RAREBK
QC
879.4
.C66
1959

Conference on Infrared Measurements from Satellites.

Transcript of the May 1, 1959 conference.

109 892

UNITED STATES DEPARTMENT OF COMMERCE
WEATHER BUREAU
WASHINGTON

CONFERENCE ON INFRA-RED MEASUREMENTS FROM SATELLITES.

Transcript of the May 1, 1959 conference

U. S. Weather Bureau, Washington, D. C.

Friday, May 1, 1959

Agenda

I. Introduction — Sigmund Fritz

II. Theory

A. The ideal requirements of measurements of atmospheric parameters from satellites without regard to practical limitations. Fifteen minute presentations of key theoretical requirements regarding:

1. Water vapor -- G. M. Plass
2. Temperature from carbon dioxide measurements; Ozone measurements -- L. D. Kaplan
3. Temperature -- J. L. King
4. Heat Budget -- D. Q. Wark

B. Questions which have come up so far in the determination of meteorological parameters include:

1. What spectral and angular resolutions are needed?
2. What are the viewing angle requirements?

III. Instrumentation

A. Discussion of:

1. What is currently available to meet the requirements indicated by theory?
2. The outlook for the near future in improved instrumentation.

B. Consideration of:

1. Spectral and angular resolution requirements.

IV. Combination of theory and instrumentation

A. What compromise between available instrumentation and theoretical requirements must be made?

B. What do measurements, made with available instruments, mean in terms of atmospheric parameters?

RAREBOOK
QC
879.4
.C66
1959

LIBRARY
JUL 20 2009
National Oceanic & Atmospheric Administration
U.S. Dept. of Commerce

National Oceanic and Atmospheric Administration
TIROS Satellites and Satellite Meteorology

ERRATA NOTICE

One or more conditions of the original document may affect the quality of the image, such as:

Discolored pages
Faded or light ink
Binding intrudes into the text

This has been a co-operative project between the NOAA Central Library and the Climate Database Modernization Program, National Climate Data Center (NCDC). To view the original document contact the NOAA Central Library in Silver Spring, MD at (301) 713-2607 x124 or Library.Reference@noaa.gov.

HOV Services
Imaging Contractor
12200 Kiln Court
Beltsville, MD 20704-1387
January 26, 2009

PARTICIPANTS

Name	Organization
Stan Greenfield	Rand Corporation
Lewis Kaplan	Massachusetts Institute of Technology
G. Plass	Aeronutronic Systems
Richard Goody	Harvard University
David M. Gates	National Bureau of Standards, Boulder, Colorado
Walter M. Elsasser	University of California, La Jolla
M. Tepper	National Aeronautics and Space Administration
John N. Howard	Geophysics Research Directorate, AFCRC
Arthur Adel	Atmospheric Research Observatory
David Q. Wark	US Weather Bureau
John Shaw	Ohio State University
David S. Johnson	US Weather Bureau
Sigmund Fritz	US Weather Bureau
Jean I. F. King	General Electric
R. Bowling Barnes	Barnes Engineering Company
W. F. Staats	US Weather Bureau
W. G. Stroud	National Aeronautics and Space Administration
R. C. Curtis	Geophysics Research Directorate, AFCRC
E. M. Wormser	Barnes Engineering Company

CONFERENCE ON
INFRA-RED MEASUREMENTS FROM SATELLITES

Summary

A meeting of experts in the theoretical and instrumental problems of atmospheric infra-red measurements was held at the U. S. Weather Bureau, Washington, D. C., May 1, 1959. Those on the accompanying list were in attendance.

Dr. Sigmund Fritz, chairman, outlined briefly the purpose of the meeting, which was to obtain guidance in planning programs for satellite measurements in the infra-red. The need for this was emphasized by discussions at the meetings of the Space Science Board-Committee on Meteorology and of the Advanced Research Projects Agency-Committee on Meteorological Satellites. He outlined the TIROS satellite instrumentation and indicated that subsequent satellites would probably be stabilized and would be capable of carrying about 500 pounds, with a payload length up to about six feet.

Theoretical Requirements

The theoretical requirements without regard to satellite instrument limitations were discussed. The first speaker, Dr. Plass, discussed a simple isothermal atmosphere, without clouds, dust or earth, and considered the radiation one would see coming out. He then outlined the behavior of a dimensionless quantity which is dependent on line strength and concentration. He showed how the quantity changed with concentration and indicated that a measurement of total water vapor amount might be possible; however, the determination of the distribution of water vapor would be very difficult and would require extremely accurate measurements. He went on to explain that complications due to a non-isothermal atmosphere and the presence of the earth and clouds added to the difficulties.

The second speaker, Dr. Kaplan, discussed a method by which the temper-

ature distribution in the atmosphere could be determined from measurements made in several carefully selected narrow wave-length intervals in the 15 micron carbon dioxide band. He mentioned a number of details of that band and outlined most of the theory and the requirements for the calculation of outgoing radiation for clear skies and for various overcast cloud conditions. In order to determine temperature distribution, a resolution of about five wave numbers in the 15 micron carbon-dioxide band would be needed. He expressed his belief that ten temperatures through the atmosphere,with an accuracy of one degree, could be found from ten measures in different parts of the band.

Kaplan next discussed Goody's ground measurements of downcoming radiation and pointed out that the only real window is the eight wave number interval at 11.1 microns. He went on to discuss the measurement of water vapor and pointed out the advantages of the 25 micron region over the 6.3 micron band in determining concentration.

He then discussed the 9.6 micron ozone band, mentioning the relative transparency of the band, its low temperature dependence, the details and the behavior of the band, some figures on ozone concentration and the limitations in ozone distribution determinations using this band. Resolutions of the order of 2 wave numbers would be needed in this band if several intervals were examined.

Dr. King spoke next. He described the general principles of limb-darkening and showed how the intensity of an emergent beam varies with angle for a given temperature distribution. He suggested that an inversion of this process might be possible. He explained, then, the shortcomings of the inversion technique. Then, using the ERTOR measures of Adel, he went through a method of determining the vertical temperature distribution for the ozone region. The results, he found, were not encouraging. Contaminating water vapor radiation was a serious factor in introducing errors in the results; small errors of measurement upset the results. He indicated that looking down from above would be an improvement. He felt that

the entire problem should be examined more closely to check the seriousness of the indeterminacy.

Dr. Wark began the afternoon session with an outline of the infra-red heat budget. He pointed out that the only quantity directly observable from a satellite is the upward intensity from the top of the atmosphere. To determine the heat budget of the atmosphere alone, one would have to calculate the upward and downward radiation at the surface from a model of the atmosphere inferred from the upward intensity. This means that details of the atmosphere must be determined by a method such as that proposed by Kaplan.

To determine the planetary heat budget one would have to develop a means of getting the flux, or intensity integrated over all directions in a hemisphere, from a single intensity measurement taken at some arbitrary angle. This would probably best be done through empirical relations between intensity and flux. He then showed the filter characteristics of some sensors to be used in TIROS and explained the difficulty in inferring the heat budget from measurements made through them. He concluded by showing the results of some calculations of intensity; these showed the regions of the atmosphere from which the emission arises for various parts of the spectrum.

During and between the talks there was a great deal of discussion on a wide variety of topics relating to satellite observations. A summary of the requirements is shown in the table:

Atmospheric Quantity	Spectral Region of Measurement (Microns)	Resolution Required ($cm.^{-1}$)	Number of Intervals in Band to be Measured
Water vapor	25	10 to 25; perhaps 2	?
Ozone	9.6	1 to 2	5
Temperature	15	5	10
Heat budget	4-100	broad	—

Comments by Howard dealt with the amount of energy available in 6.3 and 25 micron bands and on the resolution to be used. He felt the 6.3 micron region should not be abandoned too quickly.

The relative merits of several detectors was discussed by Goody and Shaw. The positive superiority of the grating spectrometer over a filter system to isolate a region of the spectrum, to compare regions and to get details was brought out by Howard, Goody and others. It was pointed out that the principal problem lay in the detectors, not in the spectrometer; that spectrometers of the type envisagedhave been built; that ultra-violet spectrometers have been flown in rockets; but no decision was made regarding prism spectrometers.

Dr. Goody next showed several slides. The first was a grating spectrograph and a solar spectrum, with about 5 cm^{-1} resolution and therefore no detail. Next, a prism spectrograph with a resolution of 15 cm^{-1}, the spectrum showing only broad features. Response time was 1 second with ~~noise~~ signal to noise ratio about 100, with scanning time 45 seconds. He indicated that indium antimomide filters bloomed with lead chloride can give transmissions up to 98%, for use with grating spectrographs. Another instrument of 1/2 cm^{-1} resolution was shown. He then showed an instrument with a 6 X 6 inch grating, 5 feet long, with a 6° angle of view and using a Golay cell detector; it was suggested that this sort of instrument might satisfy the needs put forth in this meeting. This instrument has a 1 cm^{-1} resolution and a 1 second time constant; the time constant and the grating size are the things which can be adjusted. He showed a slide of a spectrum, with a resolution of 1 cm^{-1} and a time constant of 1 second.

A discussion followed on the merits of scanning with a single detector, rather than having several fixed detectors; on the time constant requirements; on the type of detector; and on the relative merits of observing in the 6.3 and 25 micron regions for water vapor determination. No final decisions were made on any of these questions, although it was indicated

that fixed detectors might be used; a time constant of 1 second was satisfactory; a thermal detector was probably best; and that 25 microns is a better region in which to work.

Fritz then remarked that present instrumental capabilities seem to be within the range of theoretical requirements. In the ensuing discussion, however, the problems in determining the heat budget were brought out; it was pointed out that empirical relations must be established, in the case of TIROS, to translate individual measures into flux; further theoretical work should be done, but spectral detail would solve much of the problem.

The following topics were then brought up: a grazing incidence observation of the sun, which would be useful but very difficult; measuring ozone amounts from mean surface temperatures over the ocean, which was not thought feasible, but which should be looked into; problems in scattered light in the ultra-violet; and the use of balloons and rocket soundings.

King then raised some questions about the ability of the experiment proposed by Kaplan to determine atmospheric temperatures and also to determine water vapor distribution.

The meeting concluded with a résumé of the points brought up and the conclusions reached.

INFRA-RED MEASUREMENTS MEETING

MAY 1, 1959

Introduction

Dr. Fritz: To begin with, I though I might outline some of the things that have happened and maybe give you a clear idea why this meeting is necessary. Of course, meteorologists are interested in sampling the atmosphere as thoroughly as they can and the satellite is sort of a natural instrument with which to make these observations. Organizationally, the NASA is in charge of the national meteorological satellite observation program and has designated the Weather Bureau as its meteorological agent. As a matter of fact, we now have in the Weather Bureau a Meteorological Satellite Section which is growing rather rapidly which is financed entirely with NASA funds. It is in this framework that we felt that it is necessary to have guidance from the experts in planning programs for satellite measurements in this field. I felt that we had here a very good group who can tell us about both the theory and instrumental capabilities and requirements. Of course, among the things that we would like to measure are, obviously, temperature, atmospheric composition, in particular water vapor, ozone and perhaps CO_2 would be of interest. It is one thing to say that we would like to do it and quite another thing to say just how we do it. The present status of satellite measurements is that we have a fairly good program in progress now, called project TIROS. This is a fairly large satellite on which there will be more than one television camera and several radiation sensors. The hardware for this is far along in production now, some of it is already produced, and the satellite may be launched for example toward the end of this year.

Goody: Could you give us an idea of sizes and weights, please?

Fritz: That is mostly classified, Dick. I think for planning purposes here we can say what we can expect, but on that package I just can't tell you. The name is project TIROS, maybe meaning Television Infra-Red Observations Satellites. As I said there will be some television cameras and there are five radiation sensors which I will recite. There is one that has a transmission in the region from .2 to 6 micron which is essentially the solar radiation reflected by the earth. The other one is from 7.2 to 35 microns which is essentially the terrestrial radiation emitted by the earth with certain limitations.

Question: What are the sensors?

Fritz: They are thermistor elements and they have various filters in front of them in lenses. There is a window measurement from 8.5 to 11.5 microns. There is another measurement which is parallel to the TV in a way from .56 to .75 microns which will measure cloud cover, say in the southern hemisphere. And something which is related to water vapor in 5.8 to 6.8 microns. In addition, there are two other sensors with bands similar to the first two. Now these five that I mentioned are mounted at 45° to the axis of the satellite which will be spinning. Each sensor has a 5° field of view and so you will sweep out an area on the earth.

Goody: Is this telemetered back?

Fritz: No, it is stored on tape and transmitted back on command. Once each orbit, you can send the information back. There will be some orbits where the information cannot be read out. There are not enough read-out stations to read out all the orbits. The satellite will have about a 51° inclination to the equator. I think in this case we just get one orbit. Isn't that right?

Fritz: It erases. If you don't read out, the orbit information is erased and it reads the next one. You can only store for one orbit. You won't get complete information, but this is a technicality I guess. I am merely bringing this up to show you what is being done now. For the future, I think that we can expect some modification. For one thing, say a year after this one is launched, or something like that, there will be a stabilized satellite. Namely, one that looks continuously down at the earth and will not be spinning. And of course you can mount sensors on this one at any angle to the axis, the figure axis of the thing.

Goody: When do you hope to have this?

Fritz: Say a year or so after TIROS is launched which might be two years from now or something like that. Now weight capabilities that we should be thinking of are something like say 500 pounds at 500 miles. We can plan on that kind of a framework. This is instrumental payload including the necessary power supplies.

Goody: Approximate length or size?

Johnson: I would say that we might be able to handle something as long as six feet. This is still debatable.

Goody: Well 500 pounds in 2 meters is essentially all any of our equipment would require.

Fritz: Well I would say that in our thinking here today we can assume that this will be available. These numbers are merely to give that kind of a feeling. This should not be considered a serious limitation if you can work within that kind of a framework. These are the state of the art and something a little bit about the future as far capabilities are concerned. We have to turn now to the question of what we want to measure and how to

measure it. There are a great many problems in these things. For example, communications - how do you get the stuff down. Data reduction, data analysis - these are formidable when you deal with large quantities of data as we will inevitably have to do. I think we should set all the questions aside today because those are topics for a completely separate meeting. And I think today we should just consider the optical system. What are the optics that you need and perhaps this includes detectors too? Forgetting about how you would get it down, communications-wise and how you would analyze it. If we can get anywhere with this question, what optics do we use to solve the problem. I think we would have already made substantial start.

Question: We are not limited by payload at this time?

Fritz: I think you can't figure on 50 tons, but I mentioned 500 pounds as really just an order of (magnitude?).

Tepper: I think Goodys' point is that It really doesn't matter. We can do everything we want to.

Fritz: But Morris feels that we can carry a ton or two tons, or whatever you want, but I think he is right, we shouldn't consider that a serious limitation, especially since 500 pounds is anyway enough.

Kaplan: Also you might want to do several things at one time.

Fritz: Yes, that's a possibility.

Elsasser: One question. Do we expect that all these satellites will rotate?

Fritz: I don't understand, do you mean that it will rotate about it's own axis?

Elsasser: Yes.

Fritz: Well we can make it rotate if we want it to, but we can also stop it from rotating if we want to, and I think our plan is that we will not have it rotate.

Elsasser: I am not so sure that we can stop it from rotating.

Fritz: Why not? They say they can.

Goody: This thing will be doing this, won't it?

Fritz: Yes, there will be jets or fly wheels, or things of that sort which will stop it from spinning or rotating, if you want it to, but if you want it to spin, you can make it spin. You can put jets on it for example which will make it spin. Let me ask you this, do you want it to rotate or don't you want it to rotate.

Elsasser: It will be a darn sight better to have it rotate so you can calibrate it against vacuum, because you have the ideal calibration here.

Goody: I think we should mention here that the second satellite of yours is a directive satellite. It points toward the middle of the earth.

Fritz: That's what I meant.

Goody: So you look out one end and you see the earth, you look out the other end and you see space. You don't need to spin it, you can do it optically with the gila-system.

Elsasser: Well you can keep it that way.

Goody: That's the intention. This is very valuable I think.

Fritz: I think if for any reason we don't want to do it one way, we can do it either way, that is, you can have it pointing directly towards the center of the earth without rotating or spinning in any way, or you can have it spin or rotate or anything you want, so that again is something we can use as a frame work. Now I think we should start the program. What I had in mind initially in sending this out to the people involved was that we could start with some sort of theoretical guidance as to what we need to measure water vapor, ozone, carbon dioxide, temperature and the heat budget without regard to any practical limitations. With this I really had myself in mind. Although I don't know if this needs to be considered. I had in mind no more limitations than you would have

for measuring something on the surface of the earth. Maybe we shouldn't even consider those limitations. Without any regard to any limitations, what does theory demand of us in order to be able to measure these things. I hope that the people who are listed on the program will speak something like 15 minutes. I don't think that we should consider this a scientific session in which we have to prove completely to everyone's satisfaction what they are saying. But if they give us a brief, very brief, guide lines as to how they arrived at their results, and then come up with a definite recommendation as to what they think is required in order to measure these things. This would give us the framework about which we could then proceed to the next question. What instruments are now available to meet these requirements? Now, obviously, I think in every case we will not be able to supply the instruments to give the theoreticians what they say they need. We will have to consider then, what is available in the way of instrumentation and then return to the universe question. With the instrumentation that is now available what will theory enable us to deduce from the measurements in terms of atmospheric parameters. This is quite a big project but I still feel that we might make an attempt to finish this today. We have made an attempt here to record the proceedings and I hope that it will make enough sense so that we can actually reproduce it and make it available to other than this small group. There is a microphone at the end of the table for people who give their formal talks, and if they would talk into that thing it should work all right. For others, these mikes here will, I understand, will pick up everything around the table. We may have to hand the hand mike around. We will now hear a discussion by Gilbert Plass on Water Vapor. What are the theoretical requirements to measure water vapor from the satellite?

Plass: It has been pointed out by many people that if you could measure with infinite accuracy, the emission at an infinite number of frequencies then you could find out all you wanted to know about the distribution of water vapor in the atmosphere and the temperature distribution. Now obviously we can't do this, so the question is what is practical to do.

In order to look at this problem a little bit, I would like to first consider an idealized situation. Supposing we were in a satellite above the earth. We could look down on the earth and somehow or another we could measure merely the radiation that comes from the water vapor and we wouldn't see any of the radiation that comes from the surface of the earth, the clouds or the dust in the atmosphere, or any of these other things that make life more difficult. So for the moment let's just ask what we would see if we could just look at this water vapor radiation by itself. Supposing we measured the radiation that was coming out, in a number of different frequency bands, and we plot our results. I would like to plot the logarithm of the intensity that we observe, make this a dimensionless quantity, let me divide this by the black body intensity at the temperature that is appropriate to the top of the atmosphere where say the pressure is zero. Then the largest I can ever be is the quantity IB, ~~can be~~ so the limiting value here is where this ratio is one and the lagarithum is zero. Now supposing I plot the different' points that I get in different frequency bands as a function essentially of the strength of the lines in that band. I like to use a dimensionless quantity which I call gamma. This is defined to be the intensity of the lines times the concentration of the gas times the pressure times the secant of the angle, where theta is the angle to the vertical, divided by two pi times the half width of the lines times the acceleration of gravity. And if the concentration is varying with height in something like water vapor, these quantities are to be evaluated at the

surface of the earth. You can choose any standard level you want. But the important thing to notice is that gamma is proportional to the strength of the line through this line strength quantity S. It is also proportional to the concentration. Now if we have the water vapor distributed in some arbitrary way, and we make arrangements in a number of different frequency bands so we get different line strengths we're going to get some curve here. This curve has several distinctive parts. If you first of all look at the very weak lines where the absorption is very small at all frequencies in the band that you are looking at you will then get just a straight line here which will have a slope of unity and this is what is often referred to as the linear region. As you go to stronger and stronger lines, actually the division comes roughly where gamma is unity, you find that this curve bends over and there may be a portion where the slope is one half and this is called the square root region. Physically of course, the thing that distinguishes the two regions is that here the centers of the lines are black and you are getting the wing absorption which is determining how this goes up and then eventaully as you go to still stronger lines this curve will bend over and approach its limiting value and somewhere in here the lines will begin to overlap and this is what eventually causes the curves to bend over. Depending on how close the lines are together, there may or may not be a square root region and the lines could start overlapping somewhere down here. There isn't a linear region either (unintelligible) with constant concentration you never have a linear region, except that (unintelligible) a Lorentz line.

Plass: No, that's not true. You have a linear region when gamma is less than one with constant concentration.

Reply: I don't think so.

Plass:
So this might be a typical sort of curve you would observe for a given amount

of water vapor. The question is how will this curve change as we change the various parameters we are interested in. The simplest thing is to ask how it changes if you change the total amount of water vapor and that is essentially this factor C here, and here you would find that it would vary as the concentration in this region, it would vary as the square root of the concentration in this region, it would vary as the concentration to some power K, where K is less than one half in this region. And this indicates that it perhaps is not to difficult to measure the total amount of water vapor. The difficulties of course are that in the linear region, often the emission is so small that you can't really measure it very well, but perhaps you can find a region up here where it doesn't vary quite so rapidly with the concentration and in that way determine the total amount of water vapor. How do these curves vary when we actually change the distribution of water vapor? If we now plot a number of these different curves, for different distributions of water vapor but keeping the total amount of water vapor constant, we find first of all that the curve is absolutely identical down in this region. When you are in the linear region it doesn't make any difference how the gas is distributed because there is no appreciable absorption of the radiation that is emitted by one layer by another layer. At the moment I am considering an isothermal atmospheric.

You get different curves when you come up in this region and for some other distribution you might get a different curve which would come up in some other fashion. How much can these curves vary and how easy is it to get information about different distributions? One can actually show that these curves can never vary by more than 41%, actually their ration can never be more than the sq. rt. of 2. This is taking the two most extreme distributions that you could possibly take,,

neither one of which will ever occur in nature. These two distributions are first of all having all your water vapor at the surface of the earth compared to the case where the water vapor is uniformly distributed through the whole atmosphere. Neither one of these actually occurs. So in nature you are never even going to get much of a variation between these two curves. I think perhaps you would be lucky to get a ten or 20 per cent variation in different water vapor distributions. I am now always talking about keeping the total concentration C constant.

Now in addition, there is the question of what happens as the temperature changes with height. If you calculate or measure curves of this sort where you have different temperature distributions with height you then will also get somewhat different curves, but again you always have these same regions. If we had a temperature variation with height we might get a curve for example that would look like this and it might approach a different limit up here if we had a different temperature distribution. There are a few such curves calculated in an article which I have in the Quarterly Journal of the Royal Meteorological Society in 1955. Actually, how much of a variation here you could get for different temperature distributions I don't know. Maybe some of the other speakers can give more information. At least for the few cases I tried, you again don't get more than about a 20 or 30% distribution at most for different temperature distributions. So the main thing I would like to point out is that even if we could do this very idealized experiment that it is relatively easy to find out the total amount of water vapor but that it looks as though very accurate measurements would be necessary to get any information at all as to the distribution. Actually the situation, of course, is not nearly this simple, because we can't sort out the radiation from the ground and the radiation from the cloud tops. If you then ask what would be the total radiations you would actually see at

the top of the atmosphere, and we made a similar plot here of the intensity over some normalizing intensity there will be two limiting values. If for example we are looking down on a clear day when there are no clouds, for very weak lines we will just see the black body radiation from the surface of the earth, which will be some value appropriate to the temperature of the earth and this will be the largest amount of radiation we will ever see. On the other hand, if you look in a region where there are very strong lines we will see black body radiations appropriate to the stratospheric temperature. This will usually be a much lower temperature and will give us some lower limit to our emission unless there were some very unusual inversion conditions or something like that. If we again plot this as a function of some parameter which varies as the line strength, for the very weak lines of course we will see the radiation from the surface of the earth, this curve will come down and approach the black body curve for the stratospheric temperature for the very strong lines. Here this curve starts down in a linear deviation from this limit corresponding to this curve, but the trouble is that it is only a very small deviation from the total black body radiation. This is probably the region where it is easiest to measure devaitions and then eventually the curve approaches this limit. I haven't made any detailed calculations about water vapor. I'm afraid I don't have any numbers to present, I just wanted to give these general ideas here.

(Question) Where did you say the cloud tops would be in this graph?

Plass: If you had cloud tops then you would just get some different limit here. If your sky were completely cloudy then you would get some limit appropriate to the temperature of the cloud tops.

Dr. Fritz: Do you have any feeling at all for what wave length and what resolutions you would need within the particular (wave-length?) measurements?

Or is it something that you haven't thought about? Maybe Lewis can tell us a little about that?

Plass: I really haven't thought about it.

Gates? We know pretty well where the strong lines are and we know where the weak lines are.

Goody: Yes we have a good selection, there is no problem there.

Fritz: What would be the width of a band or measurement? The wave length width and the measurement that you would need?

Goody: There are essentially three significant wave lengths bands. I think Lewis will have more to say about this, but either you take an average of a selection ofclose lines in which case you will want to have a range which will be at least 25 wave numbers, or you will go to a range which will include just one line, so you know which line you are talking about, and that's on the average of about 2 wave numbers, or you could resolve this line, in which case you want about 100th of a wave number. That's impossible.

Fritz: The other two that are possible, would you use the same in the strong lines as in the weak or would you change this?

Elsasser: Are you thinking actually in terms of measuring individual lines or merely a region where you have individual strong lines.

Goody: Individual lines?

Elsasser: Individual lines, then you would put actually a sprectroscope with long tongs out there?

Fritz: That is an instrumental detail which maybe we should talk about. In other words, the theoretical requirement would ask that you measure things down to two wave numbers.

Goody: This weight and size is easy to do that.

Fritz: Do we have any idea how many wave lengths we would like to look at?

Goody: I think we ought to hear Lewis' contribution. Can I make a general remark about water vapor? Putting a few numbers into this we have when stopping to think of what one can sensibly obtain by other means, there are ways of knowing the total amount of water vapor up to about 15 kilometers, one can measure it very precisely from aircraft we know quite a lot about this.

Fritz: I would like to mention that this is world wide and it is difficult to do things without a satellite on a world-wide basis.

Goody: Well it has been pretty good coverage between tropics in African and polar region by aircraft, it's really quite easy to go backwards and forwards in a jet.

Fritz: You say there is good coverage now? Is it being done today? I think one mightmight think in satellite use for example, were it ever to become operational let us say, you might want to have measurements every day, and over essentially the whole world. So it becomes difficult to do it with anything but satellites.

Goody: You may get certainly better results that way. But just let's remember what we can do. We can do it from 15 kilometers, how accurately, with not too serious difficulties from jet aircraft, you can't do it over enemy territory or something like this. Point number two. These water vapor concentrations have pretty good correlations with temperature. Very high degree of correlation. If you take partial regression coefficients you get a very good statistical account, so that in fact if you merely have the climatology or the measured temperature then up to perhaps the lower stratosphere you can get pretty good information on the amount of water vapor and this is the datum against which one has to judge the measurements. It;s no good to set up a measurement which doesn't do as well as this, because one can do this well already, so that the key in all this, it seems to me, is, are these satellite measurements going to give us anything information which is badly observed

which is to say push it down to it's lower limit, which is say about 10 kilometers. So let's ask a question, can we say anything about the water vapor above 10 kilometers, rather then the total amount? Now you say that you have got to deal with an amount which is approximately 1,000th of the total. If you are going to do this looking downward to the earth you are measuring an amount of water vapor which is 1,000th of the total. Now two things about it. First of all as to how you measure it. You can measure it by either looking at the emission spectrum as you've been suggesting or by looking at the absorption spectrum. This is an old technique. But the absorption spectrum has an enormous advantage, namely that it doesn't involve the thermal properties of the atmosphere where as this emission does. And so I think if one were measuring the total quantity, one would immediately, without any hesitation, choose absorption not emission as the way to measure.

Frits: Does this require a source?
Goody: To put it simplest you would simply measure spectrograph in the visible and
Goody: So I feel ~~that in this case one would~~ go
take the reflected light off the earth, or off the clouds, the reflective solar radiation, rather then the emitted radiation. So I feel that in this case one would go immediately to the non thermal region and in any case as I say with either method you would have to measure one part in 1000, as I think, to get any useful meteorological information and you can't do that. That is really all there is to it. So that I would say that there is extremely little useful information on water vapor began by looking down except, the one thing, and that is to tell us how much radiation comes off it. In other words, forget what information it tells you about the water vapor and talk about in terms of telling about what causes it. In that case we don't have to worry about analyzing it, we're then collecting data on the outward radiation and we needn't worry about making a

backward inversion. Lastly, I would say if one really wants to measure the water vapor at these levels, of course one only has one choice, I think and that is not to look down but to look sideways. This is the only way in which your one part in a thousand becomes some sensible fraction with some conceivable chance of measuring, from a physicist's point of view.

Q: Do you mean to look at the Sun?

Goody: No. look at the atmosphere sideways, take a look along the path sideways. I'm not contending it is easy to analyze, but it's at least possible.

Elsasser: Yes, but wait a minute you're up at 600 or 700 kilometers.

Goody: Yes, but you would have to choose a path that goes something like this.

Fritz: In other words you would get an average of over 2000 miles or something like that.

Goody: This is very necessary, but from a sheer ~~xxxx~~point of view of physical practicability, this is conceivable, ~~xx~~ at least in getting above the 10 kilometer region.

Question: And how are you getting absorption measurements.

Goody: That's emmission of course.

Kaplan: Or you can look at the sun in the

Adel ?: Would the residual water vapor above the cloud deck absorption in 6.3 micron region? No, not in that connection, but suppose one is looking straight down, because if significant absorption were produced one might expect the center of that band to show through using the cloud deck as a radiating source.

Goody: The center of the band is still completely blacked out above anything except perhaps a high cirrus deck. A very, very high cirrus deck. With out aircraft the center was just beginning to come out in our middle latitudes. This radiation was just beginning to come out at about 25,000 feet.

Gates: I think that the 6 mule bands in absorption from the sun will be quite weak at 50,000 feet.

Goody: 50,000 yes.

Fritz: If I may get some feeling for a summary on this, if you feel that measuring the water vapor above 15 kilometers (is that the number you mentioned) let's say 10 kilometers, you feel that this cannot be done by measuring the emission spectrum, in 6 microns or anywhere else.

Goody: I feel (1) that it can be done better with the absorption spectrum and (2) it cain't be done by either if you simply look down.

Fritz: In absorption you look down, don't you?

Goody: That's right, it cain't be done by either if you look down. You stand a better chance with the absorption than with the emission, but neither of them is any good if you look down.

Kaplan: No I am not as pessimistic as Richard.

Fritz: You say you cain't do it in either case if you look down, but I thought that your radiating source was the clouds, for instance in the case of the near infra-red.

Gates: His point was that you couldn't with reflected light from the clouds because the stratosphere moisture quantity is so low and the strength of the bands are not strong enough, so that you cain't do it with this technique.

Kaplan: What you're measuring mostly is the amount just above the clouds, which has the most amount of water vapor.

Fritz: Are you saying then that you cain't do it at all? Either by emission or absorption? You cannot measure the water vapor above 10 kilometers.

Goody: Not by looking down, you could by looking sideways.

Fritz: Oh, yes, I understand now. Looking downwards is not a question of absorption or emission, but you just cain't do it anyway.

Goody: This brings up many other problems. There's is a very active guidance problem. This might turn on a tenth of a degree or something.

Fritz: And now this includes the 6 micron thing that Art was talking about.

Goody: Well I suppose you're suggesting that you get above a nice high cloud you can measure the - . Clearly you can, so if you came acrost a cloud nicely perched at 10 kilometers, obviously you can either by absorption or emission measure the amount above that cloud.

Gates: There are other ways of doing this.

Goody: You can still do this in the visible light, I suppose.

Gates: No, I mean I would do it from a balloon looking up.

Fritz: The balloons get you away from the world-wide coverage, of course. I think balloon and aircraft are very valuable for checkcng out procedures, if you're getting world-wide coverage then you can use the balloon and aircraft for checking, but you can't get everyday world-wide ocverage by any other means.

Goody: My remark was meant to apply to a cloud free atmosphere, but it's true if there is a conveniently placed cloud, this is wonderful. But of course we cain't count on this sort of thing.

Fritz: If one wanted to measure the water vapor below that from a satellite, say the total amount of water or some distribution in say three layers, or something below 10 kilometers. Did you rule that out too, or is this a possibility?

Goody: Oh no, obviously you can measure the amount below it.

Fritz: You can.

Goody: You can, certainly. This is exactly the same problem as measuring from' down below, which has been done. The whole thing is quite reversible.

Fritz: On that then, let me ask this. Let's say you wanted to measure the water vapor in three layers, or something like that below 10 kilometers divided in some way.

Goody: Well let's hear what Lewis has to say about this sort of thing, because he's thought about just this sort of thing.

Fritz: I think this would still be important to consider. I think we should wait until he does it, but I wanted to summarize your view that this is still a possibility.

Goody: Well, my personal bias is that the really valuable thing to do would be to measure this high level water vapor.

Fritz: But you also say that you cain't do it, so I now want to get to something that maybe we can do. Sahll we proceed then to the next one, as you see on the agenda it says Ozone by R. M. Goody, but this should not be changed to not even carbon dioxide but the Measurement of Temperature by Looking at Carbon Dioxide by Dr. Lewis Kaplan who will also, I hope say a little about ozone and maybe start off Goody on some comments about that.

Kaplan: The reason I have refused to talk about measurement of carbon dioxide is that present opinion seems to be that carbon dioxide is very, very uniformely distributed in the atmosphere and I think it is impossible to measure the kind of fluctuations that for example (Keeland) at La Jolla has gotten or that other people have gotten in the atmosphere. I am going to talk about the carbon dioxide band and I'am going to show first of all a slide that will give the emission in detail, the emission coming up with the top of the atmosphere due to carbon dioxide and from this I will indicate how I think one would go about or show that it might be possible to go about getting the temperature from this. As a matter of fact, I think that from measurements in the 15 micron carbon dioxide band we can get a very good picture of the temperature distribution in the atmosphere.

The first slide is a schematic picture, not exactly a schematic picture, it is very old picture using old values of carbon dioxide intensity of what one part of the carbon dioxide band looks like, and its from about 13,9 microns to 14.3 microns, its on the high frequency end of the carbon dioxide band. The individual lines give the intensity or really the square root of the intensity of various transitions, all of which are labeled, according to their color here. The yellow lines, for example, are the fundamental. Then there are various other transitions. There is the pruple lines here which is the band that is sensitive, about 720 microns and there are various others. Many of these bands

which are called hot bands in which it is a transition, in most instances, from one elevated vibrational state to another higher elevated vibrational state in absorption. These all overlap with one another. They have different spacings, if you follow any two pairs of colors, for example the yellow and the purple. Here they are together, as you move out further in the band they become further apart, their spacing is different in each of these cases. With each of these transitions there is a very nice smooth distribution of intensities given by the Maxwell-Boltzmann distribution and in a band of this type it has been found by several people and Kostkowski and I have used it even to analyze bands for intensities further out in the high frequency end of this where there are overlapping bands and we get a perfect fit with observation. You can represent each of these intensities by an Elsasser type band which has lines of equal intensity, equal spacing, determined at any particular place, say at 705 wave numbers in here and we represent this by an Elsasser band in which the arror due to the overlapping of the strong lines and the difference from the Elsasser band would be almost exactly compensated by the effects of the weak lines on the other side. It can be shown (and I stated this in the Toronto meetings of 1953) that if you obtain a series of Elsasser transmission (this is a fractional transmission) for eachoof these intensities, then the total intensity is equal to the product of all the individual Elsasser type intensities. This can be shown merely by making fourier analysis at both of these sides. This is under the condition that the spacing is different and essentially that the reciprocal to the spacings are linearly independent of one another. This is the model that was used for my calculation and this can give homogeneous path absorption and I calculate homogeneous path absorption in here and I treat the non-homogeneous atmosphere with variations of temperature and pressure by the introduction of the Curtis approximations for treating the pressure into account.

In general the treatment of the Lorentz line shape for example, but this doesn't have to apply just to a Lorentz line shape, is the intensity over pi, intensity is a function of temperature and actually a very strong function of temperature in regions of the atmosphere. In this region which gives a lot of the emission to space and would be a very important region for determining the temperature distribution, this temperature dependence is such in this region as

to cause a difference of a factor of about 4 in the intensities between the ground level and the level of the tropopause. If we want to get accurate pictures of temperatures from this spectrum it has to be taken into account. This is a half width of the line. Scaling approximations essentially assumes that in these lines it is the wing effect that is important, that is near the center of the line. The lines are so opaguq that practically no transfer takes place so that we can ignore the half width in here compared to the distance from the center of the lines especially when we take the square of these things so that the half width is proportional to the pressure and since the absorption coefficient only appeared as a product with the mass, and the mass only appears as a product with the absorption coefficient, the mass can be scaled instead of scaling the absorption coefficient and it is ignored. I claim that this not give an accurate picture for the purpose of obtaining accurate temperature distribution. This would not be precise enough for our purposes and the method I used is the method that in the fundamentals is due to Curtis and that is you define an average half width. If you define an average half width for the strong lines in which you can ignore the transmission at the center, for these strong lines it doesn't matter whether you ignore this or whether you take an average value of this.s But, you can pick this average half width so that the very weak lines gives precisely the correct absorption. This was done. This is in the literature. It is the frequency integrated optical path average of the half width. If you divide the layers in the atmosphere now, into regions in which you have a constant lapse rate for carbon dioxide, then it turns our that these, introducing all the Boltzmann factors and so on, taking temperature into account in great detail, these are just incomplete gamma functions and they can be very easily programmed on the machine. The next slide is just an illustration of the Curtis approximation

for an atmosphere in which S does not change. The intensity does not change with altitude and the bottom curve is the line shape at one pressure, the top is the line shape at 1/3 that pressure and the optical path for effective mean pressure, with essentially the integrated optical path at any frequency, is given by the solid line. And here a portion of the optical path, the effective mean as determined by the Curtiss approximation, is given by the circle, and it seemed that even for a variation of the factor of three in pressure it is a very good thing. In this paper that I referred to and which will appear in the Rossby Memorial Volume, I have compared actually flux and flux transmission with the Curtis model as obtained with accurately determined values and this I think is the first time that an approximation of this sort has been used in which one can actually set a reasonable upper limit to the maximum error that you can get in calculating the transmission. Well, this is introducing the carbon dioxide model and the next slide shows the actual result. This is a picture of the radiation received outside the atmosphere in the 15 micron band as a function of frequency and the top curve in here is the radiation that would be received if there were no clouds. The other curve, and various curves that appear down here, as if there were clouds with tops at 900 millibars, 800 millibars and 700 millibars and so on. This is assuming a constant lapse rate for the whole atmosphere. It goes up to 100 millibars and this is hemispheric radiation and this is not what would really be measured by the satellite but in the general picture it is very similar to what would be measured by the satellite. Hemispheric radiation is hemispheric radiation at 100 millibars. This is done for quite another purpose. It is done for a meteorological purpose to get a scheme to introduce into dynamic models of the atmosphere, but at least you get some sort of an indication of what goes on. Now again here is an illustration of one of the points that Gilbert mentioned. In the center of the band, what you see is the very top of the

atmosphere, radiation from the very top of the atmosphere, the top layer. What you see in the wings of the band is the radiation from the clouds or from the ground. The stratosphere is practically transparent here and the atmosphere is practically opaque. In between there are different fractions of the radiation and they come from different layers of the atmosphere. This is five wave numbers resolution and five wave numbers resolution I think would be just about w the most efficient resolution to use in order to be able to get temperature from this.

Goody: This is equivalent to the ( ) of twenty five wave numbers in water vapor. It contains about how many lines?

Kaplan: It depends upon what part of the spectrum you are in. Each transition contains about three lines for five wave numbers and if you say you have a half dozen transitions about twenty lines.

(In reply to a question): Yes, this the Q-branch. If you look really at the Q-branch at 15 micron you would see the radiation just from the top of the atmosphere.

Goody: Could you be more specific as to what I would actually see? No, because I haven't made calculations yet, I've just gone up to 100 millibars. This is an atmosphere that stops at 100 millibars. So what I have attempted isn't right at all. It is approximately right. And that is the reason I've drawn these lines representing emission from the top of the atmosphere that goes underneath these curves. Because we don't really know what comes from the first 100 millibars, the second hundred or the atmosphere and so on. But now in this region in between each layer has a different fraction of the total emission of the atmosphere. This fraction changes with frequency and because this fraction is different it is possible, I believe, to analyse this in actual terms of temperature. The condition in which say given ten frequencies in here, and I have picked out the 10 arrows representing the 10 frequencies, the condition in which given ten frequency one can get ten temperatures from this or ten hundred millibar layers is that at each of these frequencies there is one layer and a different layer in each case in which the ratio of the emission to space of this layer to that of any other layer in here should be different from that at any other frequency. There are other conditions.in which it is determined the way the convergence should take place. But it turns out that the convergence takes place in the right direction here. There are conditions on the temperature dependence that turns out that the temperature is dependent in this case of the

line and the relation between temperature dependent and theopacity of the atmosphere with frequency is in the direction to make it more possible to determine a unique solution to this thing. I will not go into detail here, later on if anyone wants I will go into more detail. Given this it would be possible also to analyze if there were no temperature dependence but as I am going to show in the next slide, but not right this minute, there is a temperature dependence, a very strong temperature dependence, but fortunately that is in the right direction to accentuate the type of convergence that we have in this direction. As a matter of fact, I think we have seen enough of this, I think we ought to go to the next slide and actually look at the temperatute dependence. This is the absorption now in the atmosphere between 100 millibars and 1,000 milibars for the two top curves. For a lapse rate of 5 1/2° centigrade per kilometer as a function of temperature of the ground. And if the temperature of the ground of 40° centigrade, temperature of the ground 0° centigrade. The two bottom curves are the same thing but it is the absorption between the 500 millibar level and the 100 millibar level, again it is a function of temperature. You see the temperature dependence in here is very appreciable and the further you go out in the wing the more the temperature dependence is, and it is certainly something that has to be taken into account and as I say the actual way in which a temperature dependent occurs in here will help the analysis of the band actually in terms of temperature.

We had several different frequencies before which we measured the radiation and from this I would say you can solve 10 simultaneous equations. Actually you don't have ten simultaneous equations, because in a way the central portion of the band are practically opaque to most of the atmosphere. You just see the top of the atmosphere so they can be separated off, or your matrix has many zeros in it, which is another way of putting it, you may solve these equations in terms of departure in temperatures from some standard atmosphere. This requires a linearization in the departures of temperatures. It is not obvious from what I have done so far that linearization can be made but at the very worst you can pick a most likely model. A model can be set up to calculate the emission going out at the top of the atmosphere and you can make a fit, a least squared fit or a least difference fit, to the best possible atmosphere that will give you the different amount of outgoing radiation at the top of the atmosphere. Probably the best thing to do is to make some sort of a fit like this, close enough so that you know that departures of temperature that would have a linear

effect upon the outgoing radiation and then to solve your simultaneous equation in terms of departures of temperatures from this standard atmosphere. I won't say much more about this now but if you want i'll go into a little more detail on the method. The point is I think that given these ten measurements you can actually determine uniquely and temperatures that will give the outgoing radiation. From preliminary calculations I've made I think that it is not impossible to determine within one degree if you can measure the radiation accurately, to the accuracy to t which you had done your emission measurement.

Fritz: What were the accuracies?

Kaplan: Considering that Richard's emission data is correct, correct within one per cent, the variation that he ha d gotten in the emission data I think would be enough to give 1 or 2 degrees in the temperature. The thing that I want to definitely point out is that in order to get ten temperatures or in order to get any sort of a real distribution of the atmosphere you must have ten measurements. You can't get any more temperatures thatn the number of measurements that you have. My next slide illustrates this point. Next slide please. This is a curve showing the infinite number of conditions in ground temperature and, temperature increase with height assuming the lapse rate constant all the way up to the 100 milibar level. It will give an outgoing radiation in the whole band of carbon dioxide out of five calories per centimeter squared per hour. The bottom curve is for cloudless condition and this will occur for temperatures, depending upon lapse rate whether it is an isothermal lapse rate or an adiabatic lapse rate, the temperatures are from 25° at the ground to temperatures of + 15° at the ground. If there are clouds with tops at 800 milibars, 600 milibars, 400 milibars, these curves are different and the situation of clouds gets even worse. The slope of these curves is even larger as far as determining the temperature is concerned. If you know that you have a constant lapse rate and you know what the temperature of the ground is, say by making measurements in a window then you can determine what the lapse rate is. But if you don't know what the lapse rate is you have no information which will give you anything about the variation in lapse rate, except to determine some sort of mean temperature, whose interpretation would have to depend on a lapse rate. This illustrates that if we do not get enough measurements that the results are quite ambiguous as far as interpretation is concerned. Next slide please . The next slide shows the emission spectra of Goody's that

I have talked about. This is done around London. Looking up at the atmosphere and this is done with about one wave number resoselution, now I'm talking about five wave number resolution instead of one wave number resolution and you see that there is tremendous detail in here. This is very beautiful spectrum in here, and I think that if we do as well as this that we can lick the problem as far as temperature distribution is concerned, as I shall indicate shortly there are other instrumental requirements that are necessary for this.

Question: Would you mention the wave length limits on the lower diagram please?

Kaplan: This is at 1400 wave numbers which is seven microns and this is 720 or about 14 microns. This is the window region and you see here how little of the window this actually is. But, there is a region of eight wave numbers in here in which there are no lines. There are lines but they are lines that are so very weak that they are completely trivial. * Goody has a window marked off in here in which there were no lines listed in the Migeotte spectrum. Actually there are lines in here due to the 6 μ band.

Elsasser: Could you give us some idea of the ordinate, I mean what fraction of blacknesses is.

Goody: This is through 3 air masses. I think if you looked at liquid air that it would be displaced up here.

Elsasser: That's a few percent, let's say of the black body emission.

Goody: Oh, as much as 20 or 30 percent, this is at three air masses.

Kaplan: And one thing that I would mention at this time, this isn't a real window anyway, because there are wings of lines, there is some background in absorption which Mr. Goody has talked about, and talking with people the work that they have just recently done at Johns Hopkins, that dependence on mass and pressure in here of the absorption, indicates that it is not really entirely wings of lines, but indicates that there might be something like _____ perlimerazation. In other words one knows if it's the wings of lines, for a constant path length, it should be proportional to the square of the pressure. It is proportional to the square of the pressure until you get air saturation. Once you start getting air saturation then it goes up very, very rapidly. So there is something else that comes in here, perlimerisation is what Benedict thinks is most likely.

Question: Those lines in the N^{20} region are largely water lines with N20 superimposed.

Goody: Yes, there are merely individual N_2O lines there. Actually we have

better resolutions now. We can get the individual CO_2 lines in here now.
Kaplan: Now what I say is that one does not need much better than this. One does not need this resolution. This was done I understand from Dr. Goody, with a one second time constant with a ground recorded, and that it was done with a cone of 6°. Actually I think we want a smaller cone, but I think that Sig has other ideas. If one looked in a smaller cone then, one could still get the same amount of energy, because of the wider slit in this case. However, we are looking down, part of the radiation we get is from the ground, part of it is from cloud tops. We have to be able to determine where there are clouds of course. This thing won't tell us, I mean to some extent tell us whether, probably measurements will tell us whether there are clouds or not, but it will not tell us what faction of the field of view is covered by clouds. There should be a television picture of the clouds exactly at the same time that this is made, but the ten frequencies from which measurements are made should be done simultaneously so that they are all from the same background. This thing that we want done in 15 micron band can probably best be done with a grating and it can be done say with a series of cells all acting as exit slits from the grating.

Assuming the carbon dioxide is analyzed and the next curve shows water vapor spectrum in the rotational band, the read on that I am thinking about the rotational band is that there is more energy in the upper < part of the atmosphere and the stratosphere in the rotational band. Also there is this clustering together of lines, this also occurs at 6.3 but using 5 wave numbers resolution in 15 microns better resolution would be obtained from the same grating out in this region. If you had some sort of a cut off here, a strong cut off by reflection, or something. Reflection by a crystal at 12 1/2 microns the photo cells would cover the entire band and you would get the radiation out about 25 microns. The 50 gram series is I believe is the condition. These are the conditions something like the conditions in the stratosphere. Average conditions in the stratosphere are as the amount of water vapor is concerned, these are the various pressures marked in centimeters on here 02 centimeters, 6 etc. Now take the 25 micron band for example, this band contains a series of lines, a series of strong lines. If you go from one end of this series of strong lines to another end of this series of strong lines it is the order of 2 to 3 wave numbers.

This was done with about 5 wave number resolution. If you use the same grating and got five wave number resolution at 15 microns then the peaks of these lines will be increased very greatly and at the center of the lines. You would have complete absorption for the stratospheric layer. So for the region in which Richard was speaking about you actually by setting on one of these lines can really get radiation from the stratosphere and given the temperature although I have not made an actual calculation for this, I believe you can actually analyze the stratospheric content of water vapor, and of course as you go out further to a region or a series of lines where the absorption is less, it is more and more certain that we can determine the water vapor structure.

Question: Were you looking straight down at the earth.

Kaplan: I am looking straight down at the earth now and these two curves represent the amount of water vapor you have in the stratosphere.

Goody: Yes but what about _____.

Kaplan: Oh, but you look down as long as the stratospheric _____ _____.

Eisaeser: The long wave length window ends at 24 microns.

Kaplan: Yes, but I am talking about something else. I mean it ends at 24 microns depending on how high you are in the atmosphere.

Comment: It takes a small amount of water vapor to determine the spectrum at 24 microns.

Kaplan: Yes, well what I am saying is, when you look at a particular, or you look at a series of lines in which it is x opaque below a certain level you get the emission, then this will tell you how much water vapor you have above this point. Do I make myself clear? I have not done the analysis on this, but I think it is possible.

Fritz: Wouldn't this same arguement that you are giving now work on the 6 micron band. Isn;t that opaque right in the middle of the 6 micron band.

Kaplan: One of the things about this band is that we can pick series of lines like this, it is difficult using the same grating. We get the same resolution as the for the 6 micron bands; however, there is less energy in the 6 micron bands, there is more energy in this wave length, and if we cain't use this line which is comparible to absorption over a broad region to a 6 micron bandla frequency could be picked at which it is opaque.

Fritz: In other words you are saying that there would be places in the spectrum. What is the wave number? Was it two wave numbers you wanted on this too.

Kaplan: Yes about two wave numbers.

Fritz: To get to the middle of the band you want about two wave numbers, so you want to make measurements at several wave lengths in the vicinity say of 25 microns with resolutions of about two wave numbers.

Kaplan: Yes that's right.

Adel ?: When you say there is more energy here you mean the energy from a black body at -65 degrees.

Kaplan: Yes that's right.

Gates: Well the main contribution to the emission to space in the stratosphere is from this region. This is the major contribution to the total energy loss. I have forgotten the exact percentages, but it's well over 50%. It may be 70 or 80% or something like that.

Kaplan: And certainly all the contribution comes from the rotational band.

Gates: We can use Co_2 _____ _____ _____ the 9.6 ozone, isn't that right, something like that. I have seen some figures on this once.

Plass: I don't know what fraction of the total it is on hand.

Fritz: Lewis you were going to say something now about the wave lengths and the resolutions you would need to measure the water vapor below the stratosphere.

Kaplan: May I have the next slide please. This is the situation for the stratospheric temperatures. The argument is the same. Here you have about 50 percent to go about 100 percent if you have twice the resolution. In this batch of lines.

Goody: Suppose you have a line blacked out, what information does that give you about the amount of water vapor above what level.

Kaplan: The emission is a function of the distribution of water and the temperature, and from this distribution, and it is a different function of the distribution and the temperature at different wave lengths, so getting again a series of n measurements, what I am saying is that you can obtain water vapor distribution assuming some sort of distribution within each layer that you have for n layers.

King: I'll discuss this in version technique in a different context and also show some of the pitfalls.

Goody: We've tried out this inversion technique and found it a very tricky business. Unless you get a very big variation in your paramaters it's very difficult to solve the thing.

Kaplan: Yes, well maybe we better talk later about the inversion technique and from what Gene says, maybe we ought to wait until he is through. Well

let me say that I am very optimistic about the inversion techniques.

Gates: I would just like to say one thing while you have this in front of us. Next year by this time or before nowwe hope to have the absorption spectrum in this region of the sun from balloons up to high altitudes.

Fritz: Why don't you look up and down at the same time.

Gates: Okay

Fritz: All right what about the other regions looking into the water vapor below the stratosphere.

Kaplan: Oh yes, well if you're looking between lines you go further in here.

Fritz: In other words you're using the same portion of the spectrum for this too.

Kaplan: Yes you can use the same. The difficulty in any portion of the spectrum is to find a line that's strong enough. Once you find a line that's strong enough then nearby there are lines that are weak.

Fritz: What would be the resolution for that. Say to measure the water vapor distribution from the ground up in some crude way.

Kaplan: If you have two wave number wave resolutions and I don't know any reason why you shouldn't use it, except that it isn't required for the other parts you undoubtedly you can use a larger number of wave numbers resolutions.

Goody: If you do a 2 or 3 layer model in the troposphere you certainly shouldn't need more than 25 to 50 wave numbers should you.

Kaplan: Well I think you would need better then that.

Fritz: Will you settle for 10.

Kaplan: Yes I would say 10.

Fritz: Well I guess it has to be analyzed a little further . It should be somewhere between 10 and 50.

Goody: It's easy, not difficult, I think that's the main thing here.

Kaplan: But there are a few frequencies in which you should use two wave numbers.

Fritz: But, Richard, do you agree that you would use this portion of the spectrum, too, or would you use another one? Or doesn't it make any difference?

Goody: On Water vapor, well certainly you want the region in which the absorption properties change very rapidly. You can either go to very high resolution and work on a single line with a quick change absopption characteristic this way or if you are going to have wider slits you want to go over a spectral region in which the mean absorption characteristic vary and

this is a good mregion.

Fritz: For example, if you were going from 6 to 8 microns, you wouldn't do that, you would go rather in.

Goody: Well there is more energy, and we are much more certain of the laboratory data.

Gates: Also of the theoretical data. Your theoretical data are much better determined here, too, for the rotation bands.

Fritz: Are you going to say something about ozone?

Kaplan: Yes, the next slide is ozone. This is the Ozone Spectrum. It's very messy as you can see in here, this is Migeotte measurements from Jungfraujoch of ozone and above this is my analysis of the measurements broken down, but there are irregularities in the band, but very much like some sort of a random model. I have treated it going from maximum to maximum completely at a random.

Fritz: What wave length are you at? Is this the 9.6 micron band?

Kaplan: Yes, this is the 9.6 micron band. The center of the 9.6 micron band. The analysis if you look is quite good, but in some reqions the Q branches fit in beautifully and then some of these lines fit absolutely perfectly. As you go out in here it gets worse, this is only going from maximum to maximum. It already gets worse/even at a J, less than 20, and here it's very, very bad and that is because lines are coming back on themselves in here. There is a very strong corriolous inter-action between this and the 9 micron band and somebody, Dr. Howard/Blair there has just analyzed the 9 micron band and he will shortly have out an analysis which will tell us what the positions are I hope. But this is know and calculations can be made for the band. The region which I am going to discuss very shortly as far as the absorption is concerned in trying to measure the ozone the distribution of temperature in the atmosphere from the ozone is the 3 wave number region in here and the 3 wave number region in here. These are two regions in which Walshaw has made measurements. There would be advantages to using high resolution in here. This is one wave number in here and you don't have a dip in between. Now one of the difficulties with carbon dioxide is that even in the strongest part of the absorption you can see the ground. Some of the radiation from the ground gets in. I mean in ozone. There is no place where ozone is really, except again if you look really at very high resolutions at centers of lines.

No, probably not even in clusters like this, you can probably even in a cluster like this see some thing coming from the ground. So that, as much information can probably not be obtained from the ozone as from the carbon dioxide. One of the nice things about ozone is that and as you recall carbon dioxide is terribly temperature dependent in absorption, ozone is not quite so temperature dependent at all. Ozone is a very heavy molecule and because of this, even for high J's the lowest state energy is not so very large, so that over a large part of the band the temperature dependence is much less less important than for carbon dioxide, and because of this using for example, the Curtis method it is possible ~~it is possible~~ to use laboratory data in order to determine what the absorption is. Now how transparent this for the ~~kkmx~~ tiny amount of ozone that is up near the top of the atmosphere is that it is so transparent that you can not get any information, because really at the very top layers you are in the linear regions of the spectrum,,and it is will not go into this because I ~~kmmkx~~ have well run over my time. If it ~~xk~~ is very ~~kmmx~~ transparent so that you are in the linear region than you can not analyze, because you set up your equations and equations all dependent upon one another, you don't have independent equations. I will give some figures to show that even for a small amount of ozone in the region around 50 kilometers the absorption is far from linear in the strongest ridge of these 3 wave numbers here. The next slide which is the last slide I am going to show, shows a fit of the random model to the region from maximum to maximum essentially over the curve against observed values made by Walshaw. This pressure goes from 30 to 730 milometers, a miss from .03 to .20 centimeter. This is the average value as Goody and Walshaw have shown if you pick particular regions and try to fit them with a random model, then the departure is very very great. This shows you that if you go over a large enough region you have cancellation of area, but what this indicates is that in making the analysis you have to be careful to either pick a region in which the fit to your model is good as compared to laboratory measurement or else if it is not to try to apply some correction somewhat along the line to perhaps what Jean King is going to talk about, the work that he has done in the spottiness of the spectrum. But now, yes this is 23.7 wave number, the dot this didn't show up. Well I think this is the end of the slides and I want to give some numbers on ozone and then I am through, and I am sorry to have talked so long.

Fritz: Oh that is allright you are giving two talks and you are entitled to a little more time.

Kaplan: Well what I was going to say was that the ozone absorption in the upper layers of the atmosphere departs from linearity and I have gotten in a hurry some figures of ozone concentration from Burch's Progress Report and I'll even give his layer numbers. Here, layer 10, 9, 8 and 7, the height in here in which the ozone curve in here is in 5 kilometer heights from 44, 39, 39, 34, 34, 29 and 29. I don't the layer just above this which has just small amounts of ozone because I don't have any data to compare with it. This is the mean average pressure in mb and it is 1.2, 2.4, 4.7, 9.4 and I have written down the pressure in millimeters also because Walshaw data one in this form. Then the amounts reduced centimeters, the reduced path length in these layers are 005, 010, 020, 040 about, and sum of the masses, which is used to get, using your laboratory data, is .005, .015, .035 etce in here. And now mean pressure defines our Curtis for these layers, are again .9 which is the same as this, 1.5, 3.7, 6.0, this is just ot give you an indication of the distribution of ozone. These are Umkehr problems.

Gates: What are your units here on the masses?

Kaplan: The masses are in centimeters of ozone reduced to STP. And now I am going to take runs- I'll even give the number of the run, these are available at the R.M.S. I am taking two runs at low pressure and with the smallest amount of mass, and it most closely comes to these conditions. Actually it does not quite to these conditions, but now I have the mass in this case is 0.216. 258 is the number of the run of Walshaw, in case you have to look up the data at the R.M.S. Cloud here 0.312. And now I am going to give a P infinity, and P infinity is the pressure as though it were ozone mixed with nitrogen for example, or ozone mixed with air. It's a pressure in which, if a correction is made to eliminate the effect of ozone, ozone collision, so it is essentially ozone-air collisions, unit this is 1.02 mm, 1.22 mm. and now the absorption at 1056.8 wave numbers, and I am going to give you absorption also, at 1053.3 wave numbers and this is the region of the maximum absorption, both of these in the region of maximum absorption. Absorption about the same in both cases as you will see, .072 in this case .083 in this case, .075 in this case, and .086 in this case. And now I am going to give the absorption that would occur if the absorption were linear, due to this,

and it depends upon whether you use my method of extrapolation or Walshaw's method, extrapolation is slightly different in here, and my method I am multiplying the mass by 10, while Walshaw is multilpying the mass by 9. Essentially this is very close to it and it doesn't make an awful lot of difference, but this is the linear absorption in this region and you see that it is way below the limit. The mass is definitely higher than these masses that we have. I am comparing this absorption with the absorption that it would be if it were linear. This is the absorption right here, 10m is the coincidence. It is enough to compare this and now also since the pressures are about the same, approximately the same, we can actually divide the masses and see what the departure is from linearity of these things, and ratios that we get are 1.44 and despite the fact that the pressure is larger in here we get 1.15 here. So there is enough of a departure from linearity in here that it appears likely that you can get some information from it. However, probably only a few frequencies can be used to deduct their temperature. It would be a shame if these frequencies would have to be used to determine the ozone distribution. I think the best thing to do is to determine the ozone distribution by the Umkehr effect and reserve all frequencies in which you can make any kind of measurements to determine the distribution of temperature between say 30 kilometers or 25 kilometers, you really can't go below this. Say 25 or 30 kilometers to 60 kilometers. I think the ozone can possibly give this information.

Fritz: Which wave lengths would you use to measure either the temperature or the ozone?

Goody: You would measure in the region near 10 microns.

Fritz: What would be the Resolution in this case?

Goody: 2 wave numbers or something like that.

Kaplan: Yes, it would nice to get one wave number in a few places like that I showed, something like that plateau. One would need one wave number in order to get away from the dip. The better the resolution the better off you are here.

Adel ?: When you say to use the Umkerh to measure the distribution you mean at 3,000 angstroms or so.

Kaplan: Yes, actually you can go further in the ultra violet. That's another advantage of being above.

Fritz: You mean Singer's technique for measuring or scanning the spectrum in the ultra-violet, reflected ultra-violet.

Kaplan: I wouldn't say that it's his technique.

Fritz: Well, he wrote about it.

Goody: The Umkehr is a notoriously inaccurate method of measurements, you know that.

Kaplan: It doesn't have to be an accurate method of measurements. I spoke to Sekera a month ago, and Sekera beleives the Umkehr can be a very accurate method of measurement if it is done right, that is, if the calculations are made correctly and that it would be a beautiful thing to do from a satellite. It could be made much more accurate if it were done from a satellite, but it is certainly something that should be looked into and it is my feeling now is that you can probably determine more accurately the ozone distribution by the Umkehr method than you can by emission.

Fritz: Well I think that we have purposely stayed from solar radiation because there is a whole spectrum of problems with that and maybe this is subject we should leave. Now, in using these particular wave lengths you wouldn't use these to determine temperature, would you?

Kaplan: Well in any case I don't think you can get the ozone in a region below, anyway you certainly can't get it by the methods I have been using because the Curtis approximation is absolutely no good for a very large variation of pressure and for an increase in concentration with height. This is what you have when you are looking below the ozone maximum. I think this method is good only for the region above the ozone maximum and there we are only saved by the fact that the temperature is so low there compared to the temperature around 50 kilometers. So that is only a small fraction of the energy that gets out. But this is a little ticklish. I am less sure of the ozone than I am of the temperature through the carbon dioxide, or even of the water vapor.

Goody: I think that this question that we are concerned with might/at least be used for illustration, because several people here have been involved in this. It is the one case of the type of inversion which will have to be used. Adel has worked on this for a long time, so has Jean King and the rest of us can speak with some familiarity of the type of problem which is involved, that it is perhaps the one case of analysis which has been made _____ _____.

Goody: (Showing Slides) I thought this might illustrate. Without laboring theoretical analysis, Walshaw has done a lot of work on this band by the more fruitful but perhaps esthetically less satisfying method of showing it through an electronic computer. What he has done is this, first of all this resolution and this is quite an important point, you have to go to this kind of resolution one or perhaps two wave numbers. Actually for certan you can get inbetween all contaminating lines. This was most important for a really exact analysis. One does know where these lines are, these water NC_{02} lines, and for example, you can set your slits and get in there and be sure that you have no contaminating line touched by your spectrum. Let me give you a few figures. I have picked five useful points which are uncontaminated with this degree of resolution, and naturally one goes then to the problem of getting two component analyses, if possible. Now the second thing is that as you work with your digital computer it rapidly becomes clear that with this kind of resolution what you want is a big effective change and not intensity. Ignoring the temperature affect and assuming the that the Plank function is the same all acrost the band, the value you will really want to vary is the optical density. Now, you can change that in two ways. You can change either the absorption co-efficient by looking first of all here, then here you can jsut squeeze out a change of 10 to 1 without being unreasonable about this, and the other way you can change optical depth is by viewing the strong air masses, perhaps 6 air masses. At any rate I think that intoto by looking at all air masses that the maximum range of absorption coefficient you can change the optical depth in that band by about 50 to 1, so we used 5 points plus scanners, about 5 or 6 air masses to get the curve which effectively gave you the emission. It could be just one point. It doesn't make much difference which point it is from the point of view of analysis, but if you ignore the temperature effect you can effectively coalesce all your results and get a one curve or an idealized region with a change of optical depth of about 50 to 1. Now what will you get out of that? Well, the accuracy of these is about 1 percent and the noise is somewhere about the 1 percent level. We certainly have absolute accuracy to about 1 percent by looking at different air surfaces and water surfaces. Now we tried this out fairly thoroughly. You can put it into a digital computer and give it a multi layer model of the ozone, give it the temperatures and let the machine think it over and pick

out the least squares that best fit. It's pretty limited in what it will do. It will pick out about five layers from this amount of noise. Given no other information this is with about 1 percent noise and 50 to 1 change in optical density. This, however, would be really satisfactory, but if you are going to start looking down ____ _____ _____ because you have the difficulty/if you normalize on liquid air, of course/you don't know about any continui across this background and although you can measure the continui and we do measure in the/there and in there, and in here some place we get the continuous background. One has to have information. This is the kind of thing you run into, that is, given the continui in here and in here, what is the continum in here. That is a necessary part of the analysis and upon that particular rock I understand that Dr. Walshaw has completedly foundered. He can't even make this/five layer analysis and so I mention this just as practical experience. About the limit to which you can drive these things. Of course the analysis can either be temperature or density. It can either be temperature/given density or density given temperature. Which way you take it depends upon your secondary data. If a good Umkehr method has been developed, well and good, but as long as it has been brought up that the Umkehr method is perhaps the least accurate way to measure ozone that is being used today. If there have some massive improvements this is good; however, that is the state of the art as it is. Without improving the Umkehr method one would be much better advised to use the carbon dioxide to get the temperature and use the temperature to get the ozone I would guess but this is a practicle problem which must be worked out yet.

Kaplan: Well this can not be done because the carbon dioxide cannot be used to give you the temperature in the region where you have maximum ozone and below the region of maximum ozone the path lengths are to large and with a distribution of ozone increasing with height I am afraid that even the Curtis approximation is very impracticle for treating this problem.

Goody: True, but we can assume that we will get something better, presumably.

Kaplan: Now as far as the background is concerned, if you are looking down you have much less trouble with background looking down than if you are looking up because background is partly immerged with the ground. The background would come mostly with the bottom layers of the atmosphere which is not so very much different from the temperature of the ground and the change between that in the radiation between that and what the background

temperature of the ground is would be quite small indeed. /~~If~~ So that you are looking at a background that you understand quite well as against a background which you do not understand when you are looking in a upward direction.

Goody: You assume that the ground is a grey body which is probably quite close to it but as a matter of fact this is a very ticklish point. In the case of a satellite of course you can't scan an angle for obvious reasons. You would be looking over one continent at one angle and over another continent at another angle so you can only do this wave length business so this wave length business is really important. Now to get your 10 to 1 you have to come to places like this and to find what of that is ozone and what is background you have to know the background with a fairly high degree of precision and although you can probably regard it as a black body I wouldn't want to say that it is that ~~gmmx~~ grey or that one can get a good analyses out of such a simple assumption.

Question: Well you monitor the background in a transparent region wouldn't you?

Goody: Well, yes you would.

Kaplan: Then you would have to extrevolate.

King: What sort of/~~transmission~~ transmission did Walshaw?

Goody: Well we measure it in these regions. We know where the stuff isd We know it is pretty accurate, say near 950 millibars and we know it's mean emission temperature. We can measure that quite accurately, so by measuring the emission in here we can tell it's absorption. So we have it's absorption at these points and we have information on the balance of the spectrum but it's got to be exceedingly good information you get in here.

Kaplan: May I make one point that I neglected to say in all of this, anything that is done theoretically, should of course be compared ~~mkkx~~ with experimental measurement and I think that in an early stage, way before equipment is put in the satellite, and this is particularly true of background studies in here, that this be done from say a balloon or reconnaissance aircraft in which simultaneous radiosonde measurements are made in the below conditions. It has to be calibrated.

Dr. Fritz: ~~X~~ Lewis to hear you say that warms the cockles of my heart! Well let me summarize on the ozone then. Ozone may be more difficult but still suggestions are as I see it, I think we are going to stick to the infrared, that we can hope to get something like 5 positions in the spectrum

centered more or less around the 9.6 micron band, that we would need resolutions of something like 1 to 2 wave numbers in order to do us any good and with that we might be able to determine the ozone distribution in a few layers in the vertical.

Kaplan: No, you would be able to determine a temperature given the ozone.

Fritz: If you have five wave numbers can't you get two layers of ozone and two temperature?

Goody: No! you have to be given one of the two.

Kaplan: As bad as the Umkehr method is I would bet that it is better than the emission.

Fritz: Well what would we get? We would get neither ozone or temperature then. What would we get?

Goody: You still have to know one to get the other.

Fritz: But we don't know either one of them.

Gates: You may know the temperature from other information, oh from soundings of the atmosphere.

Fritz: Not over the Southern Hemisphere or over the oceans in some places.

Question: By temperature do you mean distribution of the ozone. Suppose you want to measure the total amount as well as the distribution.

Kaplan: The total amount is not so bad this can be done from reflected radiation, probably from the earth in the ultra-violet.

Fritz: In the end then, we can measure in five wave lengths perhaps but we would not be able to separate out the ozone from the temperature unless we could get one or the other by some other means. I would suggest that we take 5 minutes out and then let Jean King have a crack at us.

Jean you can have 20 minutes or so and then I think we should break up and go to lunch, we have to be at this place a little after 12:00. I would like to ask Jean King if he would tell us his ideas about measuring temperatures in the atmosphere from satellites.

King: This work was done about three years ago, but because of diversion of my interests in other research areas it hasn't been with the vigor that I think it deserves, so at least here I think we can decide to follow through it or at least give the work a decent interment.

Fritz: Would you take the hand mike please.

King: The basic idea starts from astrophysics really, and the well known

limb darkening effect that one gets with all stars in particular, the sun. What you find is that as you move from the center of the disc to the limb of the sun, the light intensity falls off by a factor of about three and this is due to the fact that when you are looking at grazing incidence at the limb, your eyes are intercepting only radiation from the outer layer of the solar atmosphere, wherever that is. While if you look at the central point, then you are intercepting photons from the deeper layers. So that, in principle, a knowledge of the variation of the m emergent intensity from center to limb gives you information on the vertical stratification of the solar temperature. If it gets darker towards the limb it is obvious that the atmosphere is getting warmer as you go down. This was first used to check our certain radiative equilibrium theories which give temperature distributions for the sun and this was a way of checking on various radiative equilibrium models. Well, it is a natural application to think of this in terms of an earth satellite where you have roughly similar conditions of looking down from above, and presumably by selective choice of wave length you could observe a limb darkening or a limb brigthening effect. The basic equation is the intensity emerging and I'll connect this up with what Lewis and Richard Goody discussed. The emergent intensity is seen to be a linear transform of the planck black body function as a function of depth. The kernel in the gray case is just the simple Lambert function, and we can write this as a Laplace transform, so essentially what the problem devolves into inverting the transform and symbolically if we invert the transform we can obtain B as a function of depth, given the emergent intensity at various angles. Msi here is the cosine of the zenith angle or lambda here is the mean absorption coefficient divided by the monochromatic absorption coefficient, so that what I will talk about will be the inversion over angle. In other words we will vary the angle while what Lewis has concentrated on is inverting the varying intensity with position in the spectrum. So either of these two methods is possible and the way that one proceeds with this inversion is -there are all kinds of ways of doing it - the simplest one that I came up with was a broken line segment - we call it a polygamel arc-where you assume-lets say this is the actual distribution of B with depth. I assume a broken

line segment here of given length, and then what you do is take the intensity as a function, measured as a function of the mu, here and if you have five different measurements this then determines a set of five equations and the unknowns in this case would be the temperature here and the 4 slopes of these line segments which you can get. So in principle it looks extremely favorable. So in practice, for a synthetic example, it works out pretty well here, I assume a distribution of B with depth then saw what intensity zero mu would be for such an assumed B. Then work backwards with this arc technique to determine the slope and it indeed does work out very well. This however turns ~~out~~ out to be falsely encouraging, for reasons which I'll proceed to go into. In the first place you kind of unconsciously set up a favorable situation, and in the second place, the whole problem of inversion is more difficult than it seems because, well you are trying to get information about a temperature distribution here while you are looking through a lot of intervening material which tends to mask the effect of the deeper layer. In other words, it is like trying to tell if it is raining out doors while you are in a shower bath. You are having to look through something that is obscuring, which makes for a grave indeterminancy of the values at the deeper layers, although it does determine, as one would expect, since most radiation comes from here, these slopes of the upper portion quite well. Well, what I wanted to do then was to check this out with some terrestrial measurements and the first thing that came to mind was to talk with Professor Adel. He has, over the years, on a routine basis, made Extor measurements of ozone and it seemed to me that if I could get from him this extor parameter as a function of air mass, well then I would be able to work backwards and construct what the vertical temperature structure of the ozone actually is. Before I tell the results, it is worthwhile mentioning just what this extor is, what is it Arthur, I never remember the actual name. Effective radiation temperature of the ozone. Anyway, simple viewed, say if you are looking here, your radiation is coming out, now we're having the bottom of the ozone atmosphere right down here and we are looking up. This extor is, lets assume the ozone is completely isothermal and that it has an optical depth of say tau subt. If it is

isothermal, then, the emission coming down here, just from Schwarzschild's equation, it will be B times 1 minus transivity and this B then is the extor, since for the isothermal case this is just a emissivity. I can write extor just as I zero mu over the emissivity. Ordinarily this extor is measured at the air mass where your sun is, and so I didn't know whether or not he had made any scans of extor. Extor will be a function of your thickness of your medium. And physically it represents some means temperature, for instance we know that the temperature structure of the ozone is increasing, throughout most of it, so it will represent a temperature weighted towards the bottom of the ozone and I think you can see, for instance, why it should change with position. If you are looking through a long air mass traverse, your extor temperature will be essentially the temperature at the base of the ozone, and conversely if you are looking straight through, the extor will be a represnetative temperature higher up in your ozone layer. This fits our problem here and so let's put down just a few sets of values which Professor Adel obligingly furnished me earlier this week. This is the air mass thickness. 16.8 would correspond to looking through practically grazing along the horizon. M is one over e. I don't know the date of this, ~~xxxxxxxxxxxxxxxx~~ you didn't give it to me perhaps (1948). Do you know what season? (Spring) Here is just one more for comparison. s The air masses go 1, 2, 2.9, 3.8, 5.8 and if you calculate the extor values from the data that Dr. Adel furnished me it is worth noting that actually I have the extor temperatures - actually all you need are the emissivities and the total optical depth. This is in principle a simple measurement to take. You don't need much data to reconstruct this vertical thermal structure. Also, another thing, it turns out that the pattern you get from the inversion is quite insensitive to not only the total thinkness that you assume but also to the precise form of your transmission function. It really doesn't matter too much because all we are obtaining is sime profile here, we are not specifying the thickness or the airmass depth at/~~xxxxxx~~ which these temperatures occur. The first thing that alarms you is the following: If the ozone temperature goes up throughout the layer, say from 25 to 35, something like that, we would expect that as our air mass traverse increases we would expect that our extor temperature should go down and precisely the opposite effect is occuring here. The reason for

this I don't think is hard to uncover, it is simply that in this long parallel to the horizon traverse, you are certainly picking up some of the contaminating water vapor radiation here, and it is all the more serious because the temperature of the water vapor that you are picking up in that path would be much higher than say the base of the ozone temperature that you are trying to discern, so that is very upsetting. That is one of the difficulties. Trying to seperate the two effects. It seems such a striking increase that it was hard for me to think of it as being just exclusively the effect of tropospheric ozone which is very slight. The same effect is shown here, that a slight warming as you get down to the bottom, and cools off, then rises again which is the behavior you would expect. All this is well and good, until you try to carry out the inversion and this is really the discouraging aspect. You find that the actual ozone structure, temperature is going to be something like this, that the polygamal fit you get varies wildly and in very little meaningful sense here, you can get huge slopes which of course are patently unreal and the whole trouble, I feel, and this is something that I want to check into more carefully, is the extreme sensitivity of these temperatures to a slight variation in your emergent radiation here. In other words, if you would just make a slight error in the determination of these quantities here, the indeterminancy of the system, due to the fact that the upper part here is essentially hidden, is such that the coefficients are really not very well tied down. I am not expressing this very well, but if you carry out a calculation I think you would get a feeling for this indeterminancy of the scheme. The only way that I think that one can improve the measurements, situation is by making extremely sure and more careful measurements, maybe it is actually the local inhomogenieties in the air mass. For instance, all of this assumes, as a basis, a plane parallel atmosphere. Maybe in the course of the measurements, advection would be enough to render this inapplicable.

The one good thing it is that it is an excellent self consistency check. That is, if you do get a series of extor measurements, which do make sense, and provide a meansigful close approximation to some reasonable distribution, then as I say, that is a good self consistency check. Now certain things will be improved by looking down from above, rather than the reverse, in that you wouldn't have a contaminating effect of the atmosphere from beneath. I think that the conclusions are that the water vapor

masking is serious or we must look into this tropospheric ozone possibility. It is a good self consistency check. The satellite would be more favorable, and as for a proposal for future work, I think what is needed isi more observations of this extor type and a complete checking out of how serious is this lack of determinancy in the calculated lapse rate.

Fritz: I wonder if Dick would want to repeat his question of before? Namely, when you look at large angles, which I presume what youhave in mind here, you might have to look 60°, for instance, from the sub satellite point as your air mass 2. That you might be looking very very far away from the sub satellite point.

King: You have a satellite rotating vertical axis with ports at various angles here and if it is spinning you would integrate the effect, presumably and smooth out the horizontal inhomogeneities.

Fritz: What was this? I didn't understand.

King: Well if you would have a satellite which would rotate above a vertical axis with ports at various angles it's clear then that if part of the failure is due to the say ozone clouds or the failure of the plain of parallel assumption this would automatically integrate out those effects. Any affective effects would also be taken care of here because these observations at various angles would have been made simultaneously.

Fritz: The one looking put at far angles, you would look at Northern Canada and Mexico for example and average those out? Look at 60° from 600 miles you would probably see Northern Canda and Mexico and then you would average those out.

Goody: I think it's important to realise that any kind of inversion like this is an insensitive kind of inversion. You do need accurate data to get anything out of it. You cain't do this kind of inversion roughly.

Kaplan: Did you assume a grey atmosphere like you had in the other thing?

King: First I took the transmission functions that Epstein, Adel and Osterburg report which was the usual averaging over several lines. Then I took just a straight E to the minus tau over mu and it made very little difference. The results are very insensitive to the form of the transmission function. You take, that is as far as the coefficients that you get.

Fritz: To summarize this one I suppose you would be satisfied with rather course spectral resolution. You look in a rather course part of the spectrum

in this absorbing ozone band but you would want to get angular resolution over I should think at least three or four air masses. Because the variation as you go from one to two is as you go from angle o to 60 is very slow but after that is goes rapidly.d Then this question about the geographic thing still remains a serious problem.

King: What is needed in all this is a mathematical analysis. If you put in an error here, how does it get propogated in solving this matrix of simultaneous equations.

Kaplan: One of the difficulties that you mentioned is that very little radiation comes out from the top of the atmosphere. If you look at a real atmosphere we have lines and lines broaden out as you go down with height so that actually from my slide on the carbon dioxide this shows up. You get about an equivalent amount in the middle part of the spectrum from each of the layers as you go up. There is a decrease but this is compensated by the black body ____ _____ _____ _____. This is the big advantage of looking down versus looking up in trying to get an analysis.

Adel: This particular parameter has rather interesting variations. Apparently short period variations and a satellite might be a good vehicle from which to monitor them. Ertor varies with a period of 18 days and also with a period of 10 days and the ten day periodisity appears to be confined to the upper portion of the stratosphere. The 18 day one appears to prevade the entire stratosphere. The total ozone amount varies with precisely the same periods and the westerly component of winds aloft apparently has the 18 day period. Three completely different sets of observations all happen with the same periods.

Fritz: Let's break up for lunch now

LUNCH

Dr. Fritz: We might as well start. We have one remaining talk left and that was the one by Mr. Wark from the Weather Bureau, who has been thinking about how we would use the TIROS measurements for example to get the heat budget of the atmosphere, and also he had some other thoughts along these lines.

Wark: Well before I get into the problem I wanted to state exactly what we are trying to accomplish here in the heat budget and we are not concerned

with the visible part of the spectrum today, so when I speak of the heat budget I'm speaking only of the infra-red portion. This is probably one of the most important things we are going to get from a satellite. Just how much heating goes into the atmosphere, because this is the thing meteorologists need in radiation. After all it is the energy that is absorbed that drives the atmosphere and causes the weather. So we want to find out just what this quantity is. Now, the heat budget of the atmosphere itself as a whole is the total inward and outward radiation from the top plus total inward and outward radiation from the bottom. The total downward amount at the top is zero. The total going out of atmosphere itself, not the amount of radiation seen from above, but the total amount coming out, I'll simply indicate as I'll just write it symbolically like this, the upward radiation at a given wave length and in the direction theta. This is a loss so it would be minus. The loss from the bottom of the atmosphere would be the downward radiation of the same sort. And the amount going into the atmosphere would be that contributed from the ground, which would be the emission from the ground, Bo, whether it is the black body or not is of no concern at the moment, times 1 minus the transmission through the entire atmosphere. Call this quantity H sublamba of theta. This is in a given direction, just an arbitrary direction here. The radiation entering at this angle and leaving at this angle. This is monochromatic. This is actually the quantity that we want to obtain. However, we can't observe this from the satellite. It is not observable. What we do observe, though is the amount leaving the surface and getting through the top of the atmosphere, which is B nought times T nought, plus the upward radiation from the atmosphere itself. So that we have this equal to B nought minus the observed intensity in the direction theta, wave-length lambda, minus the downward radiation. This much is known. These two things are not known. Unless there is a particularly clear part of the spectrum which we can't observe this emission from the surface, although perhaps this-what was it-8 wave number region.

Kaplan: That's 11 microns is the best window that you have, I think in the spectrum but even there near the ground particularly or in regions where there is a large amount of water vapor there is difficulty.

Wark: Now the only way that we can get these two quantities is to calculate them. The only way we can calculate them is by knowing the structure of the

atmosphere, knowing the temperature and humidity distribution. The temperature must be know with some considerable accuracy. I am not sure just what the accuracy would be in the water vapor in contributing to this factor here. This thing has to be calculated, it is simply the reverse of this problem here. If I had all the parameters put in I could calculate this upward quantity and I could also calculate this downward quantity. Before we can get this we have to go through the calculation. This might be done by some method such as Dr. Kaplan has suggested of going through some typical model atmospheres and getting a good fit finding deviations and try to get this quantity by differential methods. This then has to be, also, to get the ultimate quantity that we want to get which is the flux divergence of the atmosphere as a whole, we have to integrate this over lambda and over theta. I don't believe the wave length integration presents any particular problems, simply this quantity times times of lambda integrated from zero to infinity. The other problem, however, of getting the flux divergence in all directions, means that we have to know the dependence on theta and here, again, if we know what the atmosphere looks like we can calculate it. But this quantity here, which is the observable quantity, is definitely dependent on theta, since at large angles we are looking edsentially at the top of the atmosphere.

In summary here, I might say, that to get the heat budget you have to have data of the type which Dr. Kaplan spoke of which would get the exact structure of the atmosphere. Without it we cannot get this quantity. We have one other problem, we might want to get the planetary emission, which would be only this quantity here. This would be the emission from the ground and from the atmosphere. And to get this, however, we have to measure over a number of angles and a number of wave lengths and I don't think it is really a much simpler problem as I will indicate with the following:

In the TIROS project, we have certain filters which we use to cover broad bands. I'll discuss two of these. One covers the region from about 7 microns up to about 35 microns. You can see the plot of this, the general nature of it. What we want to get is the intensity in one direction in which we are observing. The thing that we observe is equal to the integral of S-lambda. This would be the sensitivity of the sensor or the transmissions of the filter, which ever, it would be a combination of the two. Times the true

intensity. Call this I prime. This is the thing we have actually observed. The quantity, the signal that we get from the ground, is something which is proportional to this. We know this characteristic, but we don't know the functional form of this. And without this functional form we can't get an unambiguous answer to the quantity that we really want which is I of theta which is equal to the integral of I of theta lambda, which is the total intensity in direction theta over all wave lengths. There might be some means of approximating the true values here but I want to point out that the use of this sytems does not give an unambiguous answer. In the window we have even more trouble. It is possible that we might be able to infer, with some accuracy, the total emission from the earth and atmosphere from this broad band here. In the window as I indicated, we are in much greater trouble. I have here a filter characteristic for the 8.5 to 11.5 micron window transmission. You will see that it is less flat than the other. It has a shape something like this.

As has already been pointed out, even in the clear parts of the window, we are not looking just at the ground alone. To call it a window is only partially correct. With the help of Mr. Popham in our office, I went through an integration on a very simplified model of the emission I would get from the top of the atmosphere, assuming a sort of mean absorption coefficients over wide bands, and I adopted the value in the window of about 0.1 centimeter squared per gram, which I think is a reasonable average value within this region, I get the following emission coming from the top of the atmosphere. I plotted here pressure, starting at a thousand milibars at the top, going to zero here and this is the intensity plotted in this direction and I have cut the atmosphere into segments.

Kaplan: Is this water vapor you are talking about?

Wark: Yes, this is water vapor. I have ignored ozone. Just speaking of water vapor alone here, I think it is important just to get this illustration. Now looking straight down, which is the black curve here, I got 3/4 of the emission coming from the surface and about 1/4 from a range spread from the surface up to 500 milibars, halfway through the atmosphere. I think below 500 milibars you are getting significant amounts of radiation. This red curve is looking at an angle of 60° that is, sec o is 2

and the radiation coming out of the top of the atmosphere was about equally distributed between the surface and the atmosphere itself. This has an average close to 700 milibars. The other day we talked about this region with him and I asked him what he expected to get from the measurements in this region. He said he expected to determine the whether there are clouds or not clouds. This is also a handicap unless you have also on top of this the emission from the ozone . It is a very large property in this area. But you have something coming from the ground, and I think now they think that what they are going to get is the difference from the ground radiation and the cloud radiation. They will be able to distinguish if they are over clouds or not over clouds.

Fritz: Well that was the idea, the difference in the energy is greater when you are over clouds than when you are not over clouds. I think the point is here that it is masked to some extent by the water vapor but you still may be able to do it.

Kaplan: But if you do get information that will be the only information you get because there are so many constituents in here.

Fritz: In some areas you won't be able to get that because of inversions and things like that. But in the tropics it may work out.

Wark: This is about all I wanted to cover. I wanted to point out these very strong limitations on the information we can gain from the broad band type of observation and that we have to have detailed information on the entire structure of the atmosphere, to get the thing we are after, which is the heat budget.

Fritz: On the other hand Dave, I wonder if this would be so if you had broad band filters which were flat, it still wouldn't give you all of the things you need.

Unidentified: No, but might we get a close approximation that way.

Wark: Well you run into some difficulties in the window because the window doesn't cut off in a nice sharp fashion. If it were flat over the entire spectrum then yes, you would be able to avoid this pitfall.

Fritz: What I am really trying to ask is this: if we were to say what do we want to measure, well what do we have to measure the heat budget.

Wark: To get the heat budget of the atmosphere alone you would have to know the details in the atmosphere.

Fritz: You mean you have to measure the spectrum in fine detail.
Wark: You have to do the sort of thing that Kaplan was describing with carbon dioxide to get the temperature distribution.
Fritz: Except you have to do it throughout the spectrum.
Wark: If you have the temperature, that is finished. Then the rest of the calculations are based on this. The calculations of the downward radiation.
Fritz: No, I'm thinking of the upcoming. The upcoming radiation alone.
Wark: Any special measurements for the heat budget, alone, you need the type of measurements that he has descrived to get the structure of the atmosphere. There is nothing special needed here.
Fritz: So what do we need to measure the upcoming radiation from the earth and the atmosphere using a broad band filter.
Wark: If you are satisfied with a broad band it has to be flat over the spectrum in order to get the total radiation from both the earth and the atmosphere.
Kaplan: If you want the actual outgoing radiation, from the top of the atmosphere from the point of heat budget, what you want is the radiation that leaves a horizontal surface, the hemispheric radiation that leaves a horizontal surface at the top of the atmosphere and the top of the atmosphere is for all effective purposes, as far as measurements are concerned, considerably lower than the height of the satellite. If you are at the satellite height, then the radiation that you receive is partly from the earth, partly from the weighted from the earth and you are not getting the actual integrated radiation at a horizontal surface.
Wark: What you have to do is find out what this angle dependence. It has to be done by measuring the same area from different angles.
Kaplan: You had to convince yourself that there is some sort of a simple angular dependence. Now this may not be impossible to do. But I have a question to ask. Why do you want heat balance? Do you want it for forecasting purposes? Do you want it to stick into a numerical model? If so, then I think the thing to do is to determine the structure of the atmosphere by looking down from a satellite and then actually calculate the outgoing radiation. Because in any case, if it is going to be used-yes, I know, it is essentially the point in what you were saying. There is

another point, and that is if you want to determine flux divergence, the differences between radiation, then you can not do it partly by calculation and partly by measurements, because you are going to have ~~xxxx~~ instabilities brought into your system. Particularly if it is a numerical model it is going to last a long period of time. You have to have a method for determing the heat budget that is internally consistent. Or at least you have to convince yourself that any method that you have must be one that is internally consistent.

Fritz: What if we have no reason, merely what if we just want to know the heat budget of the planet?

Kaplan: If you can measure it from a satellite, good. If you can determine the radiation coming in the vertical direction and from this introduce some ~~xxxx~~ sort of a factor or some sort of an empirical relationship, it would give you a radiation coming out side the atmosphere. This is good too. This is something that should be looked into.

Fritz: Well I think this is one of the things that Dave mentioned that he had been working on.

Kaplan: Now there is also a question of time scale. If you are going over a long period of time, say, even an average of a day, then you can make some sort of assumption about the ground. If you assume the ground has a small heat capacity, then all you want for the heat budget is the radiation leaving the top of the atmosphere. Over the ocean there would be difficulty.

~~XXXX~~ Elsasser: There is this tremendously important problem of cloud climatology and I just don't think (with my limited knowledge of the atmosphere) but I feel and many people feel the same way, that the least known variable in the atmosphere is the statistics of clouds. In middle latitudes higher latitudes, they cover a large fraction of the sky, I don't know if its 50 per cent, but something of that general order. And, of course, if you have a cloud your heat balance is radically altered. Instead of getting outgoing radiation from some lower levels here and from the ground you get if ~~xxxx~~ from a high cloud which has a low temperature and you get very substantial effects which are much bigger than any of the differential effects as a rule, which Lewis has calculated.

The differential effects are important, but you have to remember that when you look down in the atmosphere, any result about the stratification of the atmosphere which you get, thermal or otherwise, is the result of a differentiation, in the mathematical sense of the work. XM Any differentiation will immediately reduce your accuracy, and everybody knows that. A numerical differentiation alwasy leads to a terrific loss in accuracy. I personally feel, I don't know if anyone ever calculated it, that as far as the gross heat balance of the atmosphere is concerned a 5 or 10 per cent change in cloud cover, is probably more important than a change in the water vapor content of mean temperature of the cloudiness atmosphere. It would be very easy from a satellite, this is the one thing I'm sure you can do, is to measure with the simplest instrumentation and get cloud statistics both in effective radiative temperature of the cloud, and just a gross distribution of the cloud. You know the albdeo of the earth all these things are very crudely determined, so if you can get for weeks at a time cloud statistics and correlate there with the general circulation, I think you have really got something. What I suggest as a possibility, for discussion let's say, to start out with a scheme in which the first thing you measure is to measure cloud properties from satellites. You get the experience how the thing will go. This gives you, instead of having to do differentiation, you merely have to do it in steps and then after one got the experience of how this works then you go on to the finer details which you have been talking about.

Fritz: We are going to have TV cloud pictures, of course, but for another purpose than the heat balance. But why can't we measure the heat balance in the way Mr. Wark has just mentioned, which integrates the _____ you don't have to know the details of the cloud statistics, but it tells you the reflectivity, the energy reflected by the clouds. As far as the heat budget is concerned, that's what you want, isn't it?

Elsasser: All I'm saying is let's concentrate on the clouds, make an attempt on the cloud problem before we go into specific details such as, let's say, the slope of the atmosphere.

Fritz: I'm afraid I don;t understand. To measure the temperature distribution, the water vapor distribution and things like that, they could be done for quite a different purpose than the heat budget. We just want to know when maybe a thunderstorm is going to occur someplace. We probably won't get the satellite to get us that detail but we want to know the general distribution of these properties in the atmosphere aside from any contributions to the heat budget. We have the heat budget problem and this we are hoping can be measured directly by broad sensors. We would have a broad sensor which would not pick out whether there is one cumulus here or there or anything else that would actually measure the energy emitted by the earth at least to the extent.

Elsasser: What I am trying to say is that it would be very valuable if you could get a statistics for a function of latitude, cloud height and effective cloud temperature. This would be about the most valuable datum and I think you do need infra-red measurements to get this. The cloud temperature I think is the most thoroughly important item in the stratification, the heat balance and the dynamics of the free atmosphere. And this is the least well known. If any one wants to contradict me on _____.

Fritz: That it is not well known, I think I would agree, I am not in but owe my mind clear that it is necessary to measure that for the heat budget, but for some other purpose perhaps.

Goody: The first thing is what we do propose to do. If you're just going to send a satellite to measure the radiation as a function of latitude, this is fine. You can do this obviously, directly with good enough instrumentation. . But now science doesn't stop at this point. You have to understand to some extent, you have to understand what is going on in the earth's atmosphere. And these detailed analyses which we have been discussing, we ought to get some understanding of the problem to interpret this in terms of some other atmospheric parameters. Now what Dr. Elsasser is saying is that the single most important one is probably cloud amount. Let's concentrate on that first and I think there is a lot to say for this point of view. I think it is a very valuable one. Probably if we understood cloud heights and temperatures and how they occur we would have something like at least half of the variables taken out of the atmospheric radiation problems. Something like this all but the one parameter if we can understand it.

Fritz: Yes, I would agree with that point of view, but I thought that perhaps you were trying to say that this is how we would get at an estimate of the heat budget, for example.

Elsasser: I am merely saying that this is the first step and so Goody says fully the biggest step and from the instrumental view point certainly the easiest.

Goody: Well it's not really so simple. If you want to measure the cloud top you have to get between two of those lines. That could be done but it's not just absolutely simple.

Kaplan: And of course there is always residual absorption. You have to cope with this and you have to take some measurements to make sure that you are really measuring the cloud tops.

Goody: I agree with Lewis.

Kaplan: The temperatues at the tops of clouds, cloud heights and cloud temperatures distribution is probably the most important information we are going to get from this as Dr. Elsasser pointed out. It is probably the easiest to obtain. I have tried in connection with some work on the circulation of the stratosphere in the equaterial regions. To try to get an estimate of what cloud top distributions were or cloud top temperatures. I could not get this. This is not known. It is extremely important.

Gates: This will also outlive the daytime measurements of cloud cover by TV cameras. This will give you your night time coverage, your dark side coverage.

Goody: This brings those together so you can see what kind of clouds you are measuring the temperatures of.

Fritz: I think that this is probably as far as we should go in what you might call the theoretical requirement determinations and I was trying to summarize this thing, but maybe my notes aren't in good enough shape to do it. What we have heard is for things like water vapor. If we are going to measure water vapor in the troposphere anyway, we might want to make measurements, if we were doing this in the infra-red, near 23u with resolutions maybe somewhere between 10 and 25 wave numbers. If we were going to do it in the stratosphere there is some disagreement on that but there was a suggestion that if you look at fine details in this same region of the spectrum where I think

you said something like one or two wave numbers, there would be some hope of getting the water vapor in the stratosphere too. In the case of ozone, we had better look somewhere in the 9.6u band and measure some specific region in the spectrum, I've forgotten the numbers now, I had it written down.
Kaplan: Well, throughout the whole band I think Goody mentioned 5 positions, 1 or 2 wave numbers in the vicinity of the 9.6 micron band, no worse than 2 wave numbers.
Kaplan: The number of frequencies would prbbably be somewhat different looking down than it would be looking up.
Dr. Fritz: There were two suggestions, one was Kaplan's suggestion looking straight down in the CO_2 15-micron band, with 5 wave number resolution and using 10 measurements with resolution of 5 wave numbers at around 15 microns. Jean King suggested doing this with perhaps a broader type of spectral resolution, but looking at different angles, but didn't, as far as I know, resolve the question of what you do about looking at rather far distant points. So you have the angle question. In the case of heat budget, it had it's own little problems, but I had the feeling that we were thinking maybe that if we had a broad filter with very flat characteristics that this might be one way that we would at least get an estimate of the heat budget, although theorectically we really need to have the whole spectrum and integrate over it, in order to get the resolution with angle. In other words, if we measure at one particular angle, we still have to get the distribution with angle and at least, in theory, we need to have complete spectral distribution to get that. But perhaps empirically we could hopefully make a measurement at one angle, with a filter which was broad but flat, and then hope to get the angular distribution from emperical results made with some theoretical guidance.
Then there was the discussion just now on clouds and, whereas we may be able to detect the presence of clouds with the window type thing even if its as crude as that, because the emission from the ground and from the clouds would be rather different, especially when the clouds are high, even though water vapor mask distribution. Still this would probably be the maximum information you could get and I suspect that it wouldn't work very well in polar regions but it might work in the tropics.
Question: Why doesn't it work in the polar regions?
Fritz: You might have too many inversions. In other words, you might have

a reversal for example. It might be colder in the clear areas than in the cloudy . It doesn't matter as long as you know, but since you may not know you may have trouble telling whether you are looking at a cloud or an ocean or a lake or what it might be.

Goody: You know you are looking at the surface, which has a certain temperature.

Fritz: Which is different from the temperature from the surface near it. You wouldn't know which was cloudy and which was clear, for example. In the tropics it seems like it might be less ambiguous. If you look with

Goody: You would assume ultimately that you would record them visually at the same time.

Fritz: Well at night you wouldn't. In the daytime, for the present at least we are going to have TV pictures which will surely be superior to anything we can get with this kind of a filter. But at night we would depend on this for some idea of the clouds. The resolution spacially is going to be very crude. It is 30 miles or something like that, so that it will only tell us roughly-it will tell us large scale cloudiness, you might say, and then in the ~~xx~~ polar regions I susgpect you might have trouble.

Question: What is this 30 miles? Is it infra-red or television?

Fritz: No it's not television. The angular opering is 5 degrees. This is a tenth of the height so that is 30 miles. For 300 miles it is 30 miles. That's looking straight down. If you're looking an angle it becomes worse.

Question: Just to clear this up the present one that's on the Vanguard is 1°.

Fritz: Oh, the Vanguard II, I'm talking about the TIROS package. Your angular opening is 5 degrees. Of the 8 to 12 micron thing you're television is entirely different.

King: Trying to get in a last word here to buttress up Dr. Elsassers arguement this paragraph surprised me. It says ~~studyingx~~ studies of the drifting station alpha in the Arctic ocean indicate the great importance of long wave radiation from the overcast skies on melting snow and ice. During the melt season nothing proceeds much more rapidly under the overcast than it does with clear skies and bright sunshine. With clear summer skies we found freezing frequently occured.

Fritz: Well of course this is what we would expect fo find if you stop to think about it.

Elsasser: Well I think there should be some way of discriminating, even in the Arctic combining optical and infra-red measurements, whether you have cloud top or ground.

Fritz: I think we should turn now to the instrumental part, mainly we have assumed the requirements. To measure water vapor in the infra-red, we want to measure them in the 25 microns with resolutions of something like 10-25 wave numbers, to measure ozone we want to measure in the 9.6 micron band with 5 wave lengths, with resolutions of one to two wave numbers. To measure the temperature we would like to measure, in the 15 micron band, 10 wave lengths which resolutions of 5 wave numbers. The question of doing it at one wave length with angles cam in. Heat budget-hopefully we could get away with a broad filter if the filter was flat. In the stratosphere a suggestion was made that we might be able to measure the water vapor by looking with 1 wave number resolution in the region near 25 microns. It seems to me that it is up to the instrumental people to tell us whether or not they can deliver this kind of thing.

Howard: I would start with the very first point. I made a very quick calculation which may be all wrong. But even assuming that what you are looking at is -65°, if you considera a one micron interval around 25 microns, 2.7 per cent of black body radiation would be there. Only 6 percent would be in the 1 micron region between 5 1/2 and 6 1/2 microns, which is only 1/4 as much. But detectors must be at least 100 times better, which means that one should not so quickly abandon the 6 micron region.

Shaw: Which detector is the better?

Howard: Well I would say gold doped germanium or something like that.

Goody: _____ _____ _____ _____ worse signal to noise don't we.

Howard: Oh no, I would think in absolute sensitivity.

Goody: The sensitivity is terriffic, but the noise is _____ _____.

Fritz: Now you are suggesting a certain type of filter with a -.

Howard: I am saying that you shouldn't abandon the six micron region just because there is only 1/4 as much energy roughly in the same wave length interval.

When detectors are much more sensitive down there. This is true for even -65 degrees kelvin. For any higher temperature it would be better. I gave what I thought was the worst case.

Fritz: Now you are talking about detector. Do you mean also to include something like a thermistor?

Howard: No, this detector has uniform sensitivity we can say throughout this region. By going to 6 microns there is hope now with the present/kind of detectors of using something other than a thermo detector, out to say 6 microns.

Question: Provided it is cooled too?

Howard: There is hope for satellites that live for only a month or so, of cooling something that does not require liquid helium. I would exclude zinc doped things or arsenic doped which go farther out, but gold doped or something which can be cooled just with liquid nitrogen, which could be packaged-it's about the size of a pencil, the cooling device-and could be run for a month or so. You shouldn't exclude that. You are putting enough money in this package so that the additional couple of thousand for one of these is not much?

Goody: Are you sure of these figures? Are you sure if you take a thermo detector and cool it to the same temperatures you get no worse noise, you get better signal to noise with gold doped germanium. I mean is this thing measured?

Howard: What you have to compare is the gold doped germanium at 6 microns compared to a thermistor at 25.

Goody: No, let's take them both at the same wave length, 6 microns. Are you sure in fact that if you cool them both at the same temperature you get a better signal to noise out of the germanium? The end type germanium I think is slightly ahead. An enormous sensitivity but how about the noise? The noise is terrible.

Wormser: The signal to noise, the noise equivalent is slightly ahead of thermistors, or golay cells.

Goody: Is it ahead of a thermistor cooled to the same temperature? This goes down with the square root of absolute temperature, the noise.

Wormser: I'm not sure I can answer you there.

Gates ? A thermistor is noisy compared with a thermo couple.

Goody: Yes, a thermistor is a 100 times worse than a thermo couple.

Wormser: I disagree with that.

Howard: Noise equivalent power is signal to noise. A thermistor bolometer is something like $7x10^{-9}$, is what they list as kind of a haven's limit or something like that. I am sure that these are at least an order of magnitude better.

Goody: And a thermistor is at least a 100 times worse then a first class thermo couple.

Wormser: He was talking of a very slow time response.

Goody: Well a ~~thermm~~ bolometer and a thermo couple each one will run about the same. About 10 per cent of.

Howard: Doesn't the response time enter or are you taking advantage of the fact that the thermistor responds very much more quickly.

Goody: Well it loses a bit on that.

Wormser: Well actually if you take equivalent band widths, if you integrate the output from the thermistor, you get within a factor of two of the performance of that of a thermo couple. The difference comes in that the thermistor is usually used with about 100 cycle or something like that, chop rate and a thermo couple a very slow-

Goody: Well a good _____ thermistor and a good Hilger Schwartz thermocouple best thermocouples, we have thermocouples 10th second per erg which can be chopped to ten to the minus second just an order of 10 times better. ~~In~~ I only suggest that before you get into other things one checks these things.

Howard: John Shaw didn't think the Hilger Schwartz was 10 times better then anything else. Was it?

Shaw: I would like to make one point? Is it feasible, I don't know. Was it feasible to put the thermocouple into something which suffers these tremendous accelerations) initially or not these things are very fragile. Maybe you can.

Fritz: I think this an engineering thing which probably we should not consider here. Let's just consider it on the optical merits.

Goody: Can we bring one thing into focus. There is one real advantage in being in the six micron region. No one can argue against that. That if

you use indium antimodide you can get really fast response. You don't need to cool it. You can go to milisecond response and so for satellite applications this is important. Indium antimodide has about the same signal to noise as the thermocouple.

Kaplan: What is the latest on gold vapor _____ lead sulfide now. I know that Flyler is using lead sulfide in the 6 micron region. ~~XXXXXXXXXX~~

Howard: He is pushing things a little. Most people would switch to lead selanide in that region because it is about a factor of ~~fifty~~ ten better

Kaplan: He claims he's getting better response out of lead sulfide now.

Goody: At 6 microns.

Kaplan: At 6 microns, yes.

Greenfield ? Lead sulfide falls off roughly one decade per micron beyond about three microns.

Kaplan: Yes he's using lead sulfide in the 6 micron region.

Wormser: With any sensitivity at all.

Kaplan: Yes, he claims to get better sensitivity with lead sulfide.

Fritz: What can we conclude here, if anything.

Gates: While we are speaking of the 25 micron region, in our experiment we are immaging the sun on the slant. We are using half a wave number resolution. This is 4 times less resolution than we are think of here. Oh, it's a good deal less than that. You weren't speaking of 2 and -1 10 or 20 cm-1.

Goody: Wel two is worst that has been mentioned.

Fritz: It depends on the problem. To measure it in the stratosphere Lewis is suggesting something like 2, but to measure it in the troposphere 10 to 25.

Gates: We find that with half a wave number at 25 microns, the sun as a source, we are pushing signal to noise to the limit with a thermocouple. Now, if you want to make a comparison we can open up the slits here so that we have the 10 wave numbers, so there is a gain or factor of 20 times. So squared there is a gain of 400. Now you go from the sun to the earth, in sources. So you go from a 6,000° black body to a 300° black body. So there is a factor of 20. So we got 400 in one case.

Goody: Well, if you would like to calculate it, why notwork it for these emission spectrums which are much more directly the thing. You've only

got to change a very small-

Gates: Well this would indicate that it might be feasible with a thermocouple, except for time constant considerations.

Fritz: To measure in the 25 micron region, but Howard was suggesting that we might do it better in the 6 micron region.

Howard: Am going to say that you not only detectors to think about but if you want filters that are going to isolate these narrow spectral regions our filter technology is much better around 6 microns than it is at 25.

Fritz: What did you use with your quarter wave number in the sun.

Gates: Oh, the spectrometer, the grating.

Fritz: In other words you may not be able to use filters.

Wormser: No one had made one that far out yet as far as I know.

Fritz: But you don't use a filter, you use a grating

Gates: You can use filters, R. Strohm techniques or something.

Howard: Oh yes, R. Strohm .

Fritz: But you used a grating you said. As I understand it in the 6 micron region, we struggled with this on TIROS, if you want to get something like, we didn't discuss what wave number you need, but if you need something like 10 wave numbers we are not able to do this in the 6 micron region at the moment.

Hoard: You are lucky to get a tenth of a micron or two tenths of a micron filter and that is pushing the state of the art.

Wormser: If you want to see the best available table I have it right here.

Fritz: Well I assumed that we got the best on Project TIROS.

Wormser: It's more like a micron I think.

Question: Who built this?

Wormser: This is IR-Industries, this particular one, bet we have checked.

Howard: Have you checked with this fellow in Santa Rosa? He claims that he can do a tenth of a micron or so around 6 microns, but it's an art still to make one of these things.

Wormser: I think you will have to remember that TIROS project, the infra-red part that was initiated around December of January with hardware to be ready in May. This is not necessarily representative of the best state of art, but it is representative of what could be put together in three or four months.

Fritz: Let me ask this. If we needed this kind of resolution at 6 microns apparently with gratings it can be done, as I understand it, is this correct?

With filters it might be possible that it could be done, from what you said.
Wormser: Not two wave numbers, no I think we might appraoch a tenth of a micron.
Fritz: If we need 2 or 10 wave numbers then we have to go to a grating.
Howard: This grating is going to have to have windows and things and here you are getting in trouble again. Perhaps ~~xxxx~~ it"s not so bad if its just a little window, but if what are you going to use at 25 microns?
Fritz: I don't know, what did you use in a balloon?
Howard: KRS-5?
Kaplan: Why cain't you use KRS5?
Fritz: Just a minute, what did you use in your balloon?
Gates: You can use a number of things.
Fritz: Why cain't you leave it open?
Goody: You can leave it evacuated to space.
Fritz: John was raising the question that you need a window but if you don't need a window I would think that you would be better off. 1 You don't need a window so what's the.
Howard: These are not perfect vacuum where your satellite is however, and your bolometer and such things are going to have to be enclosed.
Goody: You don't have any dynamic heating does it, this thing, otherwise it would be down in the next few minutes.
Shaw: Goody made a point this morning when he said that the 500 pound payload that it's easy to build a spectrometer with that limitation very easily. Greenfield? Why couldn't one use a double prism monopromoter and recombine the radiation after it leaves the first prism. Use the slit to limit the band pass. Then it would be combined into the slit image and place that on _____.
Goody: You get better performance out of a grating thought. You can always get better performance out of a grating, cain't you?
Greenfield: H If it's as mechanically stabile as the 2 prism combination/
Goody: I think that as long as John Shaw raised this question I cain't help thinking that is the thing we can decide quite precisely. When you have got an enormous object like this with such a weight, why make life difficult by using anything but a good spectrometer, they don't cost anything.
Fritz: This is really what I am trying to get at. This is the feeling I have since when it comes to instruments, I am sitting on the outside listening.

Fritz: I have this feeling that if these are the requirements that you are trying to meet, namely something like 10 wave numbers, in the 6 micron region or the 25 micron region or anywhere, I have the feeling that filters are out. That filters cannot produce, but that gratings can.

Barnes: Call it spectrometers, whether its a grating or a prism spectrometer.

Fritz: I don't know if prisms can or not, but let's say that prisms can.

Goody: Well I don't think you will manage the one or two wave numbers in emission with a prism, not without a very very long time constant.

Gates: I think the other important thing is that you get a lot more information with a grating spectrometer. More than with a filter system. You learn a heck of a lot more because you have a lot to check points through the spectrum. You get the details. You get the envelope. You get a lot more than just the region you are isolating with the filter, you see. This is very valuable in the experiment, because by scanning across a section of the spectrum this information gives you a much better basis on which to give good interpretation to the data.

Fritz: There seems to me that nobody is disagreeing then with the thought that filters cannot do the job but that the spectrometer, gratings for sure and prisms maybe, can do it. Therefore, it seems an abvious conclusion that we have to go th gratings if we are going to do the job at all.

Elsasser: I want to raise one point to the experimentalists. How long will it take to design a spectrometer that will withstand 50 years? Do we have to start from scratch?

Gates: The spectrometer is no problem. The only problem is the detector and the electronics.

Elsasser: Can you make it mechanically stable enough to withstand 50 g's?

Gates: Oh, yes, I think so, Yes let me put it this way Grating spectrometers have been flown in the ultra-violet by rocket, many times.

Goody: They weren't double slit adjustment, but this is a small matter.

Fritz: I think we're getting to the engineering problem again. The important thing is, as I see it, that if we're going to meet this requirement we have no choice. The only choice we would have is grating or, for instance, in another meeting, if it were decided that for engineering purposes it couldn't be done, we would have to be satisfied with filters. Then let me ask the question,

"How can filters be interpreted? As of right now, not having looked into the engineering thing, I think we must conclude that we have to certainly look very hard to try to get a grating spectrograph.

Goody: You've got three or four people who have spent their lives building spectrometers, so we can speak with some engineering certainty that this instrument can be made.

Fritz: If there are any questions let's refer that to another meeting.

Question: What is the time constant of the scanning spectrometer?

Fritz: This is an important point which I thought to bring up.

Goody: Shall I show a picture of a spectrometers performance? Then we could talk about that if you modified it you could sppeed it up if you brought it down in size. (showing slide) This was built as a prism spectrometer and this is why when it converted to gratings it had to have two gratings in it. This is an essentially small grating spectrometer in which that is about 18 inches and that is about a foot. There is a typical size instrument using a 7 by 5 centimeter Merton grating, which is blazed at 10 microns. It has 60% reflectivity at 10 microns, and one's working on the one octave delineated by the filter here, which is indium antimodide and a golay cell, and the performance of this instrument varies. With prisms in, this instrument would give you about 10 wave numbers resolution in emission with a reasonable signal to noise. With gratings in it would give you about three wave numbers, that is this half here. That will give you about three wave numbers in emission (Next Slide) If you put it on the sun this is the performance of that instrument with the prisms in and it gives you about five wave numbers resolution and this is what this region of the spectrum looks like, whether in emission or absorption with five wave numbers resolution, for those of you who are not use to this region. Many of us look at this kind of thing day and night. Perhaps some of you are not familiar with the ozone band and here is the water lines, CO_2 band water band here. This is about five wave numbers resolution. This is actually taken looking at the sun with a prism spectrometer. If you put gratings in that other spectrometer, the one that we just had up, and look for the sky you would get about this performance, roughly. You notice it doesn't pick out any detail.

Question: Did you say how many lines you have on your grating?

Goody: Oh, it's a hundred per mm. (Next Slide, please) That instrument with rockett prisms in gives this performance. Now both Prof. Adeland and Dr. Shaw get a good deal better than this with prisms with slightly bigger instruments, but this is that region looking at it emission with about 15 wave numbers resolution and better spectra than this have been obtained with slightly bigger prisms. The prisms are rather small in this instance and it wans't too well adjusted. There is the ozone band. There is all you've got left of the water structure. That's one blob of water there. (Next Slide please)

Fritz: Are you going to tell us how long it takes to scan the spectrum?

Goody: All these spectra are being taken. Virtually we standardize. We do almost everything. But the time comes to about 1 second and the signal to noise is about 100. This is virtually standard equipment we use and unless I say something different. To get across there would take perhaps 30 to 45 seconds. This is just one region of that last spectrum, still at about 15 wave numbers resolution in emission at varying air masses. This is roughly what everything looks like at 15 wave numbers. We can go to 3 wave numbers with the instrument I showed you first and we can see something better in a moment. (Next slide, please) That instrument using gratings, of course, only embraces one octave. Now you can, of course, put in 4 prisms. If you are very short of energy a filter has its advantages. This is the cutoff of an indium antimodide filter at 7 1/2 microns and we have now filters bloomed with lead chloride to give us 98% transmission here and complete the metallic cutoff here. With a one layer blooming we can 98 per cent transmission at one point along here. It will be over 60 per cent for the whole of the octave and 98 percent at the major points, perhaps up to 70 per cent over the whole octave. Now I think they are pretty handy filters for some of this work. There is no difficulty in building a 4 prism, but it is another set of mechanical parts to go wrong, and it's conceivably worth economizing on this kind of thing in satellite work. Next slide please. That instrument, turned on the sun, that instrument about this big with a grating, turned on the sun, will give you approximately this resolution in that region. It is about half a wave number. That's for a little instrument about this big with a 5 by 7 centimeter grating in it. This is pretty good for solar work. All of these lines

are completely isolated, so, for studying absorption of lines, this is quite a good resolution if you were working on the sun, but we are not working on the sun at the moment.

Goody: Let's go now to the emission of the next instrument. This is the one with which I would suggest we could scale down from because we know the performance of this thing rather well. There is a six inch square grating with an indium antimodide filter and you see the size of the thing, that is a foot. It is quite big. This raises the point that, with this kind of instrument, if you want a reasonably small looking out angle it has to be rather long. The energetics of the instrument doesn't depend on the length of thid throw here or the length of this at all. It only depends on the size of that grating. But if you want to match it to looking at a small part of the earth's surface, then you have to get that matching either by preliminary optics or, if you want to do it directly, you simply have to have your main mirrors here a long way back. It has the same signal to noise performance but it gives you a small angle of view and a longer slit. They compensate precisely.

Fritz: What angle of view is built into the one you have there?

Goody? That is six inches at five feet, whatever that is 6°. That is using a Golay cell. The important things on instruments of this size are, first of all, the detector; secondly, the solid angle with which you fill it; thirdly, the size of the grating; and fourthly the time constan t. We can scale from these things and you can redesign this instrument just as you wish, but you are stuck with this end of the thing. So that the only things you can play with are the size of the grating and the time constant to go from the performance that we will show. (Next slide). We can skip that, its not really relevant. That is only the normalizing equipment. Presumably this instrument - you want to have something like this in where the instrument can both look at the sky and then at black bodies. I think this es essential. It is not difficult toarange. Next slide. Well this brings us back to this spectrum which we can get with the signal of noise of 100 to one with a resolution of one wave number and a time constant of one second, with the instrument we had out. Now then we can go from this to any other condition you like. If you go to say 3 wave numbers, that gives you - you can open the slit three times further that gives you nine times the energy and you can come down to one thirs the

size of the prism, if you like, and you can come down to a two inch grating. Then you can scale the whole thing down to that two inches and you can bring the whole equipment down to a third of the size, getting with this same time constant the same signal to noise. If you want a more rapid time constant then we could go back to the six inch grating and I think we get five if we put the time constant up by a factor f, then we can reduce the grating by b to the 5 over 2. I think it's 5 over 2.

Question: Would you contemplate sweeping out a spectrum or would you have several detectors in the exit plane to observe certain frequencies?

Goody: Well, that's for Lewis to say. He insists on, I think, that you actually have to absorb five regions silultaneously. That could be done I suppose most ideally with a Golay multi-slit, which I've never used. Have you ever used a Golay multi-slit?

Kaplan: In any case, what you would want would be something like a single entrance slit, the prism, then have detectors lined up in an array, as a series of exit slits.

Goody: Do you mean to say that Golay does not recommend his multi-slit for this job?

Kaplan: No, he does not.

Goody: In that case you want a multiplicity of detectors. Well, this is no serious problem.

Fritz: I personally think that it still would be possible - this is a detail - it would be possible to use one detector and scan. It would not be simultaneous. If we are going to look at areas like 50 miles by 50 miles, which we may have to anyway, to do a quick scan you may have an overlap at both ends of five miles and you are looking essentially at the same area.

Dr. Goody: What is the time constant of a second in miles?

Fritz: Well - it depends on the height and so forth.

Goody: Five miles in a second - if you've got a 50 mile resolution you can have a one second time constant.

Fritz: You can scan, in other words, with a single detector provided if you do it in something like ....

Goody: You could have this time constant too, one second time constant. In other words, there really is no reason for everything you've been saying. You coul

put exactly that instrument I showed in your satellite. It's about the right size it's lighter than your weight and it has the time constant. You could get this performance, which is one wave number, which is twice as good as anyone has asked.

Kaplan: Is it much easier to scan than to have this multiplicity of detectors?

Fritz: As I understand it, if you have a lot of detectors which have to be matched so carefully, you run into problems, whereas if you had a single detector that problem is avoided.

Howard: You would always like to know what's going on between them, too, when you go to interpret thedata. Whereas, if you scan you have some feeling on what's going on between them. But now if there are variations in the emicsivity of the surface, when you are getting contributions from that, you are going to have to assume this is a much slower variation than your speed of scanning.

Fritz: When you say one second time constant - probably everyone knows what you mean except me - and you are scanning this spectrum which may be varying considerably, does one second mean that it takes a second for it to get up to some fraction of the displacement, or what?

Goody: Well it's got this unfortunate Brown recorder on it, which is not an exponential device.

Gates: This isn't really the right number to use. The time constant of the detector is a tenth of a second.

Fritz: This means more like it takes a second to scan over the spectrum.

Howeard: A second to come to 65%.

Fritz: Well, if it didthat, if that was the definition, if it took a second to come to 65% of its equilibrium balue and you have a spectrum that's doing this kind of thing, then how will you ever get to the right value in the time of a second and scan the whole spectrum.

Goody: Well, it comes to 90% in a fraction of a second, with this detector. Not the whole spectrum, of course. The whole spectrum takes about an hour to scan.

Gates: As I understand, this is a second per data point. Three to five cm-1.

Goody: Yes. It's probably, each wave number, a little bit of overlap is used, each wave number is probably taken over 2 or 3 seconds.

Fritz: Each wave number is taken 2 or 3 seconds?

Gates: If I get the definition right, it takes one second.

Goody: Yes, that's right. There's a bit of overlap. The resolution is one cm^{-1}, so perhaps it's not just one second for each wave number but two seconds.

Gates: On our balloon equipment, in the near infra-red using lead sulphide, which is a different problem from this one right here, but we covered from the visible to 2 1/2 microns in 6 seconds and this was fairly slow. We could have appeded this up.

Question: This was lead sulphide, you said?

Gates: Yes. This is not degrading the resolution of the spectrometer, which is 10 cm^{-1}. This is an awfully fast cell. You can do this with any photo-conductor.

Howard: That's why I think you might better go back to thinking about 6 microns.

Fritz: You showed one slide which went from something like 9 to 11 microns, cut off rather sharply at those two points. You mentioned in that one that it took 45 seconds to scan. The two micron region took 45 seconds.

Goody: That was at low resolution.

Fritz: I see, and with high resolution?

Goody: With high resolution you have to do more and more points.

Fritz: Let me ask this question then. If you were going to get the five points in something like one wave number resolution in your ozone band, which I think was the thing you mentioned would be required, how long would it take to get measurements at those five wave lengths?

Goody: A few seconds.

Fritz: A few seconds for each point.

Howard: So it would be better to have a band of detectors.

Fritz: You have to have a band of detectors and even then it would take a few seconds. You would go maybe 15 miles.

Howard: You can stand probably 10 seconds.

Fritz: No, you can't stand that much.

Greenfield?: You would have gone 50 miles over the earth in that time.

Fritz: Yes you would have scanned over the whole region.

Barnes?: You have a battery of detectors, why does it take you a few seconds if you have a battery of detectors? You get your answer pretty quick.

Fritz: No, he says it takes a few seconds per j point.

Goody: What I am saying is that we have a detector with a time constant of one second, and with a time constant of one second you need two of these seconds to get a nice ..

Barnes?: You get all g five points simultaneously so the whole job takes you just three seconds.

Gates: Furthermore, am I not correct, these five points are spaced apart so there is some territory in between, so it takes longer, five times as long to scan.

Fritz: But you have five detectors.

Gates: If you have five detectors, thats different.

Fritz: It takes you ~~five~~ three seconds to get one set of five points.

Greenfield: You're not going to worry about a blur of 15 miles are you?

Fritz: We might or we might not, that hasn't been decided.

Gates: The detector that Richard used was a Golay cell. Now that is one problem and one order of magnitude on time constants and scanning rates and is indeed what you might have to used at 25 microns. But if you get into the photo conductive region, then your time constant changes by another order of magnitude or so.

Goody: But to take advantage of it, you've got to throw away signal to noise. You see, this is energy limited, this noise is. So to take advantage, because you haven't got more sensitive detectors, so what it means is, these spectra with the Golay cell you couldn't speed up too much, because you would then run into the fundamental time constant of the detector which isn't so far off from the one second that I'm using. With a photo conductor you wouldn't be any better off with signal to noise, but by opening your slits you could drive it farther in time constant.

Gates: You might reduce it by, what, a factor of ten.

Goody: You couldn't drive this too far. It's no good ~~in~~ saying we just open the slits and have a shorter time constant. You should go to a quarter of a second and then you would be finished. You arrive at the fundamental time constant of the detector, but with the photo conductor you could go swapping resolution for speed for much longer waves.

Fritz: Well if you went to a quarter of a second, even if that was the limit, if you could do it, that would be satisfactory, wouldn't it.

Greenfield:? Wait a minute, I think you're kidding yourselves on this. Yov've got a fundamental problem of stabilizing the vehicle. The problem of stabilizing it is probably not going to give you better than a degree, one to three degrees, which is going to give you a fundamental blur to start off with; and trying to push this down do a quarter of a second because you want to get very detailed or keep it fixed on a point on the ground within less than a mile, it's sort of rediculous. If it is going to cost you anything in terms of your instrumentalities, this might be a fundamental limitation on how far you want to push this thing.

Fritz: If you had one degree stabilization and if you were at 300 miles for example, you would have a smear of six miles.

Greenfield: Well, then why try to push it down below that.

Fritz: Well, I didn't. One second will take you to six miles. If you take three seconds which is giving you three times as much. Well, now let me make this clear. I'm not trying to set myself up as an authority on whether this should be 50 miles or 100. But if we take this as some guide line and this was the kind of thing I was talking about, it might be that we would be quite satisfied with having data over a hundred miles by 100 miles in which case the smear would be substantially greater.

Greenfield: I should point out, that getting one degree stabilization is just one hell of a problem, and you're not going to get it in a 500 lb. package.

?Staats : Yes but you will find that that time constant of one second of that one degree will be much longer than they're talking about here. During the few seconds I don't think it will vary.

Fritz: Well I think anyway that the picture is somewhat clarified for me. Namely, that with the instruments that Richard showed you can get measurements to something like one or two wave numbers, certainly if you satisfy with measurements that may take three seconds if you have a detector in each one of the wave lengths that you want to send. It might be possible to do somewhat better by revising the spacing between things or so on.

Goody: You could throw away resolution and cut the time constant down to a half or a third and get information in a second. You could get that far with this thing.

Fritz: So it seems to me that you could say, from that point of view, that it would be possible to meet the theoretical requirements in that region with this particular type of instrument. Now Howard still feels that maybe we should go to six microns and I'm not all together clear if you use these photo conductive surfaces whether or not this damages things or not. You were saying, I think, that you could go to that other region and get perhaps faster time constant if you needed it. If you needed to go faster you could go to that region of the spectrum with somewhat more noise perhaps ..

Goody: Maybe John is right, he should know more about this. It was my firm impression that it is a hell of a job to beat the thermocouple if you treat it in the same way as your photo conductor in this region.

Howard: When we say more sensitivity, we mean more signal to noise when we talked about thermal detectors in this region the absolute limit of the best kind of Golay is 10^{-11} watts per cm^2 . Then an actual detector is maybe

an order of magnitude away from this absolute limit, so 10^{-10} say, is about what you can expect as a minimum signal detectable. With these gold doped germanium sensors you can go perhaps to the 10^{-12}.

Wormser: You can go about one order of magnitude better for the end type gold doped germanium at six microns.

Goody: With the same signal to noise.

Wormser: Cooled to liquid nitrogen.

Goody: What about when you cool Golays to liquid nitrogen.

Fritz: This gold doped germanium, is it going to give you the resolution we need or does this have a broad band?

Wormser: It's just a detector.

Fritz: I see. I thought maybe it had some kind of filter.

Dr. Goody: It has non-black characteristics but that doesn't matter too much.

Howard: Now when we talk about the sensitivity of a bolometer this is sort of a havens' limit, it's the absolute thermo dynamic sensitivity which is - - -.

Goody: So you are not talking about what the BTL actually attains.

Wormser: And what is presently being attained in the _____ _____.

Goody: I still ask this question. If you cool all these detectors to the same temperature is your gold doped germanium still there? It should be.

Wormser: We have had no good practical results yet on cooled thermistors.

Dr. Goody: There are some possibilities, of course, of cooling these detectors when you are out in space if you can simply by means of a mirror system, immerse your detector that isn't looking at the spectrometer in the radiation field just picked out from space. You will get beautifully cold and this is a practical point which we might latch onto for efficiency purposes.

Fritz: I have raised this several times at meetings and since I know nothing about it, I always have to refer to the other people. I think Stroud, who isn't here right now, Stroud and Suomi both object to this as not being practical, I don't know why.

Goody: It may be optically difficult, I quite agree with that.

Wormser: Do you have a cabity klooking out into space.

Johnson: That effect of the satellite itself, isolating it thermally from the satellite body. Initially their feeling is that conduction within the satellite will be the problem.

Howard: There is a group at Hughes which have some kind of a metal foil bolometer that they are very excited about. I have seen plots of the maximum possible

efficiency of this and I think they have cooled it to liquid nitrogen. There is no spectacular gain between the ----.

Wormser: If M this thermistor will gain a factor of four with cooling with practical materials presently available, the impedence goes so high that we have no good impedence matching devices.

Goody: Well, I'm not saying it's not practical, but before one goes away from thermal detectors there is always a great advantage that once you've got no thermal detectors on the thing you can do any wave-length range. It is a nice feeling if you've got it set up this way.

Wormser: I wanted to comment on the practicability of multiple detectors. This is quite practical now. We have completed a 24-element unit for John Howard's group which is matched within 10±per cent. This is a thermistor unit. I think you could monitor your five wave lengths simultaneously, even some wave-lengths in between if you wanted to through the use of multiple detectors.

Howard: It eliminates a lot of m oving parts, too, and certainly decreases the time considerably.

Fritz: There is no question about one changing relative to the others, for instance, over some period of time?

Wormser: No.

Howard: These can each be perhaps a half a millimeter wide, but there has to be a little gap between them, but you can't bring them closer than a certain amount.

Wormser: The gap is of the order of a 100th of a millimeter.

Howard: We need a calibrating device inside the machine, anyway, because it's just a relative measurement.

Gates: The main thing is having the sort of focal plane that gives you the dispersion.

Howard: You could mount this on and stay on the focal plane/.

Elsasser: I~~d~~ want to ~~mm~~ make one more comment on this question of 6 mu versus 25 mu. If I remember right, unfortunately I don't have my data here, there is considerably more absorption at 6.3 mu than at 25 mu. After revising these I found that the 6.3 mu band is now higher and the rotational band is lower than we originally thought.

Fritz: You mean the absorption coefficients?

Elsasser: Yes, the absorption coefficients. I want to have to verify this.

If this is so, you might get another factor of 2 or 3.

Goody: To do Lewis' analysis you have to get on the wing of the band and that would happen to the long wave-length, wouldn't it?

Kaplan: Yes, the difference is this really, if you are at 25 mu and you are actually comparing absorption for the average intensity of something like this, you can't compare this with the central portion where the maximum portion of the 6 micron band, where there are a lot of lines. At 25 mu you have alot of gaps, and you can take advantage of the fact that you do have these clusterings of lines and look at the clusters. While if you are looking at the same sort of interval - I mean, look at a part of the 6 micron spectrum. The reason the absorption may go up relative to this, I feel, is probably because there are many more lines per wave number. Also, you may be able togo out further in the infra-red. Sooner or later you come to a place which is marked, opaque. At 35 microns you can't measure anything. It's more opaque than some of the clusters.

Fritz: So in other words, as I see it, you are raising another objection to using the 6 micron band. That is, even if the 6 micron band is as good or slightly better than the 25 micron, on theoretical ground you prefer the 25 micron band.

Kaplan: Calculations should be made for both bands. Both bands should be looked into from this point of view. Both from the theoretical and instrumental point of view. I mean I don't think a decision should be made today.

Fritz: Would you be prepared to make this calculation? Say if you had some help or something like that?

Kaplan: I think I can make up a set of calculations.

Fritz: So in other words, on theoretical grounds we still have to have some decision to make as to which is preferable. On instrumental grounds maybe - do you want to look into this question further as to relative merits - you are the one who has raised the question most - and it seems to me from what I've heard here that it probably could be done in either one if the theory permits you to do it in either one, although Richard has raised some question about the signal to noise ratio, which, as I get it, is still unresolved.

Goody: I feel one could go ahead with the program without the slightest difficulty here, one can build an instrument which is equally well for waves in both regions. One can build the thing with convertible detectors, making sure, however, that it does take a thermal detector, which we know will work, and has

facilities for these others and the thing can be tested as it's brought into the stage of putting it into the satellite. These don't seem to me to be major problems.

Fritz: This is a very happy state of affairs. In preparing this agenda, you notice, I had assumed that what the theory required would not be deliverable by the instrument people and therefore we would have to marry the two at the end saying that since we can't get what we want what can we do about using what we've got, but things have turned out much happier. Apparently we can deliver what is required, on an instrumental basis and we don't have to discuss the other one, except if we want to go into project TIROS, which I think is really not our purpose. It seems to me that it is possible that this is the direction in which to try. Now if it turns out that it becomes so difficult that we don't get it then we should at that time perhaps discuss what the interpretation would be and what we can come up with.

Gates: Of the five things you listed as conclusions from our first theoretical session, I think we will find out that there is an order of delivery which you could do. In other words, certan ones will be easiest to produce with the most confidence and certain ones less so, and so on down the line. I have the feeling that the cloud cover will be the one you can produce with the most confidence. There is that sort of thing that will come out of the experimental discussion.

Elsasser: There is also the logistics problem. Can you find somebody that will build this thing? With the stepup in NASA activities, this is q quite serious problem.

Fritz: I don't understand. Stepup in NASA activities? This is a NASA activity.

Elsasser: I know, but everybody will be wanting instruments. Can you find somebody who spent his time on this?

Fritz: Well, again I think this is in the order of the difficult problems which don't have to concern us here. I don't know the answer to this. I want to find out waht we need and what principal can be delivered. There are many other problems, as I mentioned. Communications will be very difficult if we get too many wave lengths and then data analysis

is going to be formidable. Let's fight that battle another time.

Kaplan: Could we get an opinion? My feeling is that there has to be a very good spectroscopist involved in this and I wonder if we could get the consensus of these experimental people, on this.

Fritz: Would anybody disagree with this? I don't know. I would think that more than one would be better. Do you want to ask somebody a question?

Kaplan: No, No.

Goody: Well this is a point you're going to have to watch. If you feed this kind of thing to a firm along the specifications that you draw up they could produce the thing but in the end it would have so many knobs and things on it that is produced without a feeling for what they were supposed to do that you could wait longer than for a voyage to the moon.

Fritz: Well I think you are right. I think somebody who knows what he is doing will have to have some hand in it. As things are now set up, I would think that there would be maybe more than one group actually working on the design or the specification that should finally be made out, maybe even with some pieces of hardware. Then when something, in somebodies'laboratory, became satisfactory, all this being directed by somebody, I don't know who. It may be that NASA, in its Vanguard Division or somebody else, would have a major hand or we might or somebody here might. It might be donesimultaneously by several groups. When finally something that competent people thought was adequate came about, then I should think you could draw up the specifications and follow through and see that they were made to specification. This is what you have in mind, I suppose?

Goody: Yes. I've just heard harrowing stories about specifications being passed out to big optical firms and what thereafter happens. It's a long, long, long wait until you get an instrument. This, one has got to watch, that one doesn't fall into this trap.

Fritz: I agree with you, and at the moment I don't know what the course of events will be to avoid it.

Goody: Well youv'e got to get a good spectroscopist into the thing.

Fritz: Is anybody going to suggest somebody? We'll have to try, anyway.

Shaw?: The problem is simplified somewhat, because you're going to build

a semi-rigid machine.

Fritz: Now in discussing these requirements, we mostly were talking mostly about 25 microns for a while, but is it exactly the same problem now to do it in the 9.6 micron band with one or two centimeter wave numbers resolution and then to go to the 15 micron band with something like five wave numbers resolution. Is the problem about the same in each one of these? Do we use more or less the same instruments, with the same detectors?

Howard: With those two we are certainly going to have to be restricted to thermal detectors. Only for water 6.3 microns was there a possibility of anything else.

Fritz: You were going to try to get some information resolving that versus 25.

Howard: For the 9 micron ozone or the 15 micron CO_2, since liquid helium carriers are just not practical, this excludes all of the photo-conductive cells.

Fritz: Yes, it excludes them, but also are we saying then that this other detector, the thermo-couple detector, is the one which will deliver the goods, so to speak, for these requirements in these wave-lengths. I kind of feel that it would be about the same instrument to do the job in anyone of these areas.

Kaplan: As a matter of fact, there is a big advantage of having the same optics for all of these things, since these are related and the data that you are getting are going to be cross-used in order to obtain other data. I would think from this point of view it would be better.

Howard: Something monitoring a window for example can give you a signal which can be used as a comparison to the 9.6 micron ozone or the 15 micron CO_2.

Greenfield:? The cam would turn the grating from one position to another. Three sweep positions and three set detectors.

Kaplan: Or else to have some sort of prism in between to give a computer some discression about that and from the same signal on the grating essentially that is displaced to reach the detectors.

Fritz: Well I really wasn't thinking of that, it could be done of course, but we may for other reasons want not to put everything on the same package and then you would still use the same instruments but slightly modified to measure the various ...

Goody: Well if you stick to a relatively narrow wave-length band of course, you have the possibility of using cutoff filters. But then if you go to more than an octave of course, you are going to complicate the instrument.

Fritz: Now we still have this question of the heat budget, unfortunately you were out. Dave Wark gave us a talk on this. While in principal if you make a measurement with a certain angular detector, a detector looking at a certain angle, you still dont

get the flux coming out of a high level surface. Let's say that's what you want. In principle you should have the whole spectrum and integrate wave length by wave length. But this may become unbearably complicated. But I have a feeling and Dave agrees, maybe he doesn't, but if you had a broad filter you could make a measurment at some angle and either empirically or theoretically, or probably on the basis of both, some empirical rules guided by theory, you would attempt to find the angular distribution of the radiation in this broad filter and compute the energy coming out of a horizontal surface on the basis of a measurement in one single direction. This direction may vary. If this is so, then it seems to me that we have to have now already detectors, the ones you showed.

(Some conversation missing here while changing tape.)

Elsasser: - - - - - doing something similar here (a) measuring the total radiation going out all over or in vertical from the earth and (b) taking say 2 stretches of the spectrum and comparing their relative intensities.

Fritz: When you say 2 stretches of the spectrum, what parts of the spectrum do you have in mind?

Elsasser: Oh, let's say at 6.3 mu and in the window at 10 mu. Just to measure these have a simple gadget so that we will be 100% sure.

Howard: That sort of experiment is already on the proposed satellite.

Fritz: Yes. On TIROS as I mentioned at the beginning, we have five sensors. One of them has something like one micron wide centered around the six micron region. There is one in the window from 8 to 12, rather broad, and there are these two other ones -- one going from 7 to 30 or something like that. There is a question which Wark has mentioned, however. This experiment measure the radiation coming from one angle.area. From one, for example, you measure it. lThe angle may vary from vertical down to the horizon. An if you want the flux coming out in the vertical through a square centimeter then you still have somethat of a problem in integrating, in finding theangle of dependence. We will have to do the best we can on that.

Wark: This is particularly important where you have clouds present, as you usually do over the ocean, and there is a much bigger variation with scattered, with clear skies or with an overcast. This is the normal condition you are going to encounter over the larger part of the earth.

Fritz: If we wanted to measure the energy coming from the whole earth, without regard to its latitudinal or longitudinal distribution, it would be possible to mount a horizontal surface, which is black, let's say, having some of these other characteristics, on this stabilized satellite that we will eventually have. But this gives you no spacial resolution because of the fact that you see such a large area at the same time. You can get some out of it, but if you see Northern Canada and Mexico at the same time, then it is difficult to separate any spacial resolution from it. If you integrated this thing over time as the satellite passes over the earth for a month or so, I think you would get a pretty good estimate of the energy leaving the earth as a whole. I think Suomi has proposed something like this. It seems to me that you probably would. I think that we would like to try to get this spacial resolution also.

Howard: Even this object you have which is spin stabilized and looking down at 45°, there are certainly times when it is looking more or less straight radially down. At that time the entire 5° you accepted is practically normal radiation from the earth. You are not worried about that kind of variation of angle. You are only worrying when you are grazing over towards.....

Dr. Fritz: Even when it is looking straight down, you are looking lets say at an area and you want to know all the energy coming up throught that area. It is essentially vertical.

Kaplan: You cannot measure the hemispheric radiation at the height the satellite is going. Because it won't see what you would essentially see at the top of the atmosphere. If you want to get an empirical relation from this you must do it by airplanes, by balloons or something like this. It can either be done this way or it can be calculated.

Fritz: I think we should do both. I think we should have the balloon measurements with sensors mounted at various different angles and

also calculated and see to what extent we get.

Goody: To some extent the matter is not purely theoretical. The question of the relation between the emission at this angle and this angle is dependent on the temperature structure which is simply not reproducable

Fritz: Temperature andalso composition.

Goody: Which you just don't know. I don't know what the answer to this is. I don't see how an empirical investigation can help you too much. A careful theoretical investigation would tell you how much the various factors affect you to see whether it is tolerable or something like that.

Wark: I think that in any empirical relation you have you would put in cloud information which would have to be gained separately from TV or

Goody: Unless it is a simple theoretically predictable then anything else is impossible because it will depend on the air donditions, you don't know them.

Fritz: I agree with you. I think that theoretical work is req uired. Personally I feel that I would like to check this simehow with an empirical one. I think that the important thing here is not that we are going to get the exact answer. We won't, no matter what you do. But to get some feel for the error that we would make. In other words, lets say in the first approximations we assume that the radiation of isotropic. How much of an error do we make with that?

Goody: If you can assume that it's fine, but...

Fritz: No, I mean supposing it turned out that the error if you assume that supposing that you confine your self to using only measurements at an angle of 35 to 55°. The radiation at that angle, maybe, gives you a pretty good estimate of the radiation from the hemisphere, if you assume that radiation coming from that angle is isotropic. I don't know, but maybe that turns out and if we made an error of 3 percent, we'll say, with that, maybe this is tolerable. Maybe 3 per cent isn't even tolerable. In that case we would probably be in rough shape. I think some error analysis have to be made.

Goody: I think this brings up again the difficulties one runs into with integrating devices. They are of course handy for some purposes, but for an understanding of the atmosphere one runs into some difficulties. This particular problem here could be easily tackled if one has a spectrum. Because one would use two different opacities on the spectrum to tell you how the thing would look at different air masses. One could infer one from the other with a little intelligence. This is the difficulty of using photo

measurements, you have just one measurement and/any absolutely adjustments has to be made theoretically. You've got no cross-checks inside the thing; whereas, the spectrum is absolutely stuffed full of cross-checks. If this is so than this is so and you can deduce a whole lot of conditions given it. I do feel that that this is where the effort should go, into getting this spectral information if possible.

Fritz: I agree with you to a certain extent. If we were going to get the heat budget of the atmosphere, for example, and do it by integrating over the spectrum over every 50 miles square area over the earth, this is a problem I said we should/not discuss, but I think it is obvious almost immediately that it would be a formidable computation problem and a formidable communications problem. I don't really want to go into this now. There does seem this problem which would be almost obvious in this case. Perhaps we should sample it in some places and do the whole spectrum and the other job at the same time, somehow, maybe from balloons.

Kaplan: Of course _____ is something that has to be done eventually anyway. We have to do it in detail over the whole earth. I mean when we make long range forecasts and things like that, this will have to be done. Essentially you calculate it.

Fritz: Oh, you calculate it, I see.

Goody: One thing on this total radiation you're talking about that just might be worth doing is not only to have a downward, but also a sideways pointing sensor. It would give you the other end of your non-isotropic curve. You would have two point on it instead of one point and you would guess the whole of the rest.

Fritz: You are thinking of a satellite which points continuously towards the center of the earth. I think in that case we would want the sideways looking ones at some angles anyway, because we want to get coverage more than just under the satellites as the satellite goes around the earth in a total orbit, then the areas in the equatorial regions it would be a very wide gap, on the order of 1000 miles or more between successive measurements in that region. So you would want to look out to the sides anyway to get some more information. Say the satellite is going that way you could put a fan this way and you could also put a fan the other way you

would get some idea empirically about the distribution around the vertical. As the satellite moves along, different sensors that come into view over the same area. So this would in any case want to be done. We don't have to stay until 5:00 o'clock. Is there anything else you want to discuss. Have I finished the agenda?

Gates: Maybe you know the astronomers are talking about a somewhat different type of measurement, but also related, of looking at the sun. The low sun grazing incidence across the earth's atmosphere with an infra-red satellite detector in water vapor, and CO_2 and ozone and so forth. I don't like this experiment very well because I think it is terribly difficult to know just what you are looking at. But they are talking very seriously about this. I think we just ought to be aware of this.

Goody: The're doing this for meteorological purposes, aren't they?

Greenfield? This is a wonderful experiment for example for scattering and this is one of the for example, you know Derminjian is very interested in this experiment.

Gates: This sounds like an experiment on transmission.

Greenfield ? I'm talking about the visible part. They weren't.

Gates: Oh, I see that, I was thinking of another one. You know Goldberg was talking about it.

Greenfield ? In what part of the spectrum was he?

Gates: Well, I think 9.6 ozone and .3 co_2 and 6.3 water to try to get the amounts of these constituents as distributed through the upper atmosphere by getting the path as it slices down through the atmosphere or up. You see this is the sun and the sun coming into ~~xxxx~~ juxta position. I don't know how sensible this would be or not, but I thought how nice it would be if there were a source at the earth's surface you see, which then you could get the total amount of something by transmission ~~xxxx~~ through. Well, there aren't any real nice sources, even the earth's surface itself is not a very good source since it varies a lot in temperature. But one possibility if you knew within a few degrees say, the earth's mean surface temperature of the ocean for Arctic Ocean middle latitudes and equatorial or something, whether or not you could use the radiant energy

of the surface of the ocean as transmitted through the 9.6 ozone band to get some sort of a broad monitoring of ozone. I just put it out as a thought, because I can see that it is important. Howard, what do you use as a light source.

Fritz: The ocean, the ocean at night say. Well what about that. I think in the tropics, I'm not familiar enough with the various ocean currents, but in the absence of currents and over rather broad areas I would think the ocean surface can be considered pretty much of a uniform source within a few degrees.

Howard: The absolute temperature, which is what matters, isn't going to vary much.

Fritz: Oh, that will vary very little.

Elsasser: Well, one has to be quite careful. Right on the surface of the ocean there is a temperature gradient or there can be. In the window around 10 mu if you look at the old Bethe-Dorsey for water there is more than _____ . In this region covered by the bands, which is not surprising, this is for liquid water. It is not surprising, there is just a little more than an _____, than in the region of the window. So if you don't watch out ....

Fritz: How deep does it go?

Gates: You see to a millimeter or a few millimeters don't you. Maybe at most.

Elsasser: No, not even that. But there might be a very steep temperature gradient in the very surface layers of the ocean.

Fritz: Yes, but not to orders of a millimeter or a centimeter.

Elsasser: Well, we don't know that.

Fritz: We know something. There have been measurements made by Woods Hole, for example, in the Atlantic, where they had an instrument sensitive at 10 microns and they had rowboats with thermocouples mounted along the bow and they claimed that they could have reproduced those temperatures with the thermocouples to something like 100th of a degree and they say that they followed very faithfully with this, flying close to the ocean. When they got high up they had trouble because even in that region they had trouble with emission in the water vapor. But flying close to the ocean. When they got high up they had trouble because even in that region they had trouble with emission in the water vapor. But

flying close to the ocean they were very successful in measuring temperatures the orders of 10ths of a degree.

Elsasser: They duplicated the radiation temperatures with the .....

Fritz: They found what they call hot spots. Namely temperatures that differed from other areas by a tenth of a degree they called hot spots.

Gates: They required huge optics, I think.

Fritz: I saw the instrument, but I don't know how much of it was optics and how much was something else. But you are interested not in tenths of a degree. You are interested in several degrees.

Gates: No, that's right and I was thinking in terms of a polar orbit and the fact that over ocean areas you would be sweeping from polar regions right down to equatorial regions south and all the time remaining over water.

Fritz: Well supposing you had this temperature where you like it, can you still do anything with it? Supposing you had this uniform temperature surface and you wanted to measure ozone?

Goody: If you took a point on the edge of the ozone band you would have something close to a linear low and you know the/approximate mean emission temperature of the earth which/on the whole probably one does know. Then you have a direct law between the total amounts.

Fritz: In other words you would still need some idea of thetemperature of the ozone layer.

Goody: You always have to know something about it.

Fritz: You would have to ask the question how much of an error do you make if you don't know the ozone temperature by 10 degrees, say, if it turns out that it is not very important within a 10 degree interval then you might get away with it. But if 10 degrees was important there might be trouble, not knowing the temperature.

Gates: Well it's simply a first crude approximation to the problem but the important question is, is it worth something?

Fritz: I would say that if you got the total amount of ozone by that method or any other method over a substantial portion of the world, it would be very nice, very important to have. If this method provides that over the ocean areas, which cover enormous regions of the earth, I would think that it would be very important to go ahead and try something like this.

Howard: (?) But the measurement that you would get would provide information regarding not only the aount of ozone but the pressure distribution of ozone.
Goody: In the linear law region you wouldn't be as pressure effective, that is the point.
Howard: Up near the edge I see.
Fritz: The idea of measuring pressures in the atmosphere we don't know how to do this, but personally I feel that we should tell everybody that the pressure is very important to see that we could come up with an idea of doing this. I just pricked up my ears when you said something about that.
Howard: No, I am just afraid that would be acomplicating factor.
Goody: Since we've run out of the subject of the infra-red could we ask by chance if you would tell us or discuss what your plans are in the ultra-violet visible and other parts of the spectrum.
Fritz: Well we don't really have any firm plans. In the solar radiation we do have in the TIROS package this raw filter which is going to measure the reflected solar energy looking at certain angles. Other than that I don't know of any specific plan to do anything. Does anybody know any? We've heard about things. It seems we had a proposal about measuring the scattered radiation in the ultra-violet, and I think.....
Goody: This should be coordinated. You see it's very difficult to discuss this program entirely by itself and in Dave's suggestion of getting total ozone here, cain't be considered outside the context of what other methods are now available.
Fritz: I see this meeting as one meeting to get together with people whom I consider to be leading in this while field and then perhaps now we should think about what was said and attach each one of these elements separately. In other words, we should not have perhaps a meeting which is going to try to solve the while thing but now, say talk about ozone and not limit ourselves to infra-red but just say how would we measure ozone. I think if we had gone into all the methods today we wouldnt have been able to cover each one of theelements. I agree with you that we should think seriously about other methods of measuring these elements and consider them one at a time.

Goody: It would be well worthwhile seeing ~~an~~/integrated sum of the _____ using more _____ of the spectrum and how they can re-enforce each other before one goes blundering into with a lot of money and effort.

Gates: What could you get with the sun and the _____ of the ocean, having the sun down and back through the ozone.

Kaplan: It doesn't have to be just the ocean, it could be off of anything. During the daytime the best way to measure the ozone would be in the ultra-violet.

Goody: With a little spectrometer?

Fritz: I didn't get the complete remarks.

Goody: Well, when the sun is shining you simply make a little light in a light prism spectrometer a little faster. You just pick up the earth's surface. It doesn't matter what there is as long as it doesn't have a nasty absorption band as many materials have.

Fritz: You are talking now about the ultra-violets and you say photograph the earth's surface.

Howard: Just get a spectrum and absorption spectrum. Ultra-violet by ozone using reflection from any portion of the earth's surface.

Goody: It doesn't matter what it is.

Fritz: Would you go far enough into the spectrum so you would see the earth's surface?

Goody: You picked the point, you photograph the range just where you go from transparency to opasity using standard techniques.

Fritz: From 25,00 down to 3,300 or something like that.

Goody: Not even that. You don't care what the source is just as long as it doesn't have any nasty absorption band.

Fritz: Dobson doesn't go to 2,500 he looks at the sun which means you have to go from 3,000 up to 3,300.

Kaplan: You have to get a reflection, if you can't see the earth's surface you can't get a reflection.

Howard: I think the only other experiment that was on the original ~~eeva~~ APOLLO thing which is now the one you are doing is the solar X-ray and/or Lyman Alpha sensor,

Fritz: Looking at the sun?

Howard: That was the only thing that was not in infra-red experiment proposed in the original.

Fritz: But that's no longer on the package.

Howard: Well, that's the only one that has been proposed to be put on there. Friedmann/ is checking the NRL Lyman Alpha solar X-ray package.

Fritz: I don't understand. You mean that was the one proposed in that region of the spectrum. Look at the sun in that region of the spectrum.

Howard: That's right.

Goody: That's the only non-infra-red.

Fritz: No, that's not true, because we have two on the package now which are not infra-red. We have .5 to .8 which is not infra-red and we have one from .2 to 6 which mainly is not infra-red.

Howard: The TV scan. That was separate. You have one now which goes from .2 to 6 which is reflected solar radiation which will cover the infra-red also. But it covers the infra-red and the visible.

Howard: The lead sulphite and the TV, but I mean apart from that ----

Fritz: I'm not talking about lead sulphite. I'm talking about .2 up to 6. That's not lead sulphite. That's a thermistor element.

Howard: I see.

Fritz: I mean its true, but the packages have been called infra-red radiation but I've insisted all along that that is a mis-nomer because it includes visible and ultra-violet. On this ultra-violet reflective radiation - do you have to see the earth or does the experiment work with clouds? Or even if you don't see clouds, suppose you go from .25 microns up to 2800 angstrom units.

Goody: Well, about 3000 angstrom actually.

Fritz: Well with 3000 you would get down to the earth's surface. Is this desirable or not?

Gates: It doesn't matter what you look at, because all the light is coming from below the ozone and you compare a window with an absorption. You don't care at all what your course is.

Greenfield:? Don't you get into some problems of scattering at this point? Are you sure when you are looking down as to where your radiation is coming from. You're starting to get buildup in your beams from first second and third tertiary scattering.

Howard: Shorter wave-lengths are lost as the light goes down through the first time in any case, so they won't return, not from the reflected surface.

Goody: There is an indeterminancy when you are looking at a scattering surface

and it is a transfer problem but it _____ normal clouds and then most of the atmosphere is down below here. Dust and all, now that doesn't have much to do with the ozone up here and the sun coming down and then coming up here at various points in here. Now then it is quite clear that what has happened is all of this region above about 10 kilometers, the radiation has gone through it twice. There is nothing more complicated than that. There is no scattering up here.

Shaw: The CO_2 content _____ _____ _____ atmosphere _____ _____ _____ _____ depending on the opacity of the band they get different amounts of CO_2 in the atmosphere. In other words ifyou had a weak band where you see through the entire atmosphere you see a lot more CO_2 than if you look in a very strong band in the infra-red where you get _____ _____ _____ scattered.

Goody: Well, there is no question here you see. You pick a point in the solar spectrum in this region, plotting it as an absorption spectrum against transmission It goes like this. Where that's 3,000 angstroms. It doesn't particularly matter. Your spectrum may be like that or it may be like that. You pick a point where 50% absorption occurs. You never look at an opaque region.

Shaw: You have to be careful now. 50% absorption I think. After all, as you say you have/sloping

Goody: This problem with the exception of a slight indeterminancy as to whether some of the radiation might not be coming from the upper end proportion or not in proportion.

Greenfield:? You are still faced with the problem that you do not really know what your path length at various parts of the spectrum you are looking at. In other words you are not going to get a constant path but it's going to be ---

Howard: It can be partially calibrated by examining the earth light on the moon.

Greenfield: ? Because you are getting scattering from all parts of the tropopause troposphere.

Goody: Because 10% is scattered in the troposphere and say 40% from the ground and 50% is absorbed. Then 10% of the ozone is in the troposphere. So you know as regard 90% of the ozone, you know the path exactly. As regards 10% of it you are uncertain to about 10% of what's involved. About 1% uncertainty.

Fritz: Could you write down what you would consider to be the wave length region that you would be looking at.

Goody: 3,000, 3100.

Fritz: I see, you would not look at 2500.

Howard: It wouldn't come through.

Fritz: No, but some of it would be reflected, for example ..

Gates: You don't have clouds. Then you would have trouble.

Goody: Then you would have this objection that the point you are looking at would vary with the opacity. You deliberately pick a point. You know that is the earth's surface there, because the thing flattens out there. Because you know that this wave length will go through to the surface and you are essentially comparing that with that.

Howard: Wouldn't you get something of this sort if you examined the earth's light on the moon?? That is an even longer path isn't it.

Goody: Yes, you can get ozone from earth light on the moon.

Howard: And that's virtually the same problem. That light which has come through to the reflecting surface through the atmosphere to the moon and then back through the atmosphere to the observer.

Kaplan: In this case it doesn't even have to come back through the atmosphere. Greenfield? Are there any problems as far as reflectivity versus wave length of the earth's surface in this region.

Goody: Well you would have to be most Machiavellian to _____ _____ _____ this really nasty change between 2 nine and 3 one hundred. If you chose some really nasty bumps it would upset and you would get an almost _____ _____ _____ _____ _____ and this would give a non-sensical answer. I don't know what ---

Fritz: I should think over the ocean you might be pretty safe, but over land where there is vegetation maybe you'll be in trouble. But if we can solve problems over the ocean we have an awful lot of information right there. Adel? All I'm pointing out is that this is a region that needs a little bit of exploring before we seize on it as a ----

Goody: I think it would be a good idea to look at some standard surfaces and see.

Elsasser: I would have one concern about this particular method in the ultra-violet, namely that you don't know whether the solar intensity is constant or if it varies.

Adel? You mean over time?

Elsasser: Over time.

Fritz: Well we have made measurements with Dobson instruments measuring from 3100 to 3300 or there abouts. As far as anyone can tell that is

not very important when you compare the two wave lengths.

Goody: Apart from the scattering problem, the whole problem is just the same as the Dobson one and such objections have been tested in the Dobson technique .

King: It seems to me that we are running the danger of galloping away and loosing sight of one very important fact. First off, if you think of a 300 mile orbit, that's pretty far out, all that you can get will essentially be a global picture. The fine structure of the atmosphere, which has been discussed here, in on the order of 10 miles out. It seems to me that the most you can expect from a 300 mile object is some simple monitoring device of the nature of looking down at the earth. Looking straight down, nothing complicated. It seems to me that all your grating or prism spectrometers, I'm not sure that it will work in the first place and I'm not sure that they will give any added information, because it will scan such a big area. It seems to me that the problems of the earth's atmospheric circulation and so forth, heat budget and so forth, would better be solved by means of balloons or sounding rockets because, after all, 300 miles you are going 30 times on the other side of the atmosphere. You get something that is certainly more accessible, in some ways, from the surface of the earth.

Fritz: May I ask how you measure this? Say over the whole South Pacific in the southern hemisphere?

King: Measure what?

Fritz: Temperature, ozone or water vapor, or the South Atlantic or even the tropical regions.

King: I don'tknow, sounding rocket.

Fritz: Where would you mount them, from ships? That is the advantage of the satellite, that it can give you world wide measurement whereas the others cannot.

King: Theoretically, it can do anything, but when you see the best data that 50 or 100 years of experimental finesse has produced on the earth under ideal conditions, stable platforms and all that, and you see how poor it is in giving us any information as to, for instance, this subject wer are talking about.

Fritz: Which data would you mean?

King: For instance, darkening, or trying to obtain some feeling for temperature by looking at different regions where your spectrum has 10 different opacities.

Fritz: We know something about the ozone distribution over the world. We have done it by looking at the sun from fixed points of the earth. This has been done. It think we do know something about it.

Howard: What Jean is sort of saying is that it is a pity that the big money that is available for these satellites couldn't be partly diverted into concurrent studies from balloon platforms and other things which would check out some of these ...

Fritz: If this is what he means, I thoroughly agree. I think we should check out everything from these. But if you mean that we are going to get world widge coverage everyday with balloons or rockets or anything like that I don't quite understand how you would do this.

King: No, I just wonder if it can be done or if the information you would get would be worth while.

Gates: I think you have no choice. If you do these things you get certain approximations of the data you want, you get experience, you go ahead and refine it and ten years from now we'll have some pretty exciting results.

Howard: I think one has to be philosophic, that some of these satellites are going to be put up anyway. We might as well put something on them that will bring back useful data.

Fritz: I would like to be more positive. We spent all day today trying to see what we need to measure certain quantities. I assumed from the beginning that meteorologists would like to have worldwide measurements of water vapor and they would like to have worldwide measurements of ozone. If I was wrong about that then of course we are wasting our time.

King: No, certainly I agree. But keep it simple. For instance, it is worldwide. Your field of view is going to be very broad, you can't get much fine structure resolution from 300 miles out.

Fritz: You can get anything you want really, but what do you want? Now I was saying, myself, or some problem on area of, say, 50 mile diameter was something that we might strike for. I should think that we might occasionally want much finer resolution than that over some areas. If you get much finer resolution that that over the whole world you will be swamped by the data. Even at 50 miles you are going to be swamped. You are going to have to make selections. I am not going to sit here and decide that 50 miles is the thing you want but, it's a practical type of thing that we ought, at least to stop adn think about. You may want to make it much larger or you may want to make

selections. But if we go on the premise that we do want this kind of information, and I personnaly feel that we do, then we spent all day today saying how would we do it. The consensus of opinion that I had is that there is enough optimism about it. The theory suggests that one might get these results, to get the world-wide distribution of these quantities, with maybe even some vertical resolution, and this is what I gathered from the meeting, that theoreticians say that this can be done, and the instrumental people say they can give us the instruments to do it, Now why don't we go ahead.

Elsasser: Personally, I should make one suggestion, that you go up to the blackboard and make a list of the things, the meteorological things, that w have no decided we can get. 1,2, 3 and so forth.

Fritz: Well I think I can recite them very quickly. There was temperature, Kaplan mentioned CO_2. Now there may be some difficulty, but at least he showed us the first approximation of what you would do. There was temperature.

Elsasser: What temperature? Effective radiative temperature.

Fritz: No, this was the temperature in 10 different layers of the atmosphere, the mean temperature in 100 milibar layers.

Howard: Now these would be 10 layers though where there is a lapse rate. You are not going to do the stratosphere and into - are you in this kind of pattern or are you down here where it's still - - - -

Kaplan: What I'm thinking of is having 10 layers, including the stratosphere, every 100 milibars. You have to make some assumption as to what the disttibution is in this layer. The simplest thing is a constant lapse rate.

King: This is impossible. I defy even to construct synthetically any kind of model and then just from straight emissive scan to get out the information.

Kaplan: That's not what I'm going to try.

Goody: What information? You mean the temperature or the ---

Kaplan: The temperature.

Goody: We have set up models on digital computers with the others and worked them backwards. You can get out the - - - -

Howard: The feeling was that CO_2 could be used to measure this because at least we could barely assume that it is fairly uniformly distributed.

Fritz: We assumed that.

Howard: That's sort of implicit to the whole calculation.

Kaplan: Here is an experiment for example. I calculated the outgoing radiation

for two models, different from one another, a bit different from one another, then I solved my ten simultaneous equations with one model in terms of the departures in the other model, the coefficient in the departures of temperature, and see how closely it represents the other model. This can actually be set up. You can actually try this out with different calculations. You may be right that it is impossible. My feeling is that it is possible.

Fritz: To go ahead. This is what he said and maybe we can't. Are you writing down these things? Are you going to write down the meteorological elements or what.

Gates: I'm going to write down the five things you mentioned two hours ago and the additional ones you've mentioned since.

Howard: Wouldn't it be a good idea to take out insurance in addition to sending up the spectrometers and send up some filter devices. You would be reasonably certain them of having something that would work.

Wormser: It works all the time with spectrometers.

Howard: We have taken out that insurance. We are not even considering the ones that have already been settled on for your TIROS experiment. These are the crude experiments that will be coming out in the meantime. Supposedly we are only considering more advanced measurements.

Fritz: Yes, I think that the TIROS package was not designed to measure these specific meteorological elements, except for the heat budget, clouds, or something like that, but if you want to measure these elements, the feeling I have is that you cannot do it with filters. ~~With what would you measure ozone in the TIROS package.~~

Howard: The TIROS budget was intended to aim at the amount of ozone.

Fritz: Ozone. No not ozone. In TIROS?

Howard: In was in the earlier version.

Fritz: With what would you measure ozone in the TIROS package?

Howard: There was to be one sensor looking at 9.6 microns and one looking around 11 microns.

Fritz: There isn't now. There is one from 8 to 11.

Howard: I see.

Fritz: Well you know the elements on the TIROS package. You were on the ARPA committee.

Howard: Yes, well they seem to have changed a bit since then.

Gates: I think these are the main classifications as you reviewed them down to

this point and then we will have additions.

Fritz; I would like to put in parentheses after these things, what are the meteorological elements which you are going to measure. For instance for IR 25 microns with, what, water vapor. That was the purpose of looking at it.

Goody: Now wait a minute. Let's get a little more _____ about this. Just H_2O. We'll measure H_2O as a function of temperature, not as a function of altitude.

Howard: Now wait a minute, is this the temperature of the water in the stratosphere?

Goody: The best you can get out of the inversion is the density of water vapor as a function of the temperature in which that water vapor lies.

Howard: But now Lewis' scheme was for the distribution of water vapor in the stratosphere.

Fritz: There were two things.

Goody: This is quite a little problem. This gives you O_3 as a function of temperature, and this gives you CO_2. Now you've got to have something to sort that lot out. And there are a number of things. You can assume you know the CO_2 and you can work back to this or you can measure the ozone Umkehr as was suggested. The thing's got to be planned out, it doesn't just automatically settle the properties of the atmosphere, one has got to incorporate other measurements to make this scheme complete. So this ought to be coupled either with the assumption of CO_2 or else Umkehr.

Kaplan: As a matter of fact you have to have both, because you do get emission from below the ozone. So you have to know what the temperature distribution is and what the temperature of the ground is.

Goody: In otherwords, these temperatures which are important in these don't overlap at all. So we need both the Umkehr and the assumption of CO_2, in order to get that as a consistent scheme, and then one can work it out. It is quite a complex set up of experiments.

Greenfield:(?) We might be able to replace the Umkehr by the ultra-violet measurement. Goody: No that won't give you the distribution. Greenfield: (?) Oh, you need the distribution first, vertical distribution. You need the density as a function of temperature. That seems to me to be the critical limit and it ought to be part of this list.

Fritz: It seems to me that you called it Umkehr but Singer didn't. It seems to me that Singer in his article was trying to get the vertical distribution

of ozone by looking at many wave lengths including 2500 angstrom units. Now whether this has any theoreticaly pitfalls, I don't know.

Kaplan: I don't know how carefully Singer thought this out, but Sekera believes that if the Umkehr is treated elegantly that it can be done.

Goody: It certainly doesn't have much elegance if it requires too much instrumental accuracy, but it does seem to me that if Sekera has ideas on this, we ought to ask him what they are because it seems to me that it is central the part of this interpretative scheme.

Kaplan: Now as far as water is concerned the measurement of temperature in Co_2 is essential for all of these things. If anything is going to be done first, the single package, probably this would be the best one.

Fritz: Which one?

Kaplan: The CO_2 temperatute. I mean, getting tempeature in the CO_2 15 micron band.

Fritz: That's number one you feel?

Kaplan: This has to be number one if this scheme is going to be done in any way close to what I have suggested.

Fritz: You need the temperatures in all the others. But you already mentioned to me in private that you would have to make a water vapor correction for instance for the CO_2 measurements. You would have to have a water vapor correction.

Kaplan: In most cases it would be very small.

Fritz: You wouldn't have to have a water vapor measurement to make that correction.

Kaplan: It should be done sooner or later. It would not have to be done in all cases, it would have to be done under conditions _____ _____ _____ and in any case you can get a pretty good approximation. You would just increase your accuracy.

Barnes: (?) Can we check one more thing on that and that is these resolutions that wehave been talking about at 25. What did you conclude? Is that where you needed one to two wave numbers.

Fritz: There were two separate parts of it. One was the stratosphere where you needed one to two wave numbers, and in the troposphere you needed 25. There are two separate parts. Stratosphere and troposphere. Yes.

Kaplan:: And the two as a matter of fact depends upon being able to really set your ___ it requires a critical adjustment of your cell relative to - - - .

Greenfield (?): When you say two, what you are saying I believe is, when

you say two you really mean two. You don't mean to spill over.

Kaplan: No. You have to have a wave adjustment. You have to know exactly what wave length you are on. You can do this really by looking at the lines. You can have an automatic adjustment on it.

Gates: Sig has reviewed those resolutions earlier. That's why I didn't put them on the list.

Fritz: It's all alright. I think this is very good. Dr. Elsasser's desires are useful, I didn't realize how useful it would be.

Wormser: May I ask an experimental question? What general order of magnitude of weight and size is permissable in this project?

Fritz: We started out the meeting by saying that we could think in terms of 500 pounds at 500 miles, but Dr. Tepper mentioned that that is even not any limitation. You can go to twice that, say.

Gates: What did we call the earth's _____?

King: It's an angle scan, rather than a wave length scan.

Gates: All three angle scans.

King: No, its just for their zenith angles.

Gates: This is for ozone.

Goody: It's the same though in principal as one through three except that you are using angles in that way.

Gates: Now the thing that I was going to do, Sig, and the reason I really got up here was to discuss the ease and confidence of approach on each of these. Which ones are the easiest and which are the most difficult from the experimental standpoint, and I rather gather that this is what Walter has had in mind.

Fritz: Dr. Elsasser you asked a question for this. What else would you like to have up there? Is there anything in number 4 through 8 that we have to put down.

Elsasser: I merely think that leaving out 7 and 8 would _____ _____ _____ _____ _____.

Start out with 4 and 5, 3 _____.

Gates: Well put 1, 2, 3 and 4 over here in the order which we can do easiest. and first. Is that what you had in mind?

Elsasser:

Elsasser: Easiest is most important first also. I think 3, 4 and 5 are the most important.

Goody: Before we become immersed in this thing may I ask a question. This has now crept on me. I knew it was going to but you just can't do it otherwise. You've got U V here, and Umkehr, I don't really think you can look at this whole thing unless you look at the radiation field as a whole and I'm not sure that start putting down the list we shouldn't at least try to put down the other things which fit in naturally with these. To begin with there is the solar radiation of course. The absolute intensity in the Ultra-violet is the key thing. It must be, otherwise its the ___ ___ ___ ___. Also the solar constant.

Fritz: Does the heat budget include the things as a general category or not? Do you want it broken down finer? I have no objections.

Goody: Well, do you want us to stick to information in this half of the region of the spectrum or do you want to _ - - -

Fritz: We could talk about anything we want now because I feel that the basic purpose of the meeting has been accomplished as far as I'm concerned. My original feeling was that if we got into things other than the infra red we would not get very far. Maybe I was wrong about that. Now that we have finished what we really set out to do, I think the floor is un (_____).

Gates: Well I don't know but I would prefer to round these up. This is being done anyway. This is something that already - - - -.

Fritz: If you want you can put down on the side, if you like, the wave lengths that are now going to be on the TIROS package which, I mentioned, would be launched later this year. There is one that goes from .26 to 6 microns which I call solar. There is another one from 7.2 to 35 which I call terrestrial. There is another one from 8.5 to 11.5 which is the window, .56 to.75 which may be cloud cover for want of a better name and then one from approximately 5.8 to 6.8 which was the water vapor. It is in the water vapor band anyway. The instruments are now being produced to do this.

Elsasser: Do youg get those for the vertical beam?

Fritz: No, this is a spinning satellite and we will have to take what we get. It will be looking at all different angles and presumably we will know the angle at every instant of time.

Elsasser: Then you need a 704 for several years to - - - .

Fritz: The program now calls for the tape of the original data to be

processed in a certain way, the orbital elements to be produced by the Vanguard, those two tapes will be fed into the Weather Bureau and at the cost of about 4 hours a day these two tapes will be married so that all the information will be on one 704 tape.

Goody: Then you buy a city block and start storing it.

Fritz: No it will tape 10 tapes. It will run 90 days so you will have 90 tapes.

Question: What will you do with the tapes then.

Fritz: This becomes a meteorological problem. I think, myself, what we will do at the beginning will be to take the measurement at a particular angle and, after some study of the kind we mentioned, try to go from that to the flux in all direction by some suitalbe trigometric transformation based on whatever we can get. This will give us the distribution of the heating over the whole earth, at least over that protion of the earth which the satellite saw and during the time of the day that it saw it, and then we can from meteorological studies. I would think that one of the first things that we would try to do would be to take that average property over some days and look at the general circulation in relation to it.

Greenfield(?): You will also include in your proper registration ___ ____ proper _____ factor.

Fritz: We will know exactly where everything is located at all times, geographically. Time is the element they measure , the total time.

Johnson: On the radiation tape there is a ___ ___ ___.

Gates: Let's look for the relative importance.of cloud cover. I'm going to stick my neck out and ----

Fritz: Importance is a very difficult word to use. I don't know, it depends on the purpose and so on.

Kaplan: No wait a minute. Cloud cover, does that --- and temperature and height.

Elsasser: Cloud amount and height?

Gates: No I mean cloud cover and cloud temperature.

Kaplan: You see you can measure temperature and heights ----

Fritz: I think we are getting into a pitfall which this meeting was designed to overcome to a certain extent. There have been certain meetings which people who are meteorologists were present who made decisions in areas which I felt they didn'

I felt they didn't have any competence. And I think we are now getting to an area where this group has no competence. Namely the decision of what is most important. You see the competence of this group is in spectra not in importance of meteorological elements. So you can put it down but I don't think it means anything.

Gates: Well we can suggest that you might like to have our advice on this sometime.

Fritz: I would ask a different group. If you are going to ask what the important is I would ask a completely different group from the people who are in this room.

Howard: Since we would like to put down the relative ease ----

Fritz: Ease. That's another thing. Importance is something else. You can put it down. I just want to bring this point out, that it has no weight.

Goody: Well you can't put the importance up there.

Fritz: Well, I myself am also not completely competent. I would have some ideas on it but other people like Byers, Houghton and Haurwitz and other people like that might have completely different ideas.

Goody: Let me ask this question. These can be marked in relative importance. Used in the problem to solve the question to solve the general problem of the atmospheric radiation field.

Fritz: If you set an objective you might, but if somebody is interested in ozone, he might feel that ozone is number one.

Kaplan: I agree that sooner or later we are going to get into an argument.

Fritz: I have absolutely no objection to writing down things.

Gates: I myself would keep away from importance all the way through this, but I think that we could write three categories or something.

Goody: Why don't you limit it to ease then.

Gates: Lets talk about ease then.

Fritz: Ease.

Gates: Cloud cover is easy.

Goody: Cloud cover is much more _____. You can't (six words missng)

Gates: Well let me ask this. Which is the most easy?

Goody: You mean of those you have there?

Howard: Number 7.

Gates: O.K. we'll put it number one.

etc.

STANDARD FORM NO. 64

Office Memorandum • UNITED STATES GOVERNMENT

JDW 6/7

R-3.2

TO : Mr. John Webber, Head of Library

DATE: June 6, 1960

FROM : Meteorological Satellite Section

SUBJECT: TRANSCRIPT OF THE MAY 1, 1959 CONFERENCE ON INFRARED MEASUREMENTS FROM SATELLITES

Enclosed is a verbatim transcript of the proceedings of the Conference on Infrared Measurements from Satellites. This meeting was held on May 1, 1959, under the auspices of the Weather Bureau, and brought together a group of top theoretical and instrumental experts in the field of infrared.

I am citing this meeting as a reference in an article and I believe that there should therefore be a copy available in the library. It is submitted to you with the recommendation that it be added to the Weather Bureau library catalogue.

D. Q. Wark

D. Q. Wark

Enclosure

LIBRARY
JUN 23 1960
WEATHER BUREAU

100 892