, it is achieved somewhere between the temperature of the mountain tops and the temperature of the valleys. The mountain tops are heavily weathered where the temperatures are low and the equilibrium partial pressures would be low, and the valley bottoms are unweathered because their temperatures are higher. That's a pretty speculative suggestion.

Now the mere fact that for most of the known gases in the Venus atmosphere there are reactions that could, at the temperature of Venus, maintain the partial pressure in equilibrium, does not imply that the atmosphere of Venus is in chemical equilibrium at the surface. It's essential that plausible minerals be involved in this chemical equilibrium, minerals that are likely to be present at the surface of Venus. This is a topic that is well considered by petrologists and people like that, and Phil Orville is going to talk about this question later on this afternoon. I only mention that the buffer reactions have got to involve plausible minerals.

What the subject requires, I think, before we can really decide which gases in the atmosphere of Venus are in chemical equilibrium with the rocks, is information about erosion processes and tectonic activity on Venus, and information about the nature of the surface of Venus, the rocks that are there. In the meantime, the study we could conduct to help to answer this question is to assume a rock type for the surface of Venus, a plausible rock type, say basalt and weathering products, and, with the mineral assemblage specified, find out which gases could be in equilibrium with that plausible rock type and which gases are plainly not in equilibrium with that plausible rock type.

It seems to me if such a study were conducted, we would find that some of the gases are in equilibrium and some of them are present in excess. If the reaction rates on the surface of Venus are very rapid, I don't see how any gas could be present in less than an equilibrium amount over minerals that really exist on the surface of Venus, because the minerals would react to drive the partial pressure up. If a gas is present in excess of an equilibrium value, that means either that there is no fresh unweathered rock exposed at the surface of Venus or it means that the rate of release of disequilibrating volcanic gas is very high. So we won't get any definitive answer from a study such as this, but we will learn something about the options.

In the meantime, I conclude that it is not established that the atmosphere of Venus is in chemical equilibrium with the surface.

Getting back to the question of why Venus has a massive atmosphere, there is the suggestion that it is the consequence of chemical equilibrium at the high surface temperature of Venus. The other possibility, which I have been trying to illustrate in the preceding discussion, is that the atmosphere may be massive because the gases don't react with the surface of Venus; they don't react because the surface of Venus is not scraped to expose fresh rock; and they don't react because running water is absent on the surface of Venus. So it may be that Venus has a massive atmosphere simply because all the significant material that has been degassed is still in the atmosphere.

The absence of water, then, is an important question. And interestingly enough, the absence of water may imply that atmospheric gases react rapidly with the surface rocks on Venus. I want to try to show why it may be hard to get a dry Venus without rapid reactions between gas and fresh rocks on the surface of Venus. So let's take a look at this question of the absence of water on Venus.

First of all, if I assume that there is a half a percent of water in

the lower atmosphere of Venus, I get 1.1×10^{21} gm of water in the atmosphere. For comparison, in the oceans on the earth, there is 1.35×10^{24} gm.

So Venus has very much less water than the earth. I should also mention that there is not very much water in the crust of the earth compared to the amount in the oceans. I assume that there will be even less water in the crust of Venus because higher temperatures will tend to drive the water out of hydrated minerals. So I feel that this is about all the water on Venus.

So Venus is deficient in water with respect to the earth. One possibility, of course, is that Venus never had much water, that it was made dry. I don't think that's a tenable possibility for the following reason, which is an argument due to Turekian.

The point is that you must get carbon into Venus, and the way to get carbon into Venus is in the form of hydrocarbons. Hydrocarbons are what you find in meteorites; not elemental carbons, not oxidized carbons, but hydrocarbons. Hydrocarbons are also what you would expect to have condensed in the primitive solar nebulae. Lewis' calculations, for example, show that the carbon condenses out as methane or combinations of methane and water and things like that.

So we've got to get carbon into Venus, enough carbon to make the amount of CO_2 in the atmosphere, and it would be in the form of a hydrocarbon, which I will schematically call CH_2. This would be buried and react with some oxidized mineral in Venus which, for purpose of illustration, I will call Fe_3O_4; the hydrocarbon would be oxidized to produce carbon dioxide and water and a more reduced mineral on the surface of Venus. This would not be a surface reaction, but somewhere in the interior of Venus. And the point is that for every carbon dioxide you get out of it you also get water.

$$CH_2 + 3Fe_3O_4 \rightarrow CO_2 + H_2O + 9FeO$$
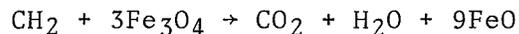
DR. MC ELROY: I wasn't aware that Lewis condensed any carbon on Venus.

DR. WALKER: My observation was simply that the kind of carbon that you do condense in the primitive solar nebulae is hydrocarbon

DR. RASOOL: Is this happening before the planet formed, or just about that time, or afterwards?

DR. WALKER: I don't think that's germane. The point is that if you want to get carbon onto a planet, given the universe we live in, the way you're going to get it in is as hydrocarbon, whether you get it in before or after, or any other time.

DR. RASOOL: Well, the carbon on earth is deficient by a factor of 10^4.

DR. PRINN: The way Lewis puts the carbon on a planet is to dissolve it in iron, if I recall correctly. I don't see why you need that reaction to retain carbon as hydrocarbons. Carbon does dissolve in iron and you can work out the extent to which it does, and you can get plenty of carbon just by that method alone.

DR. WALKER: And is carbon dissolved in iron a dominant constituent of things which we have in the solar system today, such as meteorites?

DR. JONES: I think that much of the native carbon that didn't come in meteorites and comets is present in the carbides.

DR. WALKER: Where is this?

DR. JONES: As a carbide, an iron carbide, an iron-nickel carbide.

DR. WALKER: Where? On the moon?

DR. SAGAN: About one percent of the meteorites are cabonaceous chondrites. Carbonaceous chondrites are several percent organic carbon. So to the extent that the asteroid belt is typical of the inner solar system, it is clear that the form of carbon is organic compounds.

DR. JONES: But according to Lewis, the asteroid belt is not typical of the inner solar system and I thought we were talking about Lewis' model.

DR. WALKER: I was not considering Lewis' model. I was talking about Turekian's argument. I said Lewis agrees with everybody else in condensing carbon in the form of hydrocarbons.

Thanks, Carl.

If you take this as a lower limit on the amount of water at one time in the atmosphere of Venus, you get 2.2×10^{23} gm, which is not as much as we have on the earth but is more than we have on Venus right now. And of course this is, in terms of this argument, a lower limit because there may be more H combined with the C, there may be more carbon dioxide on Venus than we know about in the atmosphere, and, in addition, there may have been water of hydration or other forms, ice, or something like that. This may not be the only way to get water into the atmosphere of Venus.

Of course it may not be the only way to get carbon in the atmosphere of Venus either. I think that is the point of the criticisms from the audience.

At any rate, I am going to accept the contention that Venus at one time had a lot more water and that it has lost a lot of water, and consider how this can have happened. This brings me to really the key element of Venus' evolution, and that is the runaway greenhouse effect, proposed, I believe, by Ingersoll. In essence, the greenhouse argument is that water would never condense on Venus, no matter how much water there was.

The idea is simply this: As you imagine increasing the amount of water in the atmosphere of Venus, the greenhouse effect causes the surface temperature to increase, and it increases more rapidly than the temperature at which water vapor would condense.

There is in fact, as Ingersoll has shown, a critical value for the solar flux. If the solar flux is less than this critical value, water will condense, as on the earth and on Mars. And if the solar flux is above this critical value, water vapor will not condense, no matter how much you have. And presumably, Venus is in the second class, the class where the solar flux exceeds the critical value.

DR. POLLACK: Are you taking into account the paper I wrote in which I point out that the solar luminosity was less when Venus was formed? This means Venus was in a stable rather than a runaway situation in its early history, and so may have had a moderate surface temperature at that time.

DR. WALKER: I do have a comment on that in my manuscript. My comment is that if we could accurately evaluate the runaway greenhouse effect, we might be able to set a limit on how much the solar luminosity has increased over the age of the solar system.

I was not aware of your paper but I doubt whether you would be able to

pin down the increase in solar luminosity with the accuracy that would be required to establish that the runaway greenhouse effect is impossible. The models of solar evolution are not that reliable.

DR. SAGAN: The models of solar evolution vary by a factor of two in delta L over L.

DR. WALKER: Provided you ignore possibilities like a very rapidly rotating core and freakish things like that. My feeling is that how much the solar luminosity has increased is one of the things we would like to know. It is just about as clear that Venus has had a runaway greenhouse effect as it is that the solar luminosity has increased significantly.

DR. SAGAN: No, no, no. I would certainly argue that. There is a whole theory of solar evolution which explains the Hertzsprung-Russell diagram and which is much more reliably understood than anything about the greenhouse effect.

DR. WALKER: I think the increase in solar luminosity is a nice point. I, too, stimulated by your work on the subject, have explored the matter with astrophysicists, and I haven't been able to convince myself that we know exactly how much the solar luminosity has increased.

I personally think that if you want to get from 10^{23} or 10^{24} gm of water to 10^{21} gm of water, it is very hard to do it without a runaway greenhouse effect on Venus. That's another way of saying what I want to say. I feel that the runaway greenhouse effect is the key historical event on Venus.

I want to consider now an instantaneous degassing model for Venus which assumes that all of the stuff in the atmosphere of Venus came out essentially at one time, when the planet was formed, when it accreted, or when the core segregated. Venus started out with a large initial atmosphere, and I want to consider what happened to this atmosphere.

For the sake of being specific, I'll assume that this initial atmosphere had 5.3×10^{23} gm of carbon dioxide and 1.35×10^{24} gm of water, one terrestrial ocean of water. That is 100 atm of CO_2 and 257 atm of H_2O. That means that it started out with a great mass of atmosphere and presumably a very high surface temperature.

The question I want to consider is how to get rid of all the water. The idea, of course, is to photodissociate the water, let the hydrogen escape, and thereby get rid of the water. Now the rate of escape of hydrogen is low on the earth because of the cold trap; the upper atmosphere is dry because the temperature of the tropopause is low. But the cold trap is possible only in an atmosphere with a low water vapor mixing ratio. On earth, with terrestrial temperatures, the water vapor mixing ratio is less than a percent or so and we have a cold trap.

But, and this is a key point made by Ingersoll, in a convecting atmosphere with more than ten percent water, a cold trap is not possible. The wet adiabatic lapse rate is very small in an atmosphere with more than ten percent water, the water vapor mixing ratio decreases very slowly with altitude, and the tropopause occurs at very high levels.

So if the primitive atmosphere of Venus was predominantly steam at lower altitudes, it would have been predominantly steam at the upper altitudes, too. There would have been no cold trap.

DR. MC ELROY: For this atmosphere, isn't the vapor pressure known as

a function of temperature? Is it just an equilibrium all the way up if you start at the bottom?

DR. WALKER: I think you can calculate it, yes.

DR. MC ELROY: Wet adiabatic?

DR. WALKER: Wet adiabatic, yes. And the water vapor mixing ratio is high all the way to the top. That's the point that Ingersoll makes.

DR. YOUNG: Yes, but there has to be a cold trap because whatever level in the atmosphere radiates to space has to be in thermal equilibrium with the incoming sunlight.

DR. WALKER: At that level, water vapor is still the dominant constituent. I agree that the water vapor pressure is very small there, but the pressure of everything else is very small there also.

This assumed primitive atmosphere would be predominantly steam. A lot of hydrogen would be produced by photochemistry. Hydrogen gives a high exospheric temperature. This is the problem that has been considered by Smith and Gross, Hunten and McElroy, Hunten, and various others.

And what we find is that hydrogen escapes from such an atmosphere essentially as fast as it is produced by the photolysis of water vapor. Ignoring the changes in the solar luminosity with time, that rate is about 10^{13} molecules cm^{-2} sec^{-1}. That would be the escape rate from the steam atmosphere on Venus, and that rate is limited by the supply of solar photons capable of dissociating water vapor. That corresponds to the destruction of 4.3×10^{16} gm yr^{-1} of water. At that rate it would take 31 million years to destroy all of the water.

So there would be a very rapid decay of this initially large water atmosphere that I've described. It seems clear that this kind of escape rate would exist in a primitive steam atmosphere. So the water content of the primitive atmosphere really comes dropping like a stone in the initial degassing model.

Now there's a problem with the oxygen that gets left behind, because this photolysis of water and escape of hydrogen is converting the water into oxygen. Well, in a nutshell, I don't for a moment think we can get rid of oxygen at anything like that rate; oxygen had to accumulate.

DR. GROSS: The blow off of hydrogen at the top of the atmosphere should carry oxygen with it.

DR. WALKER: Well, maybe, maybe not.

DR. MC ELROY: If I take the 10^{13} molecules cm^{-2} sec^{-1} you require, you'd need to supply ~8 ergs cm^{-2} sec^{-1} to keep that going. That's just the escape energy. Where do you get that from?

DR. WALKER: I don't know. I'd have to think about that for a while.

DR. MC ELROY: The question is can you keep the atmosphere hot enough to keep it going?

DR. RASOOL: Is this the thermal escape or is this the blowoff?

DR. MC ELROY: It doesn't really matter.

DR. WALKER: I guess the answer would be that it is advected up from below.

DR. MC ELROY: There should be adiabatic cooling?

DR. WALKER: The lower atmosphere will supply the energy.

DR. JONES: There's a lot of uncertainty in these calculations. I think you can do it with the solar heating of the atmosphere, if you are willing to extend the wavelength range a little bit.

Okay. What are we going to do with the oxygen?

Well, I don't for a moment think that we can consume oxygen in the crust of Venus at anything like that rate. For the assumptions we've made, oxygen is produced at a rate of 3.9×10^{16} gm yr^{-1}. It would be necessary to weather 1500 km^3 of basalt every year to consume that much oxygen.

Terrestrial weathering rates are not nearly that large. There is not that much fresh basalt delivered to the surface of the earth, let alone exposed. If you evaluate how fast oxygen is being consumed at the surface of the earth today, you find approximately 3×10^{14} gm yr^{-1}. On earth oxygen is consumed mainly by oxidizing organic material in sedimentary rock. The rate at which oxygen is consumed by erosion and weathering of terrestrial igneous rocks is very much smaller than this.

In addition, this figure on earth is enhanced by the activities of organisms which break up rocks, corrosive action on the rocks, and the activity of running water which transports rocks. For all these reasons I think that the rate of consumption of oxygen on Venus must have been smaller.

DR. JONES: What about tectonic activity? Perhaps rocks are exposed at a higher rate than you might expect.

DR. WALKER: Well, perhaps they are. But the point of what I just said is that they would have to be exposed at a very much higher rate on Venus than they are on the earth.

DR. SAGAN: How do you know it's impossible?

DR. WALKER: I don't know that it's impossible.

DR. JONES: Is it possible?

DR. WALKER: It is possible, but I don't think it is very likely.

So ignoring freakish escape processes, oxygen would accumulate. We must get rid of the oxygen sooner or later to get to the present day, and that is why, when I started talking about this, I said that the loss of water from Venus suggests that the atmosphere reacts with the rocks. I think the atmosphere of Venus has to react with the rocks to get rid of the oxygen, so I think the loss of water from Venus provides evidence for a supply of fresh rocks.

DR. MC ELROY: If you have a potential macroscopic flow of hydrogen out of the top and you're not worried about 8 ergs cm^{-2} sec^{-1} going out, why not supply 16 times as much and get some of the oxygen out with it?

DR. RASOOL: That's a good question.

DR. WALKER: It is difficult to get the oxygen out.

DR. JONES: The oxygen may still be in the atmosphere. There's plenty of oxygen in the CO_2.

DR. WALKER: I think the CO_2 went into the atmosphere in the form of CO_2, not in the form of carbon monoxide or methane.

DR. JONES: If you heat up any reasonable material that you see today out in the solar system, like meteoric material, things like methane and carbon monoxide come out.

DR. WALKER: What comes out of the earth when you heat it up is things like carbon dioxide and water.

DR. JONES: The earth is already recycled many times.

DR. WALKER: The equilibrium in the upper mantle of the earth, the FeO to Fe_2O_3 ratio, is such as to give you principally oxidized--

DR. JONES: Well, the question is where did you get the Fe_3O_4 from? Why not FeO on the left side?

DR. WALKER: This is just a representative--

DR. JONES: Then you've [expletive deleted] it.

DR. MC ELROY: Delete the expletive.

DR. WALKER: My feeling is that the gases that were released on Venus were similar to the gases that were released on the earth. They would be principally water vapor, carbon dioxide, with very much lower amounts of hydrogen and carbon monoxide.

Another reason why the loss of water from Venus implies that there has been interaction between the gas and the rocks of Venus is that photolysis of water vapor and escape of hydrogen apparently cannot get the water vapor content of the Venus atmosphere down to the present very low value.

There is a very rapid escape initially when the atmosphere is principally steam. But when water vapor becomes a minor constituent of the atmosphere, the escape rate drops off precipitously. It drops off precipitously first because, when the water vapor becomes comparable in density to the carbon dioxide or the oxygen, there is a diffusion bottleneck established and the hydrogen cannot escape faster than hydrogen compounds are transported upward into the thermosphere. As the water vapor content goes still lower, a cold trap becomes effective, drying out the upper atmosphere and further reducing the escape rate of hydrogen and the rate of destruction of water. As the water vapor content goes lower still, hypothetically the sulfate trapped in the clouds can become effective to further dry out the upper atmosphere and reduce the rate of destruction of water in the atmosphere of Venus.

I will sketch very schematically the rate of destruction of water or the escape flux of hydrogen as a function of the water vapor mixing ratio (Fig. 1). For an atmosphere that is principally water, the escape rate is about 10^{13} cm^{-2} sec^{-1}. When the water vapor becomes comparable to carbon dioxide or carbon monoxide, the rate begins to fall off due to the diffusion bottleneck. Then at around the ten percent level, a cold trap becomes

effective and the escape rate drops still further. And if water is trapped in the sulfuric acid clouds, that further dries out the upper atmosphere and the escape rate drops off still more. Finally, when the thermosphere of Venus cools down to its present low temperature, ~350°K, there is a type of escape that is no longer controlled by the diffusion bottleneck, and the rate drops down to a still lower value. It might be on the order of 10^6 cm^{-2} sec^{-1} at the present water vapor content.

Fig. 1. *Schematic graph of hydrogen escape rate.*

If the exospheric temperature of Venus were higher, and if the sulfate trap and the cold trap were still in position we'd have escape rates of about 10^6 cm^{-2} sec^{-1}. At that escape rate it would take more time than we have to get the water vapor down to the present level.

In other words, if you want to dissipate this large amount of water from Venus, you can dispose of the first 90 percent of the water very readily from photolysis and the escape of hydrogen. Then it becomes very hard to get rid of the last few percent to get down to very low mixing ratios.

Therefore, I think the water must have gone into the rocks to get down to a half a percent or a hundreth of a percent or whatever there is right now. And that provides further evidence for a supply of fresh rocks on the surface of Venus.

Let me now show you my model of the evolution of the atmosphere of Venus (Fig. 2), the one I have just described, with an assumption for an oxygen consumption rate on Venus equal to the present day oxygen consumption rate on the earth, which I think is an overestimate. But I might have overestimated the initial complement of water on Venus, too.

Water decay is rapid. Oxygen accumulates, then there is a change of time scale and the oxygen is gradually used up. Carbon dioxide doesn't do anything. That's the instantaneous degassing model. If you don't like this model you can change the initial conditions, or you can change the oxygen consumption rate. The rules of the game are easy to play, and you can get your own evolutionary history.

A more plausible model is perhaps one of gradual degassing, degassing lasting for a billion years or something of that order. The important thing about gradual degassing is that it is possible to have an equilibrium between the rate at which water is released from the interior of Venus and the rate at which it is destroyed by photolysis and escape of hydrogen. In fact, if the escape rate looks like Fig. 1, such an equilibrium would be established unless degassing was extremely rapid, that is, unless degassing occurred on a time scale of less than 30 million years.

Figure 3 illustrates the gradual degassing model. When equilibrium is established, there is no build up of a predominantly water vapor atmosphere.
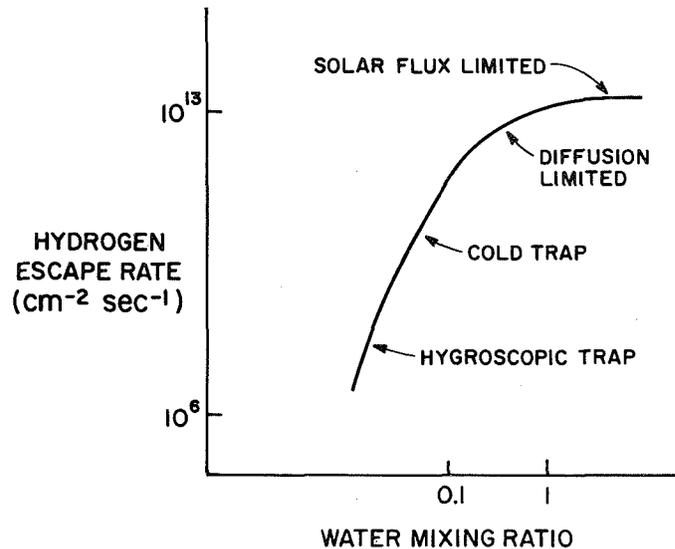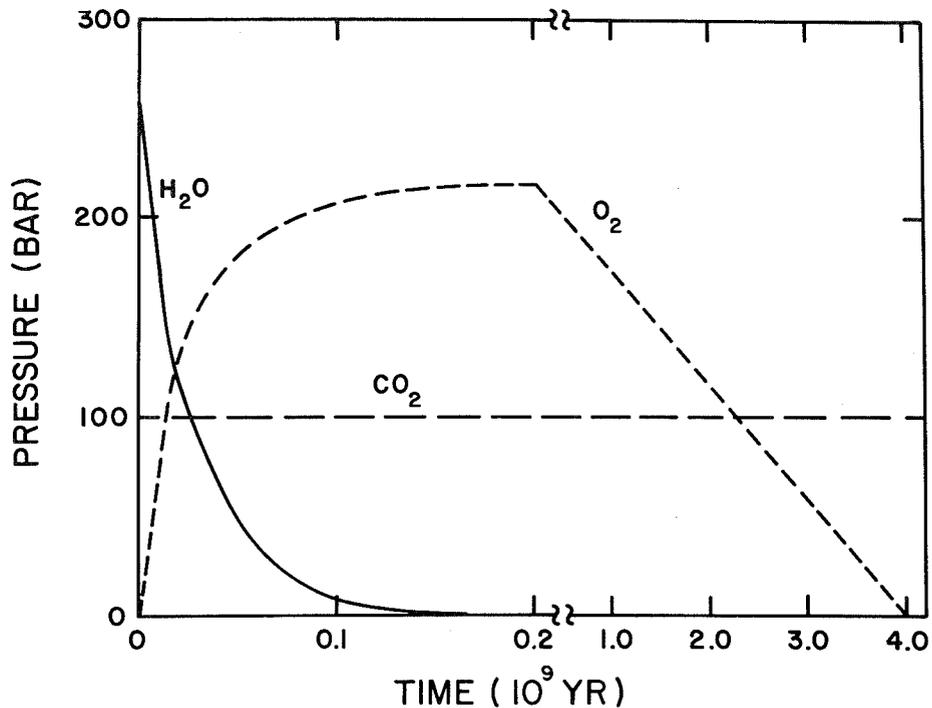
Fig. 2. Instantaneous degassing model for the evolution of the atmosphere of Venus.

It is possible to build up a predominantly oxygen atmosphere, depending on the assumptions made for the rate of release of water vapor and the rate of consumption of oxygen. But water vapor is buffered at fairly low levels by the requirement that it be destroyed and escape from the atmosphere as fast as it is released from the crust.

In this gradual degassing model it is probably essential to have water released from the planet. It is not clear to me that the present high surface temperatures on Venus could be achieved without ever having any water in the atmosphere.

Now, to summarize, I think the key evolutionary event is the runaway greenhouse. I don't see how Venus could have got the way it is today if water had condensed upon its surface. It is not clear to me that the chemical equilibrium theory is established. A lot of water has been lost from Venus, I think, and I think it is possible that at one time Venus had an oxygen atmosphere which has since dissipated.

DR. RASOOL: This number for the amount of water in the oceans of the earth is a very sacred number. When I used to worry about these problems, in this room actually where I was teaching Columbia students, I gave an assignment to a student to find in the literature on volcanic activity the rate of volcanic outgassing of water today, because nobody seems to know whether we had spontaneous outgassing in the early history or whether it's coming out slowly as a function of time.

So I said, "Fine, if you can find out how much water comes out from the volcanoes, we can probably do something." And he came back one day very excited and said, "The rate is 5×10^{14} gm yr^{-1}." So I said, "Fine. Let's divide that into the mass of the oceans." And we got 4×10^9 years, which
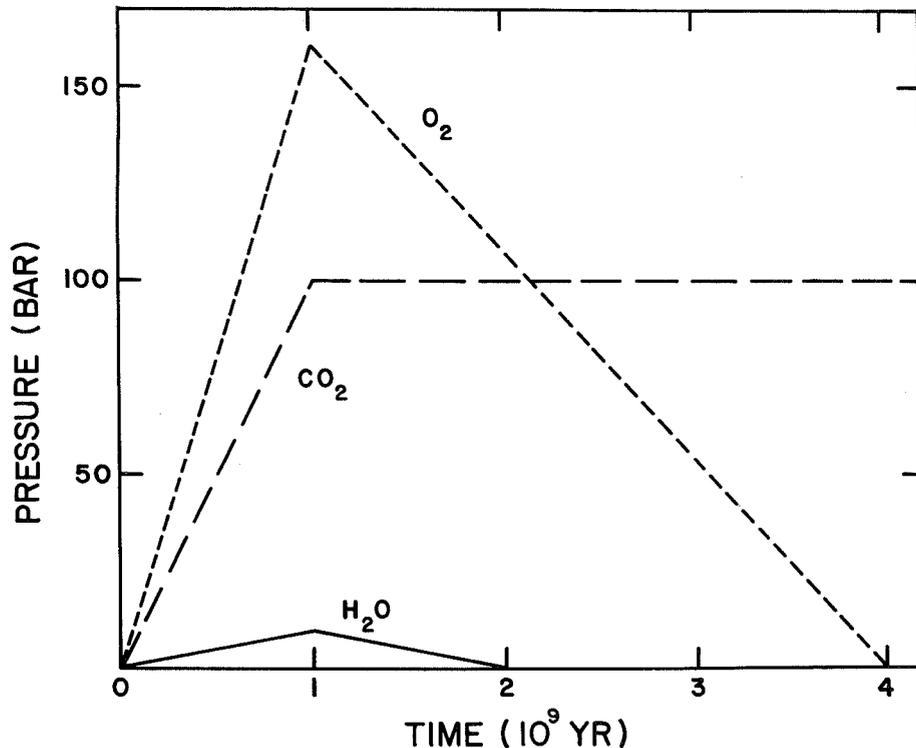
Fig. 3. Gradual degassing model for the evolution of the atmosphere of Venus.

is the age of the planet. So I said, "Let's grab a secretary and type a letter to Nature."

But it pays to be somewhat prudent. So we looked up the reference, and found that the author had taken the amount of water in the oceans and divided by the age of the earth.

DR. MC ELROY: To come back to the question of the energetics of the initial water atmosphere, I think it is interesting to think of what actually happens to the photochemistry, the aeronomy of a pure water atmosphere.

There is no oxygen in the beginning. Water dissociates and some O_2 and H_2 accumulate. And that process continues until there is sufficient feedback to start reforming water rather than H_2.

It's possible that you reach an explosive limit beforehand. You're storing chemical energy as you go along. You may actually reach an unstable limit in which case it is possible to imagine blowing the top off the atmosphere. So you'd blow off the hydrogen and the oxygen, start all over again and go through a series of big bangs.

DR. WALKER: And probably modify the orbit of the planet.

DR. RASOOL: The temperature of Venus should be 3°K now.

DR. MC ELROY: I wish you hadn't said that.

DR. SAGAN: Even molten lavas have something like a percent of water in them. Certainly terrestrial rocks have about a percent of water. I

would expect Venus rocks to have something like a percent of water in them.

If my mental arithmetic is right, a 10 km thickness of rock with one percent water in it is 10^{23} gm for either Venus or the earth, so it would certainly be less than the amount of water in terrestrial oceans. It would be two orders of magnitude more than what is in the Venus atmosphere.

So the main sink of water on Venus today would seem to be the crust and not the atmosphere.

DR. WALKER: The amount of water in the crust of the earth is not negligible, but it is smaller than that number. One percent is probably a high estimate for the water content of a typical terrestrial crustal rock.

DR. POLLACK: There is so much recycling that goes on. You can never be sure.

DR. WALKER: My feeling, which is sort of qualitative, is that because of surface temperatures that exist on Venus, less water is going to go into the crust of Venus than has gone into the crust of the earth.

DR. SAGAN: In Jeffrey's book, The Earth, he makes a point about one percent of lava being water, and lava is at a higher temperature than the surface of Venus.

DR. YOUNG: Water is what makes the stuff runny. So only the wet stuff runs out.

DR. JONES: On the question of what are the volcanic effluents of the earth on the surface: Interestingly, plate tectonics shows that we have a deep turnover, in periods of time much less than 4×10^9 years, which carries oxidizing materials down to the asthenosphere where it comes out again in volcanoes.

So it is not surprising that volcanic effluents in an oxygen atmosphere should be oxidizing. What is interesting is that there is a significantly reduced component in volcanic effluents even with this oxygen atmosphere.

DR. WALKER: The amount of oxygen in the atmosphere is totally negligible when compared with the amount of carbon in the crust. Whenever the crust goes down into the asthenosphere it carries much more reducing power than oxidizing power.

DR. SAGAN: What I'm saying is that the material which is carried down is more likely to be the material which comes back up again. Each molecule is not equal.

DR. WALKER: When it's down there it comes to equilibrium with the rocks that are there.

DR. SAGAN: There are certain locales where the circulation is more likely. Those are the places where you are going to get the volcanic effluents, and those are the places which are preferentially rich in material that has been carried down and therefore the material which has seen oxygen.

What I'm suggesting is that were there no green plants on the earth and were there no oxygen here, volcanic effluents would be strongly reducing, so it would not be CO_2 that would be coming out, but reduced gases.

DR. WALKER: That's a nice idea, but the evidence is that the oxidation state of the volcanic gases is determined by equilibrium with the kind of

rocks we think are in the upper mantle, not by equilibrium with the kind of sediments we think are going down in subduction zones.

DR. JONES: In connection with the model of Lewis whereby Venus was formed initially with very little water and you argued that of course as a result of this reaction you still would get water, 10^{23} gm, I recall that Lewis also has argued that he can get all the CO_2 in the atmosphere by reaction with FeO, which is apparently the main source for his CO_2.

DR. WALKER: Well, I don't care whether this is Fe_3O_4 or FeO. What I'm interested in is how much water I get out of it.

DR. PRINN: The carbon could react with the FeO to give CO_2.

DR. WALKER: So you don't get any water. That is definitely a different scheme.

DR. INGERSOLL: I had always worried a great deal about getting rid of the last few percent of water, but I missed your argument. It went by very fast.

DR. WALKER: I didn't give it.

DR. RASOOL: You made that argument yourself, Andy.

DR. INGERSOLL: Well, I'm worried about how do you actually get rid of the water, since it's not there.

DR. WALKER: I suppose that it goes into the rocks.

DR. INGERSOLL: It seems like a stumbling block. I was using it as evidence that there are reactions between atmospheric gases and rocks.

DR. TUREKIAN: I'm startled that the number of geologists here is very small.

The rocks that have come out of Hawaii and the mid-oceanic ridge are not the ones that have gone down in the subduction zones. If you look at, for instance, Jack Dymond's data on the rare gas composition in the glassy margin of basaltic boulders that are found along the spreading centers, the relative abundances don't look like atmospheric gases at all; they have never seen the atmosphere.

And if you look at the oxidation state of mantle-derived rocks, or if you look at the inclusions in the basalts from Hawaii, they are composed of CO_2 and H_2O, not CO and H_2, also indicating an oxidizing mantle.

DR. JONES: How do you explain the amount of hydrogen that comes out of volcanoes?

DR. TUREKIAN: It's a secondary reaction.

DR. JONES: How do you explain the sulfurous fumes that come out?

DR. TUREKIAN: Those are all second-order type things. The quantity is very small.

DR. JONES: The problem with measuring the H_2 that comes out is that it burns the minute that it gets out. So people have been underestimating its amount.

DR. WALKER: I'm assuming that the upper mantle on Venus is the same way.

DR. SAGAN: I have a general problem. This is an old story.

DR. TUREKIAN: The earth is a very old story.

DR. SAGAN: This story is not quite as old.

My problem is that an oxidizing or neutral oxidation state atmosphere cannot produce the organic compounds necessary for the origin of life.

DR. TUREKIAN: That's not at issue here. I don't think you should bring that into issue.

DR. SAGAN: I wish to bring it in.

DR. TUREKIAN: You ought to give a talk then.

DR. SAGAN: The point is that you cannot have oxidizing and neutral oxidation state conditions if you want to make organic compounds. Organic compounds are necessary for the origin of life. The origin of life on earth occurred 3 or 4 billion years ago. And the present atmosphere of the earth is of secondary origin. Therefore, the outgassing from the earth in the first billion years of the earth's history had to be reducing.

DR. RASOOL: Of course that is a major problem. There are people who have given arguments that you can make amino acids and CO.

DR. TUREKIAN: Well...

DR. RASOOL: You can speak on that during your talk.

DR. WALKER: I'm not saying you don't get any hydrogen out of volcanoes.

DR. GROSS: A number of thermospheric temperature calculations have been made. I have been as guilty as others in this. You get a very high temperature and you immediately say it's a blowoff condition. When you get this high temperature you know you've got the wrong answer because the true situation is a dynamic rather than a static condition. You have this so-called blowoff which requires solving dynamical equations rather than static equations, which are usually solved in getting an atmospheric temperature profile. When this happens, all constituents, at least in the thermosphere which is the hot region, will blow off. The exospheric temperature has no meaning under these circumstances, at least not for escape calculations.

And interestingly enough, while you were talking, I made a simple calculation. If you took the 10^{12} level for oxygen with a scale height of only about 10 km and you lost all of this, this is now not the entire oxygen atmosphere but just at the level of 10^{12}, you lose something like 10^{21} or 6×10^{21} gm yr^{-1} from the entire planet as against your figure of 10^{16} or 10^{14} gm yr^{-1}.

DR. HUNTEN: The oxygen will not blow off unless the exospheric temperature is hot enough for oxygen escape. Hydrogen will not carry it away. So despite Stan's disclaimer about the temperatures calculated, he needs a temperature of 10,000°K to get that.

DR. RASOOL: I saw Don taking notes carefully. In his review he can enlighten us further.

BOUNDARY CONDITIONS ON ATMOSPHERIC EVOLUTION

Gustaf Arrhenius, Scripps Institution of Oceanography

In discussing the history of the atmosphere of Venus, many people take as time zero a time when the planet was already formed. I think it is important to emphasize that much of the evolution of atmospheres is determined even earlier during the process of planet formation. And the conditions during formation are determined still further back, essentially when matter originally condenses during the very early history of the solar system.

This pre-history of the terrestrial planets and the atmospheres is obviously surrounded with a tremendous amount of uncertainty, so much that it would be naive to rely on some single, simple model. Nonetheless, I think one can establish boundary conditions on this problem, and I'll try to emphasize that aspect.

Such boundary conditions can be narrowed down much more today than even a decade ago. This is due partly to the much improved observational material on the planets themselves, including the moon, and also due to the fact that during the last decade we have learned an enormous amount about the behavior of the interplanetary plasma, partly by spacecraft observation and partly by significant model experiments in the laboratory. So we now understand much better how matter behaves in space.

By elimination it is then possible to narrow down the framework within which one is allowed to operate, provided that you adhere to, as one of the rules of the game, the tenet that you are not allowed to deduce new laws of physics which are not experimentally verified. That might sound truistic, but such a procedure is not unknown to cosmologists.

For example, it is now possible to immediately eliminate the type of processes for planetary formation which rely on collapse of gas clouds. This was a very favored type of theory during the 18th and 19th centuries, having originated from an idea by Laplace that he never worked out in detail; it is now well known to be erroneous. I think most people who deal with the formation of planets have now abandoned such ideas, although there are reminiscences of these theories. Gravitational collapse of gas clouds as a theory of planetary formation is not tenable because the mass of any given planet is far too small to permit accumulation by gravitational collapse. Jupiter is a possible exception, but even in this case there are other limiting conditions that preclude gravitational collapse as shown by Kumar [Astrophys. Space Sci. 16, 52, 1972].

It is also possible to eliminate essentially all pre-20th century theories which rely on the belief that matter in space behaves like rarified gases in the laboratory, and which therefore fail to take into account the controlling importance of magnetohydrodynamics in all fluids in the known universe, except in planetary atmospheres, lakes, rivers and oceans. The criterion for justifiably ignoring magnetohydrodynamic processes is essentially that the MHD counterpart of the Reynolds number be much less than one. It suffices to say here that in all observationally known objects in the universe, with the exceptions mentioned, this number is something in between 10^{21} and 10^{15}. In dark clouds it might be as low as 10^6 or 10^7, but even so it is a million times larger than that which permits the neglect of magnetohydrodynamic effects.

So at present we are left with only one kind of hypothesis for planetary

formation, namely the planetesimal type, pioneered in modern times especially by Alfvén [Stockholm Obser. Ann. 14, no. 2, 1942; no. 5, 1943; no. 9, 1946] and by Schmidt [Dokl. Akad. Nauk SSSR 45, 245, 1944] and his collaborators in the Soviet Union. I think it is true to say that it is the basic type of theory used in the field today.

What is left then is a rather restricted set of permitted courses for the formation of the primordial solid material, and one has to stay within this framework to arrive at an allowable baseline for later evolution of planets and planetary atmospheres like that of Venus. Even a decade ago it was common to discuss planetary evolution without considering the pre-history of the planet. Essentially, one assumed the planet to be presented by divine fiat, all made up in some specific fashion, and from that moment on the laws of physics and chemistry would begin to work. In this way it was possible, for example, to assemble by miracle an Earth or a Venus consisting of un-differentiated material similar to some source material, for example, meteorites. Hiding within these prefabricated planets were all the volatile components, as a source of future atmospheres and oceans to be gradually released by what is generally called, in a broad sense, volcanism.

Instead, it is now clear that as soon as the growing planetary embryo has achieved a size such that the escape velocity is significant, the kinetic energy of the impacting projectiles becomes so large that the vola-tiles, both in the projectile and in parts of the target material, are released at least temporarily into the atmosphere. There they may or may not condense, depending on the surface temperature of the embryo.

The impact velocity v_p is a function of the escape velocity v_e and the velocity v_i of the projectile relative to the growing object:

$$v_p = \left(v_e^2 + v_i^2 \right)^{1/2} \tag{1}$$

At the time when the constructive collisions occur the relative velocity is small so that the escape velocity dominates.

The kinetic energy at impact is at least $1/2\ mv_e^2$ where m is the mass of the projectile. At impact a certain mass fraction α of the projectile is melted.

$$\alpha = \frac{\gamma v_e^2}{2L} , \tag{2}$$

where γ is an efficiency constant, and L is the latent heat of fusion for the material in question, generally of the order of 500 joules gm^{-1}.

If, for example, we consider conditions when Venus had grown to one half its present radius, then, substituting for the escape velocity

$$v_e = \left(\frac{2GM}{R} \right)^{1/2} = \left(\frac{8}{3} \pi G \theta \right)^{1/2} R, \tag{3}$$

assuming a density θ of 5.5 gm cm^{-3}, and assuming that L = 500 joules gm^{-1}, we find that $\alpha = 25\gamma$. The correction factor γ takes into account the fact that not all of the kinetic energy is translated into heat; some must go into shock waves, acoustic energy, and so on. We don't know the value of γ in any given case, but even if it is as small as 4 percent, $\alpha = 1$ and the

entire projectile is melted. If, on the other hand, γ is say 100 percent, the projectile would melt 25 times its volume of material, or a substantial amount of energy would go into vaporization. A range of possibilities can be considered there.

The other major feature that determines the accretional heat profile during the growth of the planet is the rate at which the embryo, with its growing gravitational cross-section, sweeps up material. Time does not permit me here to go into the rather straightforward type of calculations. Suffice it to say that the major controlling variables are: First of all, the total mass that is involved, in other words that which eventually ends up as a planet but which originally is in this dispersed form; Secondly, the volume within which this material is distributed, for which one has at least an order of magnitude idea from the distribution of planets in the solar system; And thirdly, the total time during which material was originally added to the solar system, which is often referred to as the formation interval of the solar system. This is a very important concept that has been recognized theoretically for a long time by people like Urey [The Planets: Their Origin and Development. Yale Univ. Press, New Haven, 1952] and Levin [Tectonophysics 13, no. 7, 1972], for example, though other workers did not pay much attention to what they had to say regarding this important time factor. It is still common to see people making planets or making a solar system in very short times.

DR. MC ELROY: Six days, I believe.

DR. ARRHENIUS: Something like that.

There are various estimates for this time interval, but they generally are of the order of 10^8 years. Some suggestive information on this parameter comes from meteorite chronology.

Instead of analytically showing how these factors interact, I have tried to do that graphically. Figure 1 shows the radius of the various planets as a function of time, with 1.0 being the present radius. The abscissa shows the time scale, where 3×10^8 years is the duration of the formation interval. The calculation has very little sensitivity to the assumed time interval of formation. It doesn't make a terribly big difference if this interval is ten times less or even ten times more, although ten times more is obviously excluded from the point of view of what we know about the history of the earth.

There are basically three types of evolutionary paths. I will not discuss the group including Uranus, Neptune, Pluto and Triton at this point. For the second group, Saturn, Mars and the moon, the initial growth begins rather slowly and the final catastrophic runaway process happens very late in the formative era, so that the effects of the runaway process are seen in the surface features of the planets. For example, certain features of the lunar surface were predicted and later actually verified by what was found on the moon.

Another evolutionary path is represented by the third planetary group, the terrestrial planets and Jupiter, where the runaway process occurs quite early in the history of the formation, something like 10^7 years. After that growth is quite slow and controlled entirely by the rate at which material is fed into the system.

DR. RASOOL: What determines the grouping?

DR. ARRHENIUS: The total mass of the planet, and the volume in which

Present radius of planet --- 1.0

Absolute time →

Relative radius r/r_{sc} of growing planet
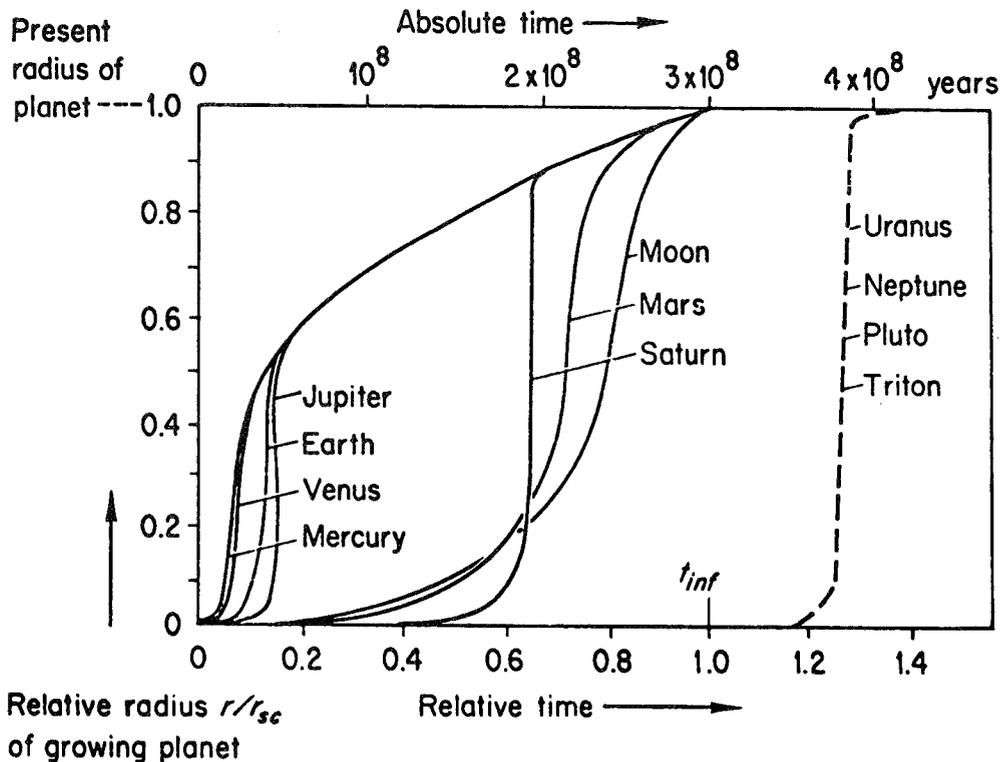
Relative time →

Fig. 1. The growth of planetary radii with respect to time. Runaway accretion occurs early for Mercury, Venus, Earth and Jupiter. The time of runaway accretion approaches that of the duration of mass infall for Saturn, Mars and the Moon. For Uranus, Neptune, Pluto and Triton runaway accretion occurs only after infall has ceased, and the jet stream has contracted; this growth is schematically represented by the dashed curve. [After Ip, Studies of small bodies in the solar system. Ph.D. Thesis, Univ. Calif., San Diego, 1974]

the material is distributed. For example, the earth has a relatively large mass, compared to Mars. The material must be distributed in a relatively small volume because it's limited outward by Mars and inward by Venus' sphere of influence, so you have a relatively small volume and a relatively large mass. For Jupiter, the mass is very large, but the volume of space in which it was originally distributed is also very large. Then applying the particle interaction mechanics to this system, the results of Figure 1 are obtained.

Figure 2 shows the thermal power delivered to the surface of the planet, in energy cm^{-2} sec^{-1} as a function of the growing size of the embryo. The thermal power essentially represents the intensity of the melting, which varies as the escape velocity which determines the impact velocity. For example, on the moon the thermal power delivered culminated at 0.8 of the present radius, i.e., near the present surface and never reached very high values because of the low gravitational field of the moon.

The thermal profiles of the earth and Venus have great similarities. Once the embryos of these planets obtained the size of the present core of the earth, about the size of the moon, the growth of the mantle occurred at a low rate, and the average thermal power developed at the surface is also low. In this situation every place where a projectile hits will be very strongly heated and melted, but as such events occur relatively rarely over the surface of the earth there is what we call a hot spot front, the growth of the planet by local impacts (hot spots) in which melting takes place,
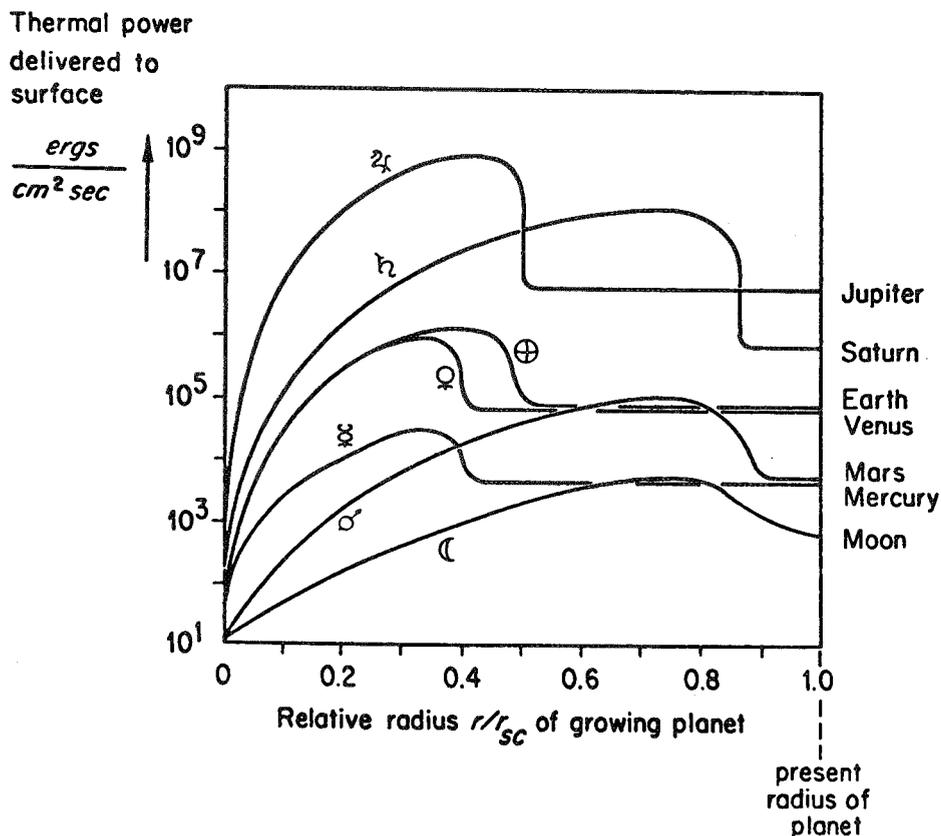
Fig. 2. *Thermal profiles of the growing planets.* [After Ip, *Studies of small bodies in the solar system. Ph.D. Thesis, Univ. Calif., San Diego, 1974*]

with the average temperature of the planet being low.

Figure 3 shows a bit more detail for the earth, and in principle the same holds true for Venus. It shows again the thermal power diagram, with the culmination followed by a very rapid dropoff of heating, on the average, during the rest of the period.

There is now some experimental evidence that the material of which the planets are made has some similarity with the material that we see in space. For example, we know that the noble gas composition of the earth's atmosphere is very similar to that of the so-called planetary gases in meteorites, suggesting that the earth and probably the other terrestrial planets are made of material roughly similar to what we've seen in meteorites.

Upon impact and melting of a projectile any volatiles within the projectile would be released. At first the volatiles could remain in solids because the heat generation is not enough to cause evaporation, but eventually the water and any volatile would be driven out into the atmosphere. However, the atmosphere is not retained at this time because the planet's escape velocity is too low, so the atmospheric gases escape.

As the embryo grows bigger the escape velocity increases, and retention of an atmosphere begins. In Figure 3 only the accumulation of water is shown, but of course the same thing holds true for any other volatile, taking into account its molecular mass.
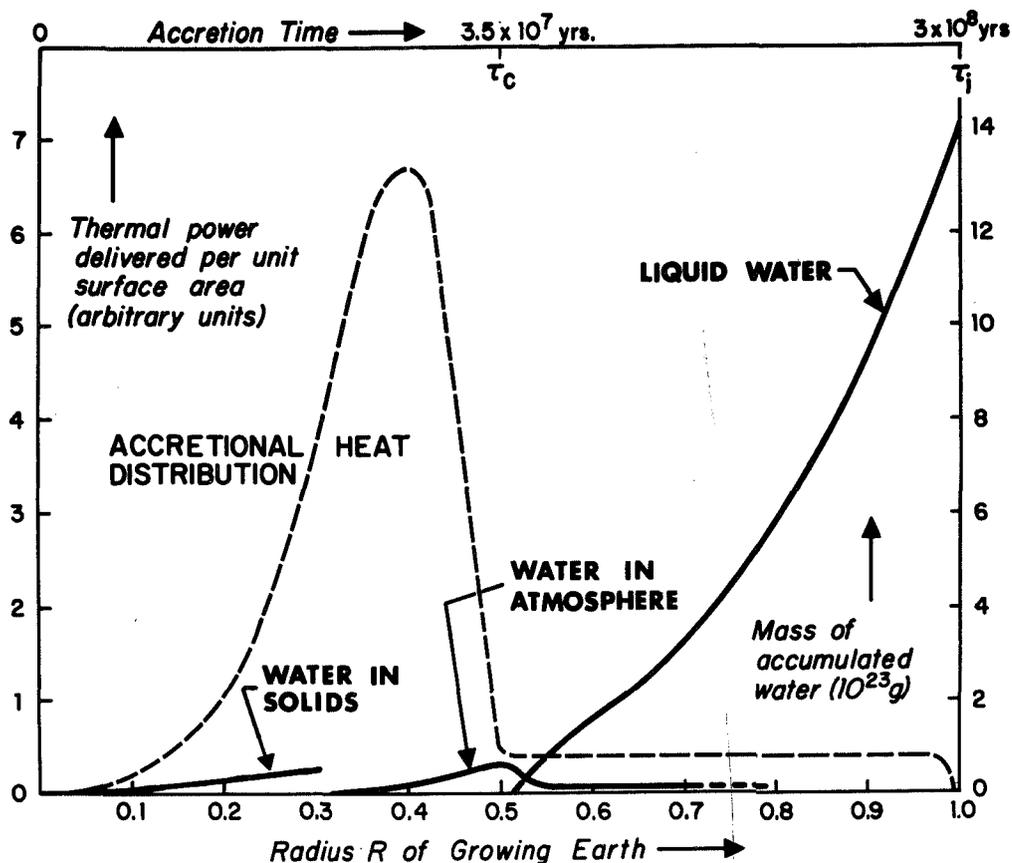
Fig. 3. The dashed curve and the left hand ordinate scale show the thermal power (in arbitrary units) delivered per unit surface area of the growing Earth by impacting planetesimals. The lower abscissa scale shows the radius of the growing Earth in fractions of the present size. The upper (non-linear) abscissa scale shows the time elapsed from inception of accretion. The three solid curves show the accumulation of water on Earth. The left curve represents the amount retained in the cooly accreted inner core. The middle curve shows the accumulated water in the atmosphere and the right hand curve shows the accumulated liquid water. The final mass of accumu-lated water has been adjusted to equal the present ocean mass. [After Arrhenius, De and Alfvén, in The Sea, 5, ed. E. Goldberg, Wiley, New York, p. 839, 1974].

   In the case of the earth an important difference from Venus has been touched upon several times here, and also in the classical paper by Rasool and de Bergh [Nature 226, 1037, 1970]. It is possible to condense water and to keep the surface temperature low on the earth, but on Venus the black body temperature is sufficiently high to keep the water and other volatiles in the atmosphere, resulting in a runaway greenhouse effect.

   I limit myself to these remarks, since there is only a short time available. Obviously they have many implications with regard to a number of questions that were raised earlier during the discussion here, and I would be happy to try to discuss these informally.

DR. JONES: Do you have a graph similar to Figure 3 for Venus?

DR. ARRHENIUS: No, but the conditions on Venus would not be terribly different from those for the earth.   The culmination comes a little earlier in time, and the thermal power is slightly lower.

In connection with what Carl Sagan said about the carbon dioxide in the atmosphere of Venus, I think there is quite substantial evidence that the early atmosphere of the earth (and by analogy that of Venus) was reducing, not only because life exists but also this is indicated in the nature of Precambrian sedimentary deposits.   It is frequently assumed that the projectile material that built up the planets was similar to meteorites. During heating of meteorites the gases released are hydrocarbons, carbon monoxide, water, cyanides and other reduced compounds.   Magnetite [$FeO \cdot Fe_2O_3$] is present in meteorites, but there is also metallic iron which was not mentioned in the previous discussion.

So it is reasonable to assume that the early atmosphere consisted mainly of a methane and carbon monoxide atmosphere; hence all the free oxygen that caused the problems in previous discussion here was not yet present and the carbon dioxide that we see now has evolved gradually.

INHOMOGENEOUS ACCUMULATION MODEL

Karl Turekian, Yale University

The presentation by Turekian is largely contained in the paper by Turekian and Clark which will appear in the special issue of the Journal of the Atmospheric Sciences [32, June 1975]. The abstract of that paper follows:

The non-homogeneous accumulation model for the formation of the terrestrial planets is described and its consequences for the formation of the Venusian atmosphere are assayed in the context of our knowledge of the composition of the Earth and carbonaceous chondrites. The relative abundances of the low temperature condensibles in the reservoirs at the Earth's surface are applied to Venus. Although carbonaceous chondrites show similar properties for the chemically bound elements, they show large deficiencies for the rare gases and less so for water. This can be explained as the result of losses somewhere along their trajectories. The major gases on Venus, by volume, are predicted to be 98.12% CO_2, 1.86% N_2 and 0.02% Ar^{40}.

DR. JONES: The chemical condensation model of Lewis is essentially homogeneous, isn't it? In a sense of course the condensation makes it inhomogeneous. Isn't the question really that of the time scales?

DR. TUREKIAN: The question concerns the condensation and accretion time scales. There are mineral isocrons in meteorites which tell you when metamorphism stopped. But they don't tell you the time when the initial body was formed. It could have taken place in 10^4 years.

DR. JONES: So you feel it is really the condensation time which determines whether it is inhomogeneous or homogeneous?

DR. TUREKIAN: I think condensation in sequence and accretion determine the composition of the planets and the zoning.

DR. ARRHENIUS: Your sound and appealing observations deserve a more realistic theoretical rationalization than through the hypothesis of equilibrium condensation. This hypothesis postulates that condensing grains in space, cooling by radiation through a hot surrounding gas remain at the same temperature as the gas. From this self contradictory assumption is derived a chemical evolution story with temperature estimates that at the high temperature end must be off by an order of magnitude.

DR. TUREKIAN: The thing that I want to say is that the inner parts of the planetary systems are gas-free high temperature condensates rich in uranium. And this must be combined with surface bombardment by low temperature condensates.

DR. ARRHENIUS: I think everybody will agree with you on that. What I was just going to suggest is that you could achieve the same thing in a different manner, by accepting that the solar system was never homogeneous. This is suggested by many facts, including the composition of the planets, inferred from their densities. The terrestrial planets have considerably higher densities than the Moon and Mars. Venus and Earth are essentially in a boundary region between much lower density material further out and high density material indicated by Mercury in the innermost part of the solar system.

It is easy to imagine (and not impossible to justify theoretically) how in a solar nebula, which would become fractionated chemically while accumulating over a time period of the order of 10^8 years, the growing embryo of Venus (and that of Earth) accreted first mainly from one type of material (A-cloud) and later from another (B-cloud) with partial overlap in effective critical velocity [Alfvén and Arrhenius, Astrophys. Space Sci. 29, 63, 1974]. You don't need to be more complicated than that to support the realistic possibility of heterogeneous accretion. But I would be sorry to see you depend on a model which brings you down in flames.

DR. TUREKIAN: I don't really care about a detailed model as long as I have a gas free high temperature component. If you want to give it to me in some other way, fine.

DR. ARRHENIUS: Another argument in your favor is also highly interesting with regard to the atmosphere-ocean system on Earth, compared with the situation on Venus. If the Earth had formed, not with an initial core, but with the iron dispersed throughout to start with, then the temperature of the planet would have risen to the order of 10^4 degrees when the core subsequently formed. The entire ocean and all volatiles including the limestone carbon dioxide or its precursor gas would have formed a thick atmosphere effectively preventing the molten planet from cooling down in time for formation of the more than 3 billion year old crust. It is questionable if such an atmosphere would ever change to the present state on Earth. If one tries to defend such a scenario it becomes necessary to invoke an *ad hoc* catastrophy such as a "solar gale" blowing away these embarrassing early atmospheres, followed by a sprinkling of the degassed planets with sufficient cometary material to provide the volatiles seen today, here and on Venus.

It appears reasonable (but not unique) to resolve the problem of the core formation in the terrestrial planets in the general manner that you have outlined, on the condition that you base it on an accretional evolution of the planets tied to a formation and evolution of the solar nebula which obeys the laws of behavior of matter in space.

DR. JONES: Do you find it at all dangerous to use CRAP [Calcium-Rich Allende Particles] as a sort of universal model for mantle formation, when analysis of the oxygen isotope data indicates that CRAP is very unusual and perhaps rare. Is that a contradiction?

DR. TUREKIAN: No. That's an important point. We have to have a planet which is essentially free of the carbonaceous chondrite veneer, except for a small superficial part called the moon which is made up of everything but CRAP, and perhaps some iron and iron silicate.

DR. JONES: The moon isn't CRAP inside?

DR. TUREKIAN: The moon is not as much CRAP as some people believe. Not according to the composition that Syd Clark and I, and also Larry Grossman, have calculated on the basis of such things as heat flow data.

DR. JONES: If the moon is CRAP then you have a problem with the relative amount of iron, don't you?

DR. TUREKIAN: No. The sequence is CRAP, metallic iron, and then essentially magnesium silicates like olivine. My argument is that this sequence accumulates, and as a result of gravitational heating things melt and react; the iron drops out, and then the CRAP, and the silicates form whatever the outer part is made of. It is not just pure CRAP.

DR. RASOOL: Let's stop at that point.

CRUST-ATMOSPHERE INTERACTIONS

Philip Orville, Yale University

I want to talk first in a general way about atmosphere-crust inter-actions, specifically buffering reactions, and then give some concrete examples for Venus.

First of all, let's make clear what we mean when we say "buffering". It is different from a steady state in which, for example, there is inter-action between atmosphere and crust and very little change in the composi-tion of the atmosphere with time because things are added at the same rate as things are taken out. This may involve completely disequilibrium states, and there may be feedback which keeps it from getting too far from the particular composition. That is not buffering, that's just steady state. By "buffering" we mean a pretty close approach to equilibrium in some particular environment at the atmosphere-crust interface or near that inter-face, in which a departure from this buffered composition of the atmosphere will drive a reaction in the opposite direction to use up what was in excess or to balance a ratio of volatiles.

Table 1 is just a list of conditions for the buffering of an atmosphere by the crust in completely general terms. First of all, there must be some reactions involving the volatile constituents being buffered and certain solid phases. You can look up thermochemical data in the literature and calculate an equilibrium constant for a balanced chemical reaction involving certain volatiles and mineral phases. But the fact that you can calculate a number, an equilibrium constant, for any particular reaction does not make the mineral phases involved in the reaction a buffering assemblage for volatiles.

A second condition is that the solid phases that take part in a reaction involving the volatile constituents must be stable together in the same rock. This eliminates many combinations of mineral phases from consideration as buffers and focuses our attention on assemblages of minerals likely to be stable in common bulk compositions of rock.

The third condition for buffering is that there is sufficient rate of reaction between the mineral phases and volatiles that equilibrium is closely approached. As a metamorphic petrologist I am willing to assume that at temperatures of 450 to 500 C at the surface of Venus there is likely to be a fairly close approach to equilibrium between mineral phases of the Venus crust and volatiles of the Venus atmosphere in geologically reasonable times.

The fourth and final point is a matter of quantity. If certain consti-tuents are to be buffered by reaction with material at the crust surface,

1) reaction involving volatile constituent and certain solid phases.

2) these solid phases together in same rock.

3) sufficient rate of reaction

4) solid phases supplied faster than volatile.

Table 1. Requirements for Buffering of Atmosphere by Crust.

the mineral phases that are
reacting must be supplied at
least as fast, preferably
faster than the volatiles.
If one of the phases that
provide the buffer is com-
pletely consumed, then there
isn't a buffer any more.

Most of my discussion
concerns the second point,
the bulk composition con-
straint.

Figure 1 is a schematic
diagram of a very general
model for mantle-crust-
atmosphere interaction in any
earth-like planet — Earth,
Mars, Venus — during a time
when there is partial melting
with transfer of heat and
molten material from the
planet's interior to the
surface by volcanic proces-
ses. It is happening at the
present time on Earth, and I
think it very likely that
there are igneous processes
acting on Venus which also
bring fresh material to the
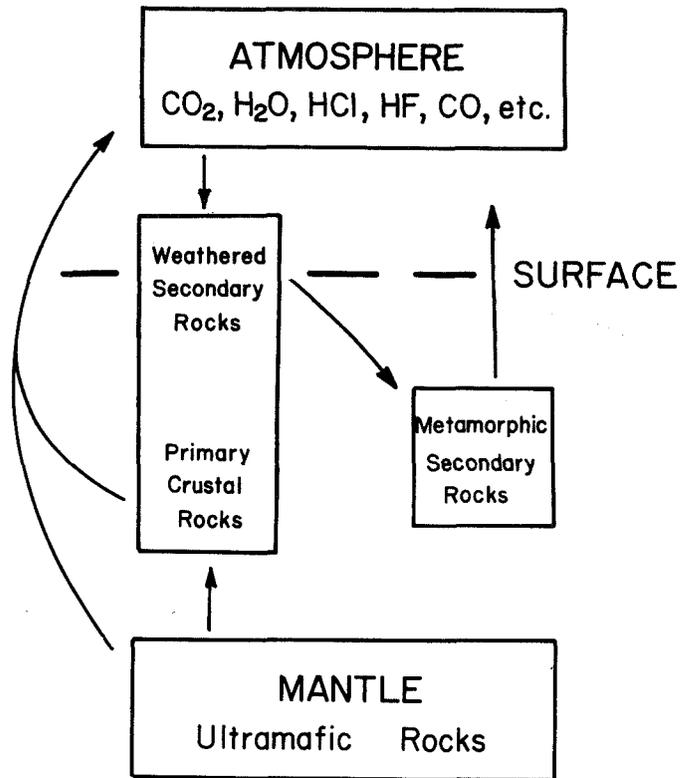surface. In Figure 1 these
are called "primary crustal
rocks".

Fig. 1. Schematic diagram of a general
model for mantle-crust-atmosphere inter-
action of any earth-like planet.

This material which comes to the surface, is hot compared to the normal
surface temperature and if there are volatiles in the melts they will be re-
leased because of the pressure decrease toward the surface. There may also
be volatiles moving from the mantle to the atmosphere independent of melting
processes, but that will just be an additional source of volatiles.

The composition range of primary crustal rocks, rocks which have been
molten at one time or crystalized from melt, is really quite restricted and
is not just a random mixture of oxide components. It has to do with low
temperature melting compositions.

Weathering can take place at the crust-atmosphere interface. It does
take place on the Earth; it could take place on Venus under appropriate
conditions. The weathering reactions will be reactions which add volatiles
to the primary crustal rocks. Therefore the weathered secondary rocks will
tend to be volatile rich. If appropriate bulk compositions of primary rock
are exposed at the Venus surface, if the surface temperature is low enough,
and if the activities of the volatiles are high enough, there will be a net
addition of volatiles to form weathered secondary rocks on Venus.

These weathered secondary rocks may remain at the surface or may be
carried to conditions of higher temperature and pressure by burial as thick
sedimentary deposits or by planetary-scale tectonic processes. The equi-
librium conditions for devolatilization reactions will cross any reasonable
geotherm at a large angle at depths of more than a few kilometers. This
means that as temperature and pressure increase with depth, H_2O-bearing or
CO_2-bearing minerals that may have formed in secondary weathered rocks will
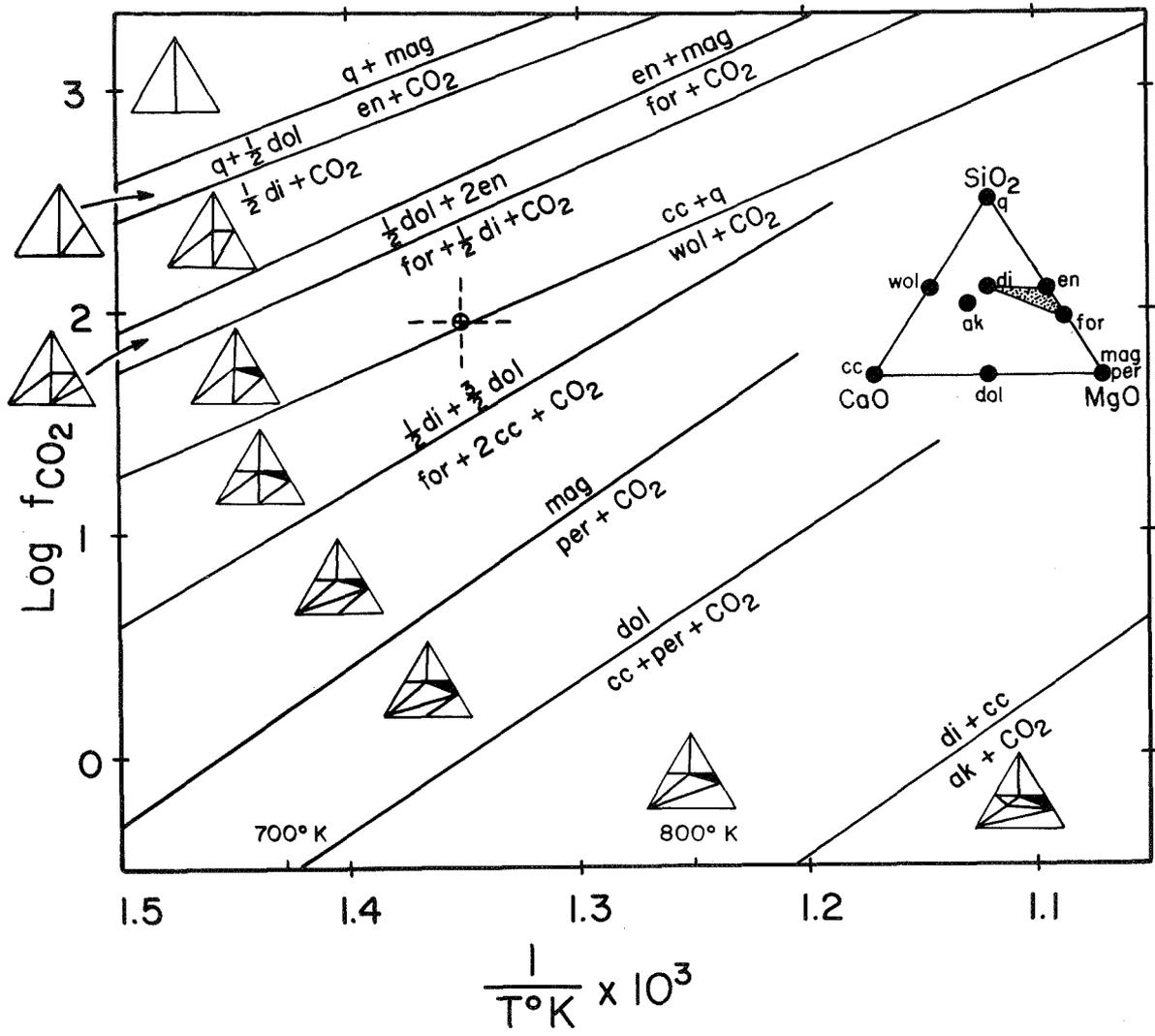break down and the volatiles may return to the atmosphere.

Fig. 2. Reactions with CO_2 in the system CaO-MgO-SiO_2. On the vertical axis is log fugacity CO_2, and on the horizontal axis the reciprocal temperature.

Some of these metamorphic secondary rocks, if one may call them that, may be carried back to the surface. The reactions which take place will tend to be the reverse reactions; if CO_2 was given off under the burial conditions, then when that material comes to the surface again it will take back the same amount of CO_2. The same thing holds for water.

The secondary metamorphic minerals produced on burial depend very much on what the initial weathered secondary rocks are. For example, there is no chance of getting andalusite or any aluminum silicate phase in the metamorphic secondary rocks unless aluminum-rich, water-rich clay minerals or micas were made in the weathering process.

To take a specific example we will consider reactions and reasonable bulk compositions of rocks which might buffer CO_2 on Venus. Figure 2 shows reactions with CO_2 in the system CaO-MgO-SiO_2 on a log fugacity CO_2-reciprocal temperature diagram. Bulk compositions of basic and ultrabasic rocks fall within the relatively small triangular area bounded by the compositions of diopside ($CaMgSi_2O_6$), enstatite ($MgSiO_3$) and forsterite (Mg_2SiO_4).

The surface pressure and temperature conditions from Venera 8, 741°K and 90 atmospheres CO_2 pressure, are indicated by a cross on Figure 2.

The small triangles in Figure 2 indicate stable mineral assemblages within each area of the diagram and the lines represent equilibrium conditions for reactions which, if the appropriate mineral phases were present, could act as CO_2 buffers.

At 741°K, the assemblages forsterite-enstatite-diopside (olivine-orthopyroxene-clinopyroxene) remains stable up to a CO_2 fugacity of several hundred atmospheres. This means that the mineral assemblage of basic or ultrabasic primary rocks would not be "weathered" to a CO_2-bearing secondary mineral assemblage until CO_2 pressures considerably higher than the approximately 100 atmospheres we now think likely.

There are a number of reactions, which, if the right mineral phases were present, would buffer CO_2 at considerably lower pressures. However, these minerals can be stable only in rock compositions outside the range of primary basic or ultrabasic igneous rocks. For example, a reaction equilibrium involving akermanite ($Ca_2MgSi_2O_7$) would hold CO_2 pressure at less than 1/10 atmosphere and a reaction involving periclase (MgO) would hold CO_2 pressure at about 10 atmospheres.

We can see in Figure 2 that there is a reaction equilibrium which passes right through the presumed Venus temperature-CO_2 pressure point. That's the calcite-quartz-wollastonite reaction. Although there is some uncertainty in the thermochemical data the question is not whether it's a good fit or not. I think the question should be, is wollastonite a reasonable mineral to find at the surface of Venus?

Wollastonite which occurs on the surface of the Earth is formed by metamorphic reaction between calcite and quartz, both minerals which are formed as weathering products of primary igneous rocks in the terrestrial weathering environment. There is no possibility of producing wollastonite as a primary phase by igneous processes on the Earth or, I would think, on Venus. Production of wollastonite by metamorphism releases one mole of CO_2 for each mole of wollastonite. "Weathering" of wollastonite at the surface could, at the most, remove from the atmosphere only the same amount of CO_2 which was released during metamorphism and could therefore not control the upper limit of CO_2 pressure. There is no point in considering other compositions of primary igneous rocks, granites or diorites, because there are no reactions involving CO_2 and primary phases of these rocks in the range of temperature-pressure considered here.

To summarize: At the Venus surface temperature at pressures of CO_2 greater than a few hundred atmospheres, there are "weathering" reactions which would involve the olivine-orthopyroxene-clinopyroxene mineral assemblage of common basic and ultrabasic rocks. These reactions would consume CO_2 and the weathered secondary rocks could store large amounts of CO_2 in the crust. The "buffering" of CO_2 provided by these reactions, however, would be at considerably higher CO_2 pressures than we think we have observed on Venus.

Table 2 is a set of reactions suggested by Lewis' Model c [Earth and Planet. Sci. Letters 10, 73, 1970]. The temperature is 747 K and the pressure 120 bars, for which there is a set of reactions which gives reasonable agreement to the observations of Venus' atmosphere. Reaction D4 which would buffer CO_2, calcite + quartz → wollastonite we have already talked about. I think we could just as well consider the atmosphere of Venus to be unbuffered, since the amount of CO_2 in the Venus atmosphere is about the same as the amount of CO_2 in the upper crust of the Earth; I see no real problem with that.
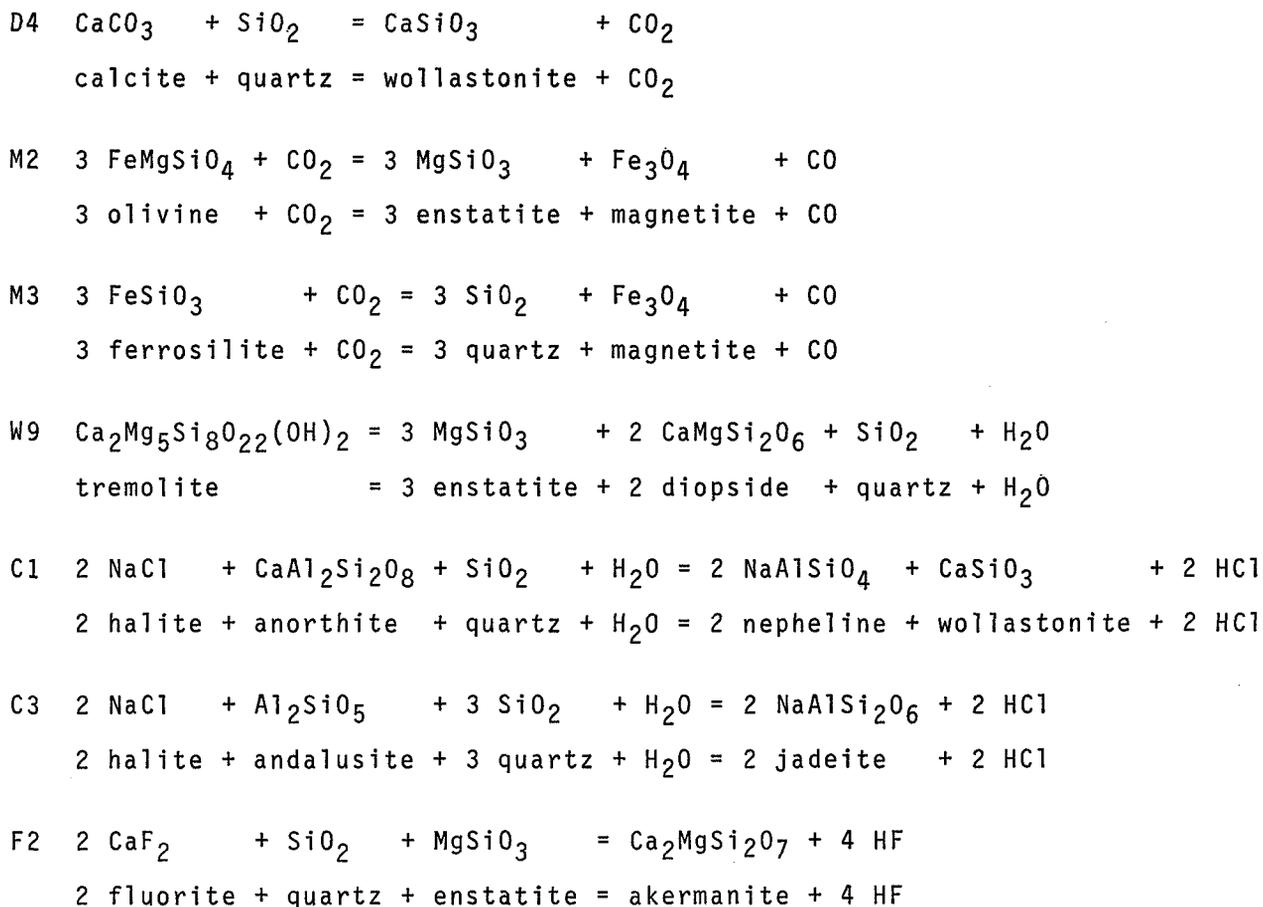
D4 $CaCO_3$ + SiO_2 = $CaSiO_3$ + CO_2

    calcite + quartz = wollastonite + CO_2

M2 $3 FeMgSiO_4$ + CO_2 = $3 MgSiO_3$ + Fe_3O_4 + CO

    3 olivine + CO_2 = 3 enstatite + magnetite + CO

M3 $3 FeSiO_3$ + CO_2 = $3 SiO_2$ + Fe_3O_4 + CO

    3 ferrosilite + CO_2 = 3 quartz + magnetite + CO

W9 $Ca_2Mg_5Si_8O_{22}(OH)_2$ = $3 MgSiO_3$ + $2 CaMgSi_2O_6$ + SiO_2 + H_2O

    tremolite = 3 enstatite + 2 diopside + quartz + H_2O

C1 $2 NaCl$ + $CaAl_2Si_2O_8$ + SiO_2 + H_2O = $2 NaAlSiO_4$ + $CaSiO_3$ + 2 HCl

    2 halite + anorthite + quartz + H_2O = 2 nepheline + wollastonite + 2 HCl

C3 $2 NaCl$ + Al_2SiO_5 + $3 SiO_2$ + H_2O = $2 NaAlSi_2O_6$ + 2 HCl

    2 halite + andalusite + 3 quartz + H_2O = 2 jadeite + 2 HCl

F2 $2 CaF_2$ + SiO_2 + $MgSiO_3$ = $Ca_2MgSi_2O_7$ + 4 HF

    2 fluorite + quartz + enstatite = akermanite + 4 HF

Table 2. Model "c" (Lewis, 1970) T = 747°K, P = 120.

Reactions M-2 and M-3 which would buffer oxygen involve phases in bulk compositions of rock that are reasonable for primary igneous rocks and it is quite reasonable to have a buffering reaction involving CO and the CO_2 ratio which in turn will determine the oxygen pressure.

Reaction W-9, a water-buffering reaction which involves quartz is a reasonable reaction for intermediate igneous rocks. There is some uncertainty about the thermochemical data also on the observations of water in the Venus atmosphere, but there is no reason why this might not be a buffer.

Reaction C-1 involves nepheline and quartz which cannot be stable together. It also involves nepheline and halite which will react to make sodalite, a much more stable phase under these conditions. The activities of sodium chloride, and HCl, must both be decreased by a factor of 10^3 or 10^4 if sodalite is present rather than nepheline plus halite.

Reaction C-3, involves jadeite, a high pressure, low temperature phase which cannot be stable anywhere in the Venus crust.

Reaction F-2, an HF-buffering reaction, involves akermanite, a phase which cannot be stable at the Venus surface conditions at CO_2 pressures of greater than .1 atmosphere. If the CO_2 pressure is greater than .1 atmosphere, akermanite will react to give diopside plus calcite. The CO_2 pressures are much higher than that, so this is not a geologically reasonable reaction to buffer the HF content.

DR. POLLACK: Since the amount of CO_2 in the atmosphere of Venus is comparable to the amount in the total system of the earth, would it be reasonable to assume that in effect there is no buffering?

DR. ORVILLE: Exactly. I think that is the case for CO_2.

However there may be buffering for other volatiles. There are some reactions which involve HCl and the minerals of a basic igneous rock which do control in about the right range.

DR. TUREKIAN: I gather that you are saying that for CO_2 there at least is a steady state concentration. Can you determine how much CO_2, or carbon in any form, is stored inside a system that is cycling with the atmosphere?

DR. ORVILLE: My guess is that little is stored.

DR. TUREKIAN: With regard to Sagan's earlier question, what would happen if a water-bearing phase were transported downward?

DR. ORVILLE: It would also tend to be released and there would be a limited storage capacity for water in the crust, corresponding to whatever the thickness of the weathered crust is. If that weathered crust were carried down by some tectonic process, the water would be released.

Acheson, Louis
Hughes Aircraft Corp.
Space & Communications Group
P. O. Box 92919
Los Angeles, CA   90009

Ainsworth, John E.
Code 623
Goddard Space Flight Center
Greenbelt, MD   20771

Anderson, Donald, Jr.
Naval Research Laboratory
Code 7121.9
Washington, DC   20375

Appleby, John F.
Dept. of Earth & Space Science
SUNY
Stony Brook, NY   11794

Arrhenius, Gustaf O.
Scripps Inst. of Oceanography
P. O. Box 1529/A-020
La Jolla, CA   92037

Avrech, Norman
Hughes Aircraft Corp.
Space & Communications Group
P. O. Box 92919
Los Angeles, CA   90009

Barker, Edwin S.
12103 Reardon Lane
Bowie, MD   20715

Barth, Charles A.
LASP
Univ. of Colorado
Boulder, CO   80302

Bauer, Siegfried J.
Code 620
Goddard Space Flight Center
Greenbelt, MD   20771

Baum, William A.
Lowell Observatory
Flagstaff, AZ   86001

Beebe, Reta F.
Astronomy Department
New Mexico State Univ.
Las Cruces, NM   88001

Belton, Michael J. S.
Kitt Peak National Observatory
950 N. Cherry Ave.
Tucson, AZ   85719

Berg, Nat
DoAll Eastern Co., Inc.
36-06 48th Ave.
Long Island City, NY

Bowell, Edward L. G.
Lowell Observatory
P. O. Box 1269
Flagstaff, AZ   86001

Brace, Larry
Code 621
Goddard Space Flight Center
Greenbelt, MD   20771

Burton, W.
SUNY
Stony Brook, NY   11794

Caldwell, John
Peyton Hall
Princeton Univ. Observatory
Princeton, NJ   08540

Cess, Robert D.
Dept. of Mechanics
SUNY
Stony Brook, NY   11794

Chahine, Moustafa
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Chylek, Petr
Dept. of Atmospheric Science
SUNY
Albany, NY   12222

Clarke, Victor
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Coffeen, David L.
Institute for Space Studies
2880 Broadway
New York, NY   10025

Colin, Larry
Pioneer Project Office
Ames Research Center
Moffett Field, CA   94035

Conrath, Barney
Code 622
Goddard Space Flight Center
Greenbelt, MD   20771

Danielson, Edward
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Danielson, Robert E.
Princeton Univ. Observatory
Peyton Hall
Princeton, NJ   08540

Dauky, Robert G.
SUNY
Stony Brook, NY   11794

Devaux, Claude
Universite de Lille
Lille - 59
FRANCE

Doan, Arthur S.
Code 644
Goddard Space Flight Center
Greenbelt, MD   20771

Dollfus, Audouin
Observatoire de Paris
Section d'Astrophysique
92-Meudon (Hauts de Seine)
FRANCE

Donahue, Thomas M.
Dept. of Atmos. & Oceanic Sci
Univ. of Michigan
Ann Arbor, MI   48104

Dow Gail E.
1618 Nostrand Ave.
Brooklyn, NY   11226

Erickson, Edwin F.
Space Sciences Division
Ames Research Center
Moffett Field, CA   94035

Elsis, M.
Harvard Univ.
Cambridge, MA   02138

Fahr, Hans
Institut fur Astrophysik
Universität Bonn
Auf dem Hügel 71
53 Bonn, W. GERMANY

Fellerman, Keith
Code 726
Goddard Space Flight Center
Greenbelt, MD   20771

Fels, Steven
GFDL - Box 308
Princeton Univ.
Princeton, NJ   08540

Fjeldbo, Gunnar
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Fymat, Alain L.
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Gierasch, Peter
Lab. for Planetary Studies
Cornell Univ.
Ithaca, NY   14850

Greyber, Howard D.
10123 Falls Rd.
Potomac, MD   20854

Gross, Stanley H.
Polytechnic Inst. of New York
Long Island Center
Farmingdale, NY   11735

Gun, S. H.
Polytechnic Inst. of New York
527 Atlantic Ave.
Freeport, NY

Hansen, James E.
Institute for Space Studies
2880 Broadway
New York, NY   10025

Hapke, Bruce W.
506 Langley Hall
Univ. of Pittsburgh
Pittsburgh, PA 15260

Hess, Seymour L.
Florida State Univ.
Meteorology Dept.
Tallahassee, FL 32306

Hogan, Joseph S.
Dept. of Mechanics
SUNY
Stony Brook, NY 11794

Hoffman, J. H.
Univ. of Texas
Box 30365
Dallas, TX 75230

Hunten, Donald M.
Kitt Peak National Observatory
950 N. Cherry Ave.
Tucson, AZ 85717

Ingersoll, Andrew P.
Geological Sciences
Calif. Inst. of Technology
Pasadena, CA 91109

Irvine, William M.
Univ. of Massachusetts
Dept. of Physics & Astronomy
Amherst, MA 01002

Jastrow, Robert
Institute for Space Studies
2880 Broadway
New York, NY 10025

Johnson, Jacques
Hughes Aircraft Corp.
Space & Communications Group
Bldg. 373/1110
Los Angeles, CA 90009

Kawabata, Kiyoshi
Institute for Space Studies
2880 Broadway
New York, NY 10025

Kawata, Yoshiyuki
Institute for Space Studies
2880 Broadway
New York, NY 10025

Kliore, Arvydas J.
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA 91103

Krauss, Robert
Univ. of Wisconsin
1225 W. Dayton St.
Madison, WI 53706

Kuriyan, Jacob G.
Dept. of Meteorology
UCLA
405 Hilgard Ave.
Los Angeles, CA 90024

Lacis, Andrew A.
Institute for Space Studies
2880 Broadway
New York, NY 10025

Landau, Robert
Astronomy Dept.
Univ. of California
Berkeley, CA 94720

Lane, Lesley B.
National Geographic
Washington, DC 20036

Lauletta, Anthony
Hughes Aircraft Corp.
El Segundo, CA

Lenoble, Jacqueline
Laboratoire d'Optique Atmosphere
BP 36, Lille - 59
FRANCE

Liu, S. C.
Dept. of Atmos. & Oceanic Science
Univ. of Michigan
Ann Arbor, MI 48104

Limaye, Sanjay
Univ. of Wisconsin
1225 W. Dayton St.
Madison, WI 53706

Mandeville, Jean C.
CERT/DERTS
2 Ave. E. Belin
31 Toulouse
FRANCE

McElroy, Michael B.
Atmospheric Sciences
Harvard Univ.
Cambridge, MA 02138

Nagy, Andrew F.
Dept. of Electrical Engineering
Univ. of Michigan
Ann Arbor, MI 48104

Nazarenko, Peter & Ruth
4034 Amundson Ave.
Bronx, NY 10466

Nelson, Robert
506 Langley Hall
Univ. of Pittsburgh
Pittsburgh, PA 15213

Niemann, Hasso B.
Code 623
Goddard Space Flight Center
Greenbelt, MD 20771

O'Leary, Brian I.
Hampshire College
Amherst, MA 01002

Orville, Philip M.
Dept. of Geology & Geophysics
Yale Univ.
New Haven, CN 06520

Owen, Tobias
Dept. of Earth & Space Science
SUNY
Stony Brook, NY 11794

Oyama, Vance I.
Ames Research Center
Moffett Field, CA 94035

Parks, R. Andrew
Hughes Aircraft Corp.
P. O. Box 92919
Los Angeles, CA 90009

Penner, Joyce
Harvard Univ.
Cambridge, MA 02138

Pirraglia, J.
Goddard Space Flight Center
Greenbelt, MD 20771

Pollack, James B.
Ames Research Center
Space Science Division
Moffett Field, CA 94035

Priester, Wolfgang
Institut fur Astrophysik
Universität Bonn
Auf dem Hügel 71
53 Bonn, W. GERMANY

Prinn, Ronald G.
Dept. of Meteorology
Mass. Inst. of Technology
Cambridge, MA 02139

Rasool, Ichtiaque
Code SL
NASA Headquarters
Washington, DC 20546

Rivas, Eugenia de
Dept. of Meteorology
Mass. Inst. of Technology
Cambridge, MA 02139

Roads, John
Dept. of Meteorology
Mass. Inst. of Technology
Cambridge, MA 02139

Rossow, William
Center for Radiophysics &
  Space Research
Cornell Univ.
Itaca, NY 14850

Sable, Claude
CERT/DERTS
2 Ave. E. Belin
31 Toulouse
FRANCE

Sagan, Carl
Center for Radiophysics &
  Space Research
Cornell Univ.
Ithaca, NY 14850

Sato, Makiko
Institute for Space Studies
2880 Broadway
New York, NY 10025

Scattergood, Thomas
SUNY
Stony Brook, NY 11794

Schubert, Gerald
Dept. of Planetary & Space Sci
Univ. of California
405 Hilgard Ave.
Los Angeles, CA 90024

Schurt, H.
Long Island Univ.
385 Flatbush Ave.
Brooklyn, NY

Seiff, Alvin
Ames Research Center
Moffett Field, CA   94035

Sill, Godfrey T. & Laurel
Lunar & Planetary Laboratory
Univ. of Arizona
Tucson, AZ   85721

Smith, Bradford A.
Dept. of Planetary Sciences
Univ. of Arizona
Tucson, AZ   85721

Smith, Harlan J.
Univ. of Texas
Austin, TX   78712

Smith, Lewis L.
Grumman Aerospace Corp.
Research Dept. - Plant 26
Bethpage, NY   11714

Somerville, Richard C. J.
P. O. Box 3000
National Center for Atmos. Research
Boulder, CO   80303

Sommer, Simon C.
Ames Research Center
Moffett Field, CA   94035

Spar, Jerome
City College of New York
138th St. & Convent Ave.
New York, NY   10031

Stewart, A. Ian
LASP
Univ. of Colorado
Boulder, CO   80302

Stief, Louis J.
Code 691
Goddard Space Flight Center
Greenbelt, MD   20771

St. Maurice, John
Yale Univ.
New Haven, CN   06520

Stone, Peter
Dept. of Meteorology
Mass. Inst. of Technology
Cambridge, MA   02139

Suomi, Verner E.
Space Sci. & Engineering Cntr.
Univ. of Wisconsin
1225 W. Dayton St.
Madison, WI   53706

Sze, Nien-Dak
Cntr. for Earth & Planetary Phys
Harvard Univ.
Cambridge, MA   02138

Taylor, Fredric W.
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Tokunaga, Alan
SUNY
Stony Brook, NY   11794

Tomasko, Martin G.
Lunar & Planetary Laboratory
Univ. of Arizona
Tucson, AZ   85721

Traub, Wesley A.
Center for Astrophysics
60 Garden St.
Cambridge, MA   02138

Travis, Larry
Institute for Space Studies
2880 Broadway
New York, NY   10025

Turekian, Karl
Geology Dept.
Yale Univ.
Box 2161-Yale Sta.
New Haven, CN   06520

Walker, James C. G.
Arecibo Observatory
P. O. Box 995
Arecibo, PR   00612

Weaver, Kenneth
National Geographic
Washington, DC   20036

Wofsy, Steven C.
Pierce Hall
Harvard Univ.
Cambridge, MA   02138

Woo, Richard
Jet Propulsion Laboratory
4800 Oak Grove Dr.
Pasadena, CA   91103

Young, Andrew T.
Texas A&M Univ.
College Station, TX   77843

Young, Richard E.
Space Sciences Division
Ames Research Center
Moffett Field, CA   94035

Yung, Y.
Harvard Univ.
Cambridge, MA   02138

*"The aeronautical and space activities of the United States shall be
conducted so as to contribute . . . to the expansion of human knowl-
edge of phenomena in the atmosphere and space. The Administration
shall provide for the widest practicable and appropriate dissemination
of information concerning its activities and the results thereof."*
—NATIONAL AERONAUTICS AND SPACE ACT OF 1958

NASA SCIENTIFIC AND TECHNICAL PUBLICATIONS

TECHNICAL REPORTS: Scientific and technical information considered important, complete, and a lasting contribution to existing knowledge.

TECHNICAL NOTES: Information less broad in scope but nevertheless of importance as a contribution to existing knowledge.

TECHNICAL MEMORANDUMS: Information receiving limited distribution because of preliminary data, security classification, or other reasons. Also includes conference proceedings with either limited or unlimited distribution.

CONTRACTOR REPORTS: Scientific and technical information generated under a NASA contract or grant and considered an important contribution to existing knowledge.

TECHNICAL TRANSLATIONS: Information published in a foreign language considered to merit NASA distribution in English.

SPECIAL PUBLICATIONS: Information derived from or of value to NASA activities. Publications include final reports of major projects, monographs, data compilations, handbooks, sourcebooks, and special bibliographies.

TECHNOLOGY UTILIZATION PUBLICATIONS: Information on technology used by NASA that may be of particular interest in commercial and other non-aerospace applications. Publications include Tech Briefs, Technology Utilization Reports and Technology Surveys.

Details on the availability of these publications may be obtained from:

SCIENTIFIC AND TECHNICAL INFORMATION OFFICE

NATIONAL AERONAUTICS AND SPACE ADMINISTRATION
Washington, D.C. 20546