Satellite Data
In Meteorological Research

H. M. E. VAN DE BOOGAARD, Editor

December 1966
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Proceedings of a Workshop held in Boulder, Colorado,
25 to 31 August 1965

NATIONAL CENTER FOR ATMOSPHERIC RESEARCH
Boulder, Colorado
CONFERENCE SPONSORS:

National Center for Atmospheric Research (NCAR)
Environmental Science Services Administration (ESSA)
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FOREWORD

Early in 1965 the National Center for Atmospheric Research (NCAR), together with the Environmental Science Services Administration (ESSA) and the National Aeronautics and Space Administration (NASA), decided to arrange a Satellite Workshop to be held in Boulder, Colorado. This meeting took place during the period from 25 to 31 August 1965. The members of the organizing committee were as follows:

S. Fritz ESSA
T. Fujita University of Chicago
D. S. Johnson ESSA
W. W. Kellogg NCAR (Co-chairman)
W. Nordberg NASA
C. A. Palmer NCAR
V. E. Suomi University of Wisconsin/ESSA
M. Tepper NASA
H. M. E. van de Boogaard NCAR (Co-chairman)

The purpose of the meeting was to acquaint members of the university community with the latest techniques for using satellite data and to stimulate and co-ordinate efforts among scientists to develop a national meteorological satellite program. A total of nine sessions were arranged, covering various aspects of satellite meteorology. Each session began with a presentation by an expert on the subject under discussion. Leading discussants, generally from the university community, were then invited to present their views on the research aspects, and a general discussion from the floor completed the session.

In the following report an attempt has been made to publish the presentations of the main speakers and discussants. Virtually all the material was recovered from tape transcripts and most of the authors were kind enough to provide summaries of their presentations. However, in a few instances where the speakers were not available, the material has been taken in its entirety from the transcript. The need to limit the size of the report made it impossible to publish all the floor discussions.

Henry M. E. van de Boogaard
Co-chairman
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SESSION I

INTRODUCTION
WELCOME

Walter Orr Roberts
National Center for Atmospheric Research

I am delighted to see the faces of so many old friends here and
it is a marvelous pleasure to be able to welcome you all to NCAR and
to this interesting workshop. I think that it is a sign of very good
judgment by our co-sponsors, the Weather Bureau and NASA, to call a
meeting of this character at this time, and we at NCAR are overjoyed
by the opportunity to be your hosts here in Boulder. It seems to me
an extremely exciting opportunity to explore some of the possibilities
of the effective use of the new satellite technology.

It appears to me that in all of the atmospheric sciences we are
faced with the challenge to use the new tools that have become availa-
ble with the fantastic rate of technological progress, not only in the
satellite area, which of course is spectacular, but also in the use of
new kinds of devices such as lasers and radar. I think that in the
whole area of satellite meteorology, moreover, a few groups have al-
ready magnificently demonstrated some of the potentialities of these
new techniques.

At this stage, perhaps, many of the things we read and see
appear to be potentialities rather than actualities. It needs many
more of us in the atmospheric science communities and the universities
and other institutions to examine, debate and explore critically the
potentialities and the shortcomings of this new technology.

As I see it, the future is going to have satellite meteorology
techniques as a central tool and we must know how to use these most
Session I

efficiently. Their use must not be explored solely for atmospheric physics but also for the many areas of the other geophysical sciences.

I congratulate you on gathering together such a fine group for the coming session and I wish you every success possible.
The real title of this should be "Satellite Meteorology Beckons the Academic Community," or "Meteorologist, Where Are You?" Frankly, it has been something of a surprise to the true believer in meteorological satellites to see that, while a few scientists have been very busy mining the wealth that satellites have been producing, by and large, the meteorological community has not taken advantage of this major new tool. This apparent apathy includes both the research meteorologists and the operational forecasters. I must hasten to emphasize that brilliant use has been made of this data by a few and most of the people in this group are in this room today, with a few important exceptions. You can see for yourself that it is not a very large group.

Now, without casting any blame or any accolades for the past use of the output of meteorological satellites, it seems to me that it would be useful to ask a couple of questions and to provide half answers to these questions. The first question is: "What have been the problems in the past that have hindered the use of the meteorological satellite data that we have already generated?" And the second question: "What does the future hold for meteorological satellites?" I will discuss these questions, but I think the speakers that will follow in the next few days are going to cast much more light than I will in my brief talk here.

Sometimes it is useful, in order to understand the present situation,
to go back and have a look at the past and recall how meteorological satellites came to where they are now. It is such a new field that even someone like myself can remember the beginning. For instance, when I joined the RAND Corporation in 1947 there was a project (then classified) to look at the feasibility of an earth-orbiting satellite vehicle, and it seemed to me as a meteorologist that it was rather obvious that if you could look at the earth from above you'd get a unique set of data from a unique vantage point. I think this feeling was shared by many of the meteorologists with whom I talked in those days, particularly J. Bjerknes, who was a real enthusiast of this idea, Tor Bergeron, who is known for his use of cloud observations, and Clarence Palmer, to mention a few. There were others, of course, who shared the same feeling.

My colleague, Stanley Greenfield, who joined RAND in 1950, and I went to work and wrote a report on the subject on how one could use satellites for meteorology.

It should be mentioned that this report contained a chapter by J. Bjerknes himself, who worked with us on this project, in which he used some of the early V-2 photographic observations recovered at White Sands in 1948. With these photographs he did a magnificent synoptic analysis, using at first just the cloud data to see what he could see, and then going back and redoing the synoptic analysis in the light of all the data available. This probably was the first time a serious attempt was ever made to analyze the atmosphere from above, and it did show what a valuable adjunct the cloud pictures could be.

This report was classified at the time, and attracted very little attention. This lack of reaction was partly because it was classified and partly because the people in the government just were not ready to put the kind of money needed into such an expensive vehicle for science. The idea of tens of millions of dollars going into a scientific satellite program just would not have been acceptable in those days.
In spite of intermittent efforts to get something moving in the scientific satellite area, this concept lapsed for several years while missile development went ahead. And, of course, it was the missiles that provided the boosters that finally made scientific satellites possible. This is all history, and I think you all know it. You remember that it was in 1955 that the President announced that a scientific satellite program would be part of the U.S. contribution to the IGY. It was two years later that the first satellite was launched by the Soviet Union, and about three months later the U.S. put up its first little scientific satellite, and the space age had started.

People sometimes forget that the third U.S. satellite, the Vanguard II, was a meteorological satellite. It did not work properly due to improper spin, and the experiment was essentially a failure. However, it is interesting to notice that the scientists who were planning this satellite program recognized right from the start that meteorological satellites would be a very important part of the scientific satellite program. I might mention that the Vanguard II, while it did not provide useful data, was nevertheless the training ground for some of the people who are still most active in the satellite business: Bill Stroud, Bill Nordberg, Rudy Hanel, Rudy Stampfel, Herb Butler and others. That Signal Corps group which subsequently moved to NASA to build Tiros and Nimbus cut its teeth, you might say, on Vanguard II.

Explorer VII was the first "thermal experiment," that is, it looked down at the ground to measure the heat budget of the atmosphere. This was one of the scientific satellites that came out during the IGY. It was here that Verner Soumi got into the business, and he is still very much in it with his University of Wisconsin colleagues.

The actual start of a program to look seriously at meteorological satellites did not begin until 1958 -- roughly three years after the scientific satellite program was started. Some people may not be aware that the Tiros program was actually begun under the Defence Department's
Advanced Research Program Agency (ARPA), and Roger Warner was the man who spearheaded this initial effort to get the Tiros program started. He had a meteorological satellite Advisory Committee, of which I was chairman, so I was able to watch this bird from the time the egg was laid. And many of the people who were engaged in this early effort (Bill Stroud, Herb Butler, Vern Soumi, Rudy Hanel, Bill Widger, Bill Nordberg, RCA's Sid Sternberg, Stan Greenfield, Charles Bates, Arnold Glazer, to mention some) are still working in the satellite program. In 1959 this project was quite properly turned over to the youthful NASA, since that is where it belonged, and most of the people who were working with the program simply went on working with it. And on April Fool's Day, 1960, Tiros I was launched, less than two years from its inception.

Now I come back to the question of the problems in using the data from these various Tiros satellites. Nearly all of you know the main reasons, because some of you have tried to use them and were frustrated and have gone on to other things, and some of you have tried to use them and solved the problems at great difficulty. There has also been an undercurrent of feeling that these cloud pictures were not enough, that you needed something more quantitative than the Tiros cloud pictures. You felt you needed temperatures, winds, pressure distributions, in order to really succeed in a conventional analysis -- and you were right, in a way. There has always been some reluctance to admit that these cloud pictures are useful as they are now proving to be.

But, let's face it, Tiros was a difficult system to work with. The system was designed with a series of compromises among what was available on the shelf (because we wanted to have something pretty quickly), what was feasible to develop within the payload constraints, and what we would like to have. Sometimes what we would really like to have was, in fact, far down the list in deciding what to accept. The Tiros satellites were spin stabilized for directing the TV cameras, and the infrared observations were obtained by a conical scan which made it difficult
to convert them to space and time. But it was practical to build such a satellite in 1958, and we did not want to wait for our dream satellite.

I wonder how many hundreds of man-years, though, have been spent in sorting out these TV pictures, and in the unraveling of the infrared data by sophisticated computer programs. This has been an effort where NASA and the Weather Bureau, I think, have carried the brunt of the work. Do you remember the big "Red Book" of the infrared data that came out? It was a magnificent tour-de-force in producing something orderly from a very complex set of raw data. We also should not forget that there were initially several other active centers in the country besides NASA and the U.S. Weather Bureau. Besides the University of Wisconsin, which I have already mentioned, there were the University of Chicago, where Ted Fujita rolled up his sleeves and made significant contributions to the use of the data, the ARACON group under Arnold Glaser (and later Bill Widger) which had some of the pioneers in learning how to use this data, and the Air Force's Cambridge Research Laboratory, notably John Conover, strongly supported by the Air Weather Service. The Navy contributed to the early development of gridding techniques, and the Naval meteorologists are being increasingly active in the use of meteorological satellite data. While most people were yearning for something better than Tiros, the people I mentioned rolled up their sleeves and got to work with what they had, but there were mighty few of them.

The next step forward in satellites was when Nimbus I was launched at 3:57 am on 28 August 1964, and the first anniversary of that date will occur during this meeting. It marked the beginning of a new era in a sense, because the Nimbus satellite was a sort of a "super-meteorological satellite," a great leap forward. The pictures can be used immediately; since it is looking straight down, it draws the map as it goes. The APT or automatic readout can give these pictures to meteorologists anywhere in the world who have the proper receiving equipment. One of the sensing systems, the high resolution infrared system (HRIR)
itself marked a significant step forward in how to look at the atmosphere, because it was a combination of heat sensor and photographic system. We will hear a lot more about this HRIR in the course of this meeting.

At the same time that the Nimbus development was going on, NASA and the Weather Bureau were working out another satellite concept which would be for regular operational use, the so-called TOS System (Tiros Operational Satellite). Although it is not strictly in the supersatellite class, it will be a polar-orbiting satellite, and it can provide a straight-down look and draw the same type of cloud-cover map that the Nimbus draws. So this kind of presentation will make it easier to use the satellite information, to correct real-time synoptic analyses, to fill in blind areas in the world, and to locate incipient storms. I think we are just beginning to understand the great value of this technique for supplementing the regular synoptic analysis.

As we develop the capacity to exploit this technique, we will learn more and it will become easier for research meteorologists and universities to use the data also. You all know the problems involved in converting the present information onto a map and comparing it. The new generation of satellites, I think, will make it much easier so we have already made a step forward in this respect. But there is still another step forward that is going to be taken. It has to do with a much broader possible advance in meteorology generally and perhaps we can approach this by asking a different sort of question: "What are the main advances both scientific and operational which we hope to see in meteorology?" I think we would come to the conclusion that probably number one, generally speaking, would be to understand the general circulation of the upper atmosphere: the large-scale motions, the heat balance of the atmosphere and all the complex interactions that go on in it which in fact are part of this atmospheric heat engine. If we could understand the general circulation, we would be able to make better forecasts, and better forecasts are an essential first step in any
scheme that anybody has proposed for weather modification. So all these things depend now on solving this scientific problem having to do with general circulation.

We can now ask ourselves the question: "What are the factors in the general circulation that are holding us up?" Well, my dynamic meteorologist friends tell me that one of the big problems is how to handle the question of convection in the atmosphere, particularly in the tropics -- the interchange between a predominantly water surface and the atmosphere and the way in which heat and moisture are injected into the atmosphere. A second problem is to get a true worldwide picture of the winds and temperature. If we could only do it just once, it would be a big step forward. But of course, in order to really do it, we have to do it every day to understand the general circulation. We have never really observed the motions and the temperature fields of the atmosphere because, well, we just do not have enough stations by a long shot. Better information on the atmospheric heat budget is essential in order to understand general circulation. And finally, we need a new generation of computers to handle the new data that we have just discussed.

I think it is clear that the first three of these special requirements are factors in which the satellite will play a role. This is very significant. Satellites will certainly contribute to the studies of tropical convections, cloud patterns and creating statistics which can be used in a computer model. Here the synchronous satellite is probably called for because this would be able to observe the development of the processes throughout the day and is what we need. Better wind and temperature data -- a tough one for the satellites to obtain -- may not be possible for the satellite to obtain, but we are optimistic, particularly here at NCAR where we have a balloon group working on this concept along with NASA. We are optimistic that some kind of combination of satellite and balloon system may be the answer to global weather observing systems.
Temperatures -- well, I won't even go into this at all now. The question of taking temperatures from a satellite is something we are going to talk about in great detail and you can decide for yourselves as to whether it is going to be feasible to get temperatures that can be useful in a global observation model.

For better information on the atmospheric energy budget, I may mention again Explorer VII, one of the early satellites to make observations of the heat budget of the atmosphere. We are still at it. We have a long way to go but already as a result of satellite observations, we know more about the heat budget, and consequently, we can define heat in the atmosphere better than we could before. I think this is an area where we have only just started to explore.

Now, there are a couple of other things which I did not mention: water vapor and precipitation. These are important in the general circulation models because water vapor, of course, plays a very major role in providing heat to the atmosphere when it rains. If we could observe this globally from satellites, this would also be a tremendous step forward. I do not know whether this is possible or not. There have been some suggestions, and maybe these will come out in the course of the discussions. But just keep in mind that anybody who can measure the precipitation pattern of the atmosphere from a satellite gets some type of fur-lined teapot.

Now, in conclusion, I would like to say that I have traced, hurriedly I fear, the origin of a concept, its early implementation by some of the people in this room, and its promise for the future. Nobody can predict the future but a lot of us have a very strong hunch that we have only seen the beginning of the use of this new tool and that our generation (and we will not have to wait for our grandchildren) will see the emergence of meteorological satellites as a mainstay in meteorological observing systems.

If this is to come about, it is not enough for a handful of
dedicated government servants at ESSA and NASA to carry this program along. The tradition in this country has been for the universities to provide the backbone of any research effort, and it should be true in this case also. Meteorologists in universities will have to provide the real push as far as the research use of this new tool is concerned, and they will be supplemented by and work with people in government laboratories and, of course, in industry. Those in charge of this program know this -- know that for a broader base in the program we need to get the university people into it. This meeting to provide more impetus for broadening the base for this joint enterprise has already begun, and more power to it.
My work over the past few years has been concerned mostly with the interpretation of the type of satellite picture which we will discuss during the next few days. At the start of this work, it soon became clear that a classification of clouds as seen by the Tiros system was needed simply because everything that we saw was on an unfamiliar scale. This was done after many comparisons of satellite pictures with surface observations, with cloud photography from the ground and from aircraft, and with weather radar observations. The clouds were classified in terms of their form, pattern, texture, structure, brightness, and dimensions of the patterns and forms. These characteristics are synthesized in the form of an organization chart (Fig. 1) to guide newcomers to the field of cloud interpretation from satellite photography. This guide applies to "wide angle pictures," or pictures having a resolution of about 5 km, obtained by Tiros satellites. Note that four qualitative levels of brightness are used. Trained interpreters can integrate these terms to produce fairly good results even when there are large variations in the illumination, such as those caused by the changing elevation of the sun. Still, there are cases where the brightness of the image can lead to misinterpretation, as shown in the overlapping portions of the two pictures in Fig. 2. One picture indicates cloud between the brightest clouds while the other picture indicates clear sky in the same area. In this case, the illumination is the same in both pictures; the difference arises in the TV system.
The problem becomes more acute if reliance is placed on digital processing of the data as illustrated by the same pictures in Fig. 3. The brightness of these pictures was digitized directly from the tapes in ten levels at 240 x 240 points. The information was then rectified and mosaiced to a Lambert Conformal projection, displayed on a CRT, and photographed.

Since the brightness of the image is so very important in determining the cloud characteristics, and since it varies with the system, the illumination, and the interpreter, considerable effort has gone into a method for determining brightness quantitatively and assessing the accuracy of this determination. The details of this work were reported in the *Journal of Applied Meteorology* (Conover, 1965). Briefly, this was done on a test set of Tiros VII data where system corrections were determined as best possible and applied. These corrections were for:

a) The nonuniformity across the lens-filter system in the satellite.

b) The nonuniformity across the CRT-camera system at the ground.

c) The warm up of the TV system on board the satellite.

d) The satellite shutter speed.

Application of these corrections will yield brightness with an accuracy of about ± 15%.

The response of satellite TV systems to light is different from that of the human eye. This difference is important when pictures of colored terrain or vegetation are under study. The relative spectral response curves of the TV system in Tiros VII and of the eye are shown in Fig. 4. The satellite "sees" nothing in the blue; it is most responsive in the orange and sees better than the eye in the red and near infrared. Brightness in the Tiros VII test case was expressed in terms of radiance received by the system. It is necessary to convert the measured radiance values to albedo before significant comparisons
can be made between clouds or other reflecting surfaces. The nomograms shown in Fig. 5 have been constructed to make this conversion. The following assumptions were made in their construction:

a) The reflecting surface was isotropic. This is not necessarily correct but at present little observational data exists to show the variations.

b) There was multiple Rayleigh scattering in the atmosphere above the reflecting surface. The data developed by Coulson (1959) were used to make these corrections.

c) The viewing angle of the satellite was always 22° from the vertical. The pictures that were used ranged from 0° to a little over 50° from the vertical. This can cause errors that may reach 5% when albedos are less than 15%.

d) Absorption was that due to 0.28 cm of ozone.

e) There was no Mie scattering in the atmosphere.

The effect of light reflected from the atmosphere is clearly shown in the nomogram for the 1-km level. The total thickness of nearly two atmospheres (down and back) contributes about 2 watts m⁻² sterad⁻¹ of scattered light when the sensor views a completely non-reflecting surface or one having an albedo of zero. As the reflecting surface rises in the atmosphere, the effect diminishes as shown in the nomograms for the 5- and 10-km levels.

Details of how the corrected radiance is obtained from negative films may be found in the paper which I referenced earlier (Conover, 1965).

From pictures obtained on the five separate orbits during the early life of the Tiros VII satellite, albedos were computed for some 86 points; 55 of these were cloud surfaces and the remainder were water, land, or snow surfaces. Each point was read in as many photographs as it appeared. This generally ranged from five to ten pictures because of a 10-sec picture interval time. The readings of each point were then reduced to comprise a series of albedos for that point.
Session I

Since the albedos of a single point varied considerably from picture to picture, an attempt was made to determine how much of this change was due to system error and how much was due to changing nadir and scattering angles.

Individual series of albedos were examined for single points. It was impossible to isolate from the noise a relation of albedo to nadir angle, even though some points were measured through nadirs which changed from 6 to 41°.

In a test to determine whether a relationship between scattering angle and albedo could be found, the layer of relatively homogeneous stratus and stratocumulus off the California coast was selected for measurement. Eleven points from an average of five consecutive pictures for each point were measured. No significant change in radiance was found for scattering angles ranging from 136 to 180°. Lower scattering angles could not be included without shifting to the measurement of other types of clouds whose reflection characteristics might be different. Other analyses of the albedos of similar clouds as a function of the azimuth angle, between the satellite and the sun, and the nadir angle, under essentially the same solar elevation, showed no systematic patterns. This may have been due to the high solar elevations which would tend to minimize these effects, according to Chu et al. (1962).

It is therefore concluded that variations in albedo due to non-Lambertian reflection characteristics of the cloud, at least at solar elevations above 48°, and to changing angles of view could not be detected over the error level of the system.

A summary of albedos determined from the satellite for various reflecting surfaces is given in Table 1 where n represents the number of reflecting points measured in the corresponding category and each point was measured, on the average, in five consecutive pictures. Since these are averages, solar elevations and nadir angles are not given, but they ranged from 48 to 87° and from 2 to 60°, respectively. Of the
Average albedos (A) in per cent determined by satellite for various cloud and terrestrial surfaces.

<table>
<thead>
<tr>
<th>Surface</th>
<th>n</th>
<th>A</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cumulonimbus—large and thick</td>
<td>8</td>
<td>92</td>
</tr>
<tr>
<td>Cumulonimbus—small, top E 6 km</td>
<td>1</td>
<td>86</td>
</tr>
<tr>
<td>Cirrostratus—thick with lower clouds and precipitation</td>
<td>7</td>
<td>74</td>
</tr>
<tr>
<td>Cirrostratus alone, over land</td>
<td>1</td>
<td>32</td>
</tr>
<tr>
<td>Cirrus alone, over land</td>
<td>2</td>
<td>36</td>
</tr>
<tr>
<td>Stratus—thick, approx. 0.5 km, over ocean</td>
<td>14</td>
<td>64</td>
</tr>
<tr>
<td>Stratus—thin, over ocean</td>
<td>2</td>
<td>42</td>
</tr>
<tr>
<td>Stratocumulus masses within cloud sheet over ocean</td>
<td>4</td>
<td>60</td>
</tr>
<tr>
<td>Stratocumulus—MCO, over land</td>
<td>3</td>
<td>68</td>
</tr>
<tr>
<td>Cumulus and stratocumulus—MCO, over land</td>
<td>4</td>
<td>69</td>
</tr>
<tr>
<td>Cumulus of fair weather—MCO, over land</td>
<td>2</td>
<td>29</td>
</tr>
<tr>
<td>Mostly snow-covered mts. above timer, 3–7 days old</td>
<td>3</td>
<td>59</td>
</tr>
<tr>
<td>Sand—White Sands, New Mexico</td>
<td>1</td>
<td>60</td>
</tr>
<tr>
<td>Sand—valleys, plains and slopes</td>
<td>5</td>
<td>27</td>
</tr>
<tr>
<td>Sand and brushwood</td>
<td>2</td>
<td>17</td>
</tr>
<tr>
<td>Coniferous forest</td>
<td>4</td>
<td>12</td>
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<td>Great Salt Lake</td>
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<td>9</td>
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<td>Ocean—Pacific</td>
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<tr>
<td>Ocean—Gulf of Mexico</td>
<td>6</td>
<td>9</td>
</tr>
<tr>
<td>Ocean—Gulf of Mexico—sunglint</td>
<td>3</td>
<td>17</td>
</tr>
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</table>
Session I

clouds used to determine an average for large and thick cumulonimbus, one was measured three times and another twice to give averages of 102 and 105%, respectively. These high values may have resulted from additional specular reflection from ice crystals above an already highly reflective cloud. The amount of additional radiance that might have been produced by lightning is not known, but probably it was not enough to account for the high values.

The table shows that one set of measurements on a cumulonimbus of much less vertical extent yielded an albedo of 86%. Frontal type clouds composed of cirrostratus and layers of middle and low clouds which yielded light precipitation were found to have an average albedo of 74%. Albedos of 32 and 36% were found for typical cirrostratus and cirrus. These values seem to be just above the threshold of transparency to objects of high contrast at the ground, such as a coastline separating semi-arid land and the sea. In one case of cirrus associated with the jet stream, a value of 25% was determined, compared with 18% found for the nearby land.

Clouds followed by the letters MCO represent "mostly cloud covered" or 50 to 80% cover, according to Meteorological Satellite Laboratory nomenclature. The table shows that stratocumulus MCO over land is slightly more reflective than stratocumulus masses within an extensive cloud sheet over the ocean, suggesting a greater ground than water contribution. Cumulus of fair weather MCO over land, verified from ground observations but shown only by a smooth, grey tone in the satellite picture, yield an albedo of 29% when integrated with the albedo of the ground and shadows below, which is known to be about 15%.

Other measurements, not tabulated here, reveal a decrease in albedo from 70 to 51% for a nearly complete snow cover 3 and 7 days old, respectively. The snow fell on rough, mountainous terrain above the tree line and totaled several inches. The 4-day change is considered quite representative, since a nearby coniferous forest in Yosemite, California, only changed from 12 to 14% as measured on the same
pictures. Also of interest is the albedo of 60\% found for White Sands, New Mexico, with a solar elevation of 78° and nadirs of 17 to 28°. This value may be compared with the 63\% measured by Coulson (1964)*, in the laboratory on a sample of gypsum from White Sands under identical light source and nadir-angle conditions. Marlatt (1965)*, recently obtained from an aircraft an albedo of 60\% over a comparable spectral range for the White Sands area.

Another series of albedos for the Mojave Desert, not included in Table 1, averaged 39\% with a solar elevation of 57°.

Incorporation of system corrections and conversion to albedo by digital processing, using the method just outlined, has been done experimentally under contract by General Dynamics. A sample showing the mosaics of four frames is shown in Fig. 6. The mosaic of uncorrected brightness is shown beside it in the same figure. Albedo is presented in nine levels of brightness corresponding to albedos of 10 to 90\%. Comparison of the mosaics showed a reduction of irregularities from the uncorrected brightness to the corrected albedo. These data were also printed out but are not included here.

From this discussion, I believe, I have illustrated the need for quantitative brightness determinations and conversion to albedo for improved interpretation of the pictures. To continue these determinations, improved calibrations and reductions will be necessary. In this respect, some improvements have been made since the Tiros VII tests. For example, the nonuniformity across the filter-lens video system can now be accurately determined. This is a contribution from the Weather Bureau; they have also improved the calibration system and developed a better light source for the absolute calibration. Other methods have been developed which will help to determine some of these corrections after the launch of the vehicle. For instance, on the Nimbus satellite, a gray scale has been added. This theoretically permits a check on the

*Personal communication.
calibration from the point where energy falls on the surface of the vidicon to the final display on film at the ground.

Other corrections which can be deduced after launch can be evaluated by a process called photo stacking, which was developed at General Dynamics. This involves scanning a large series of pictures and digitizing the data. The data are then added in the same format. In this way, provided the satellite is not stabilized so space always appears on the same part of the format, the various brightness patterns will average out to yield a pattern caused by the nonuniformities of the vidicon. When this is done in respect to time, the TV warm up can be approximated. Furthermore, this method can also be used to check the shutter speed and any nonlinearities in the scan which may develop. Nonlinearities in the scan were found in this way; they seemed to vary with the spin of the satellite as it cut through the magnetic field of the earth. The rasters, fortunately, were not otherwise distorted.

In regard to the assumption that isotropic reflection always takes place, we have initiated a program to study cloud reflection characteristics. We hope to measure the polarization and the spectral distribution of the energy reflected from clouds in respect to the sun angle and the viewing angle. These measurements will be made from an aircraft, so there will be a minimum of intervening atmosphere between the cloud and sensor to complicate the data reduction. At the same time, we will sample the reflecting cloud by measuring its thickness, liquid water content, and drop size distribution. This type of information will improve the interpretation of albedo measurements from satellites.

Other terms of the cloud classification are strongly dependent on resolution. Due to low resolution, a field of scattered cumulus may appear as a completely smooth portion of the picture, and if the scale of grayness in the picture becomes a little lighter, it may signify a uniformly broken layer of cumulus. This problem will exist as long as the resolution of previous Tiros systems is used. It is obvious that
resolution must be improved to delineate these differences.

An example of the large increase in meteorological information that can be gained from a system of higher resolution is shown in Fig. 7. This figure shows Nimbus and Tiros satellite pictures of the same area taken about 2-1/2 hr apart. For practical purposes, due to lens focal lengths, number of raster scans, picture format and satellite altitude, the resolution on the central or vertical Nimbus picture is about 1 km. In the Tiros pictures, opposite the side of the horizon or near the vertical, it is at best 5 km. Because of this difference, numerous details are visible in the Nimbus pictures which cannot be seen in the Tiros photographs. For example, individual cumulus clouds are visible. Bands of cumulus can be measured at spacings of 1.8 to 2.0 km, indicating a certain depth of overturning and wind direction. Some cumulus clouds are approaching the shower stage. The cirrus bands associated with the jet stream are clear cut and identifiable without doubt; their shadows indicate a cloud altitude of 8 km. Middle clouds can be distinguished from high clouds. Most of these details are invisible or doubtful in the Tiros pictures.

The problem remains of gridding or rectifying the pictures, which is necessary in most cases before serious use can be made of them. We badly need a rapid method of generating grids and dispensing them to researchers. Considerable time is required if the researcher decides to have gridded pictures sent to him. Gridding the pictures is a tedious job demanding large quantities of orbital and timing data. The accuracies of most of the grids are generally around 1° of latitude. The Fujita method can reduce this to 0.1 or 0.2°, and at least one simplified version of his method can be run through a computer.

Optical rectifications which involve the projection of pictures on curved surfaces have been tested and experimented with almost since the first satellite pictures were made in 1960. At Itel, a fairly simple device was developed from which it was possible to mosaic the pictures.
so they were geographically good, but control of their density was lost. The rectified mosaics which I have shown are costly but I believe they probably are the best answer to this problem.

In summary, I have shown that fairly good albedos can be obtained from the Tiros photography, but they obviously require much work. Corrections for each system must be determined and then applied. Current research will yield more information on the relation of albedo to sun and viewing angles and the physical characteristics of the cloud. The Tiros resolution does not permit definitive cloud type and structure characteristics in many cases. Resolution in the Nimbus photography is a great improvement; in this we can see individual clouds 1 km in diameter, at least in the vertical pictures. Furthermore, in future Nimbus vehicles it probably will be easier to determine albedos.

An efficient system for supplying grids or mosaics to the researcher is badly needed. However, the gridding of all Nimbus I pictures before they reached the user shows a step in the right direction.

These pictures, combined with the higher resolution IR data, which will also be in Nimbus, and which you will hear more about at this meeting, will serve well for depicting the physical processes which we all want to observe and study.

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Fig. 1 Guide to cloud interpretation from "wide angle" Tiros pictures (after Conover 1962 and 1963).
Fig. 2 Overlapping frames 24 and 29, Camera 1, of orbit 868/867 Tiros VII.
Fig. 3 Same pictures as in Fig. 2 presented on a CRT after digitization and geographical manipulation.
Fig. 4  Relative spectral response curves of a Tiros TV system and the eye.
Fig. 5 Nomograms for computing albedo of reflecting surfaces at 1-, 5- and 10-km altitude, Camera 1, Tiros VII.
Fig. 6 Mosaics of uncorrected brightness and corrected albedo for tape frames 20, 23, 26 and 29, orbit 868/867, Tiros VII, 1/4 resolution, or 120 x 120 points.
Fig. 7  Comparison of Nimbus and Tiros photographs which partially cover the same area at slightly different times.
SESSION II

TROPICAL ANALYSIS AND TROPICAL STORMS
USE OF SATELLITE DATA IN THE TROPICS*

L. F. Hubert
National Weather Satellite Center, ESSA

This presentation will illustrate some of the uses of satellite data in connection with tropical storms and tropical analysis. It will also present some of the questions which are still open. I am going to concentrate very largely on the use of picture data because the pictures have been used extensively while the infrared data have not been much used in analysis. I can only suggest some avenues of research.

In connection with satellite pictures of tropical storms, the pictures show there is a hierarchy of patterns that exist as a storm develops from a minor wave or disturbance. As the storm progresses up to maximum intensity, the cloud pattern evolves through a detectable sequence. Figure 1 illustrates this progression. The illustration is schematic in that it is made up of many storms; it was not possible to follow a single disturbance through its whole history because of gaps in the satellite coverage.

A typical progression would be as follows: First an area of dense cloudiness can be seen with no particular pattern or curvature (Fig. 1 A). If a tropical disturbance is generated, the cloud pattern takes on a more curved configuration (Fig. 1, B and C). Frequently thin spiral bands can be distinguished well outside the main overcast area in the later stages, but many storms have formed and intensified without being photographed in the pattern of Fig. 1 C. (Since satellite pictures are typically

*Abbreviated version of paper presented at meeting.
taken only once per day, any rapidly changing pattern might be missed. Therefore details of this pattern progression are still unknown.)

The earliest stage that can be confidently identified as a potential hurricane is illustrated in Fig. 1 D. Here the cirrus can be seen "feathering off" the storm in several different directions, apparently the result of active convection and the production of copious cirrus. The great amount of latent heat released and the production of a high-tropospheric anticyclone set the stage for further intensification. Most storms of this stage go on to become hurricanes. In the tropical oceans many disturbances similar to those shown in Fig. 1, A and B, exist but never intensify.

An empirical system has been developed to estimate the maximum wind speed in hurricanes by classifying the pictures. (This technique is described in Meteorological Satellite Laboratory Report 33, Washington, D.C., February 1965.) Two parameters are used. The size of the main overcast area is measured on rectified pictures and the degree of bandedness and organization of the pattern is classified. A subjective ranking from 1 to 4 was made for each hurricane picture to quantify the degree of pattern organization. A feature important to the classification is whether or not an eye can be seen. If an eye appears the pattern is classified either 3 or 4, depending upon whether the other pattern characteristics are present. Even if an eye is not visible the other pattern features may be sufficient to put it into one of these higher categories. The more poorly organized storms are classified 1 or 2.

Figure 2 is a graph on which are plotted the observed maximum wind speeds along the ordinate versus the diameter of the overcast area along the abscissa. The lines drawn in the body of the graph are approximate isopleths of the pattern categories. The maximum wind speed has a surprisingly good correlation with the diameter of the overcast area. This diagram is used operationally by measuring the overcast diameter, proceeding up to the appropriate category isopleth and reading from the ordinate the estimated maximum wind speed for that storm.
To test the usefulness of this system for operational estimates, a set of storms (not used in development of the graph) was classified by two analysts independently. This permitted the results to be analyzed for accuracy of the maximum wind estimates and also illustrated the differences introduced by the subjective judgements the analysts must make.

**TABLE 1**

Maximum Wind Speed (MWS) derived from independent data by two analysts, compared with observed MWS.

<table>
<thead>
<tr>
<th>TEST WITH INDEPENDENT DATA</th>
<th>DERIV. OBS. MWS</th>
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<tbody>
<tr>
<td>ANALYST 1</td>
<td>1</td>
</tr>
<tr>
<td>ANALYST 2</td>
<td>0</td>
</tr>
<tr>
<td>TOTALS</td>
<td>1</td>
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<tr>
<td>15-KNOT CLASS INTERVALS</td>
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Table 1 shows the results where the wind speeds are expressed in terms of 15-knot class intervals. In general, it can be said that the total error due both to subjective judgements and to the shortcomings of the system itself, does not destroy the operational usefulness of
Session II

this system. Something over 80% of the estimates of maximum wind ty-
phoons and hurricanes are within ± 15 knots of the reconnaissance-
observed wind speeds.

Storm pictures from satellites frequently show a significant fea-
ture of developing and mature storms -- the subsidence around the periph-
ery. The zone of suppressed convection at the hurricane edge has been
observed earlier, but its persistence and extent are demonstrated by sat-
ellite data. An annular ring of subsidence, sometimes broken only on one
quadrant, is commonly photographed. Figure 3 is a mosaic made from 70-mm
film from the Mercury Program. Notice the clear area around the cirrus
deck -- even the low clouds within 2000 ft of the ocean surface have
been evaporated.

I wonder if this might not be the manifestation of an atmospheric
governor? Air sinking at the edge of the cirrus shield and recirculating
through the storm comprises the type of circulation which destroys the
solenoids available for conversion to kinetic energy. It seems possible
therefore, that this part of the circulation provides a brake on the
storm strength. Furthermore, since the diameter of the cloud shield that
we see on satellite pictures is probably the diameter of this circle of
subsidence, it is suggested that this reflects the mechanism that is re-
sponsible for our empirical results (speed correlation with diameter).
This is an important area for further research.

Radiation data are also of great use in tropical storm research.
Figure 4 illustrates one obvious use, that of maintaining day and night
surveillance of a storm. The upper part of the figure shows isolines of
approximate effective radiating temperature in the 8 to 12 μ water
vapor window. Along the bottom of the page are the pictures taken ap-
proximately 12 hr earlier than the radiation data. The intensification
and motion of the system are quite easily seen by comparing the pro-
gression of radiation pattern and picture data. The hurricane symbol on
the last radiation pattern and the last picture show that the highest,
coldest clouds are well to the east of the circulation center. The location of the circulation center was obtained by use of standard data.

SUMMARY OF TROPICAL STORM RESULTS

The important result of studies of tropical storms is that operational information is already available. Furthermore, the progression of patterns leading up to hurricane formation gives promise of important input to research on these storms.

It has been known for some time that hurricanes form in pre-existing disturbances. Riehl and others have described the wave-like perturbations and the weather distribution of these early disturbances. The important difference illustrated here is the tendency for the initial circulation to commence at the edge or even west of the main area of convection. This calls for models and explanations that are not adequately provided by the simple wave disturbance in the easterlies. I do not suggest that all hurricanes and tropical storms are generated in this manner, but the satellite data indicate that a significant number do form in the manner described. No doubt some storms generate under the enhanced convection area more in agreement with the conventional model. The complete coverage of the TOS System and the continuous surveillance of the earth-synchronous satellite should provide a measure of the relative frequencies of these two different modes of storm genesis.

NON-STORM SATELLITE DATA

Cirrus can frequently be recognized on satellite pictures because it has a characteristic "feathered out" appearance when it is being sheared off cumulonimbi. This has already been mentioned in connection with developing tropical storms. Conglomerates of convection can frequently be seen in the tropics that are not associated with developing storms (for example, see Fig. 5). Such patterns frequently can be interpreted in terms of high-level wind directions and thereby provide valuable information where standard data are not available. However, caution must be used in interpreting these patterns. These cirrus
fingers (e.g., Fig. 5 at about 19°N, 171°E) frequently point downwind because the wind at cirrus levels is much faster than the lower wind where the main body of the cloud exists. Actually the cirrus streams off down-shear. The difference between high-level wind direction and shear direction between lower and upper levels must be kept in mind.

In addition, there is another factor: a developing cumulonimbus would have a spreading anvil even if no shear existed. Other workers have indicated that the spreading anvils might have velocities of 10 to 15 knots due to the cloud dynamics alone. Clearly this must be added vectorially to the vertical shear vector. Figure 5 illustrates the shear observed near the cirrus. The largest cirrus fingers are streaming off well to the south of east, but neither the shear direction nor the wind direction has exactly this orientation. The difference is probably made up of the extra component furnished by the cloud spreading itself.

Patterns consisting of cumulus and stratus clouds can frequently be distinguished even though infrared temperature data are not available. Work is presently going on to derive wind direction and speed, if possible, from patterns. Rogers (1965) published a study of southern hemisphere patterns in high latitudes. He found some correlation between certain cellular patterns with wind speed and direction. Private communication from him since that time, however, indicates that in the tropics and in the northern hemisphere these correlations have not held up. The detailed results should therefore not be applied, but the possibilities indicated by this research are suggestive. The patterns are shown schematically in Fig. 6 A, and Fig. 6 B is an example of a typical picture. It seems probable that vertical shear may be more important than the actual wind in these patterns. Undoubtedly further studies of this subject will soon appear.

Figure 7 shows the early stages of a tropical storm with organization in the low cloud layers extending vast distances to the north and northwest of the circulation center. The orientation of the bands be-
tween 15 and 20°N and west of 55°W is at right angles to the low-level flow. The upper-air stations indicated at locations 1 through 4 are the only direct evidence that winds at cloud levels are also from the east and southeast. However, inspection of the cloud rows shows evidence of shearing toward the west -- further support of the belief that winds in the trade wind layer were all easterly. It therefore appears that the large, well-formed lines of cumulus are very nearly perpendicular to both wind direction and shear direction throughout the cloud layer. There can be little doubt that this organization is associated with the generating storm. Other cases of similar pattern have been seen. The question posed by this picture is the nature of the mechanism that has organized these north-south cloud lines. Their orientation suggests instability "waves" that are proceeding outward from the storm. A more complete answer will be forthcoming, we hope, when patterns such as this stimulate research.

SUMMARY

I think it is clear that real operational information is available from satellite data. Many tantalizing questions have been raised by the data. An important question concerns a model of tropical storm genesis that produces the first circulation at the edge of the convective area instead of within the area of maximum convection. Important to the interpretation of pictures in terms of flow field is the question of the various mechanisms that orient the large cloud lines. I hope this talk has stimulated your interest and will lead to new research which will answer some of these questions.

REFERENCE

Fig. 1 Patterns, as seen by the satellites, depicting stages of development of tropical cyclones.
Fig. 2 Maximum wind speed versus diameter of overcast circle, with category isopleths. Plotted points are the dependent data. Category curves are eye-fit with subjective weights to extreme values, e.g., points "b" and "e."
Fig. 3 Hurricane Debbie -- 70-mm film mosaic
Fig. 4  Day and night surveillance of a storm using both picture and radiation data,
Fig. 5  Cloud shear at cirrus levels.
CELL PATTERN | WIND SPEED RANGE
--- | ---
a. Regular Polygonal Cells | 0-7 knots
b. Elliptical Chain | 8-22 knots, lower range
c. Scalloped, with crosswind links missing. | 8-22 knots, higher range
d. Blown-out Ellipses | 23-37 knots, lower range
e. Rows | 23-37 knots, higher range

Fig. 6 A. Relationship between wind velocity and cumuliform cellular patterns. Sketches illustrate cloud patterns observed in Tiros photographs. Arrows indicate wind direction inferred from cloud patterns.

B. Example of row pattern related to 23 to 27 knots surface wind speed with sketch of cloud pattern.
Fig. 7 Early stages of a tropical storm with organization in the low cloud layers to north and northwest of circulation center.
SOME PROBLEMS IN THE USE OF SATELLITE DATA

Herbert Riehl*
Colorado State University

Gentlemen, it is an honor to be here and to be the first discussant following Lester Hubert's very interesting and informative paper. From what I understand, the discussant, in his capacity as a member of the academic community and user of satellite data, has the objective of discussing some of his experiences encountered in using the data and at the same time offering some constructive criticism on the satellite program, either in part or as a whole.

The satellite program is a very impressive one, and I for one must admit that it is very difficult to find fault with it. However, when it comes to the point of actually using the observational data, many problems arise. I am sure that in time to come these problems will be satisfactorily solved, but presently some of you interested in starting some satellite research work may like to be informed about some of these difficulties.

We entered into satellite work in connection with our Southeast Asia weather research program. From the knowledge of what satellite data, in principle, were available I constructed, perhaps a little naively, a timetable during which we could inspect, discuss and analyze the data. A total period of six months was designated for this. However, things proved vastly different. Before the data could be used, we had to buy projectors and other machines in order to use the NASA films.

*Leading discussant.
The equipment arrived six months later. It was six months after the intended date of completion that the first mosaics began to appear on our maps, and by that time we were also quite a few thousand dollars out of pocket.

If this sort of situation still exists at present, then I am really doubtful if this is the most efficient way at our disposal. It would mean that all meteorology departments and other organizations would have to acquire all this equipment and personnel, the cost of which would certainly be a deterrent to many to do meteorological satellite research. Processing by a centralized agency of meteorological satellite data ready for use by the consumer is the inevitable answer.

Our efforts were concentrated on the data obtained from the 8 to 12 micron radiation channel. About two years elapsed before we were able to utilize the different orbits for analysis over Southeast Asia. A computer program designed by Professor Baer, and which will be outlined in a few moments, was used to deal efficiently with the large amount of data. NASA's help and cooperation by Dr. Nordberg and Mr. Bandeen are gratefully acknowledged. I also recognize the courtesy and cooperation of the Western Data Processing Center at the University of California at Los Angeles, who assisted us with our computer program. Furthermore, NCAR placed their facilities at our disposal to develop the program for the pictorial representation of the data. I repeat that without the help and cooperation of these organizations we would absolutely never have been able to conclude this satellite data project. I emphasize that, in order to encourage meteorological research with satellite data, a centralized agency would have to be responsible for processing the data so that it would be ready for use by smaller groups or institutions or even individuals.

Now, with the permission of the Chairman, I would like to introduce to you Mr. Kamm, mathematician in the Atmospheric Sciences Department at Colorado State University, who has collaborated with Baer on the development of this particular program. He will briefly describe the process by which we finally obtained the data usable for our purpose.
COMPUTER PROGRAM

W. Kamm *

Colorado State University

The basic data available were the binary tapes of Tiros III, IV and VII channel 2 (8 to 12 micron) radiation data. The object of the computer program was to enable us to extract required data for any orbit and any particular geographic region. In our case the region in question was that of Southeast Asia.

One of the main problems encountered was that of coping with the three types of scanning modes (single open, closed, and alternating open) of the satellite. Especially in the case of the closed mode attitude of the satellite, geographic mislocation of the data was noticed. Our first efforts were therefore concentrated on the single open mode data which excelled in quality. This led to loss of interesting data, and it was consequently decided to use the closed mode data as best we could despite the geographic mislocation. Finally, therefore, the program was able to accept all modes.

In order to overcome the uneven distribution of the radiation values over the region of our choice, it was advisable to establish a uniform grid of points over the region. Furthermore, since the data density about most gridpoints is reasonably high, we elected to fit the data about each gridpoint to a quadratic surface similarly to the method used by Gilchrist and Cressman (1954). It also was necessary to select an influence region about each gridpoint in order to establish the relevant

* Discussant.
data point. The size of this region is to depend on the amount of
smoothing desired or the data density.

Once the final grid is computed, contour analysis of the cloud
patterns can be performed by hand or use can be made of the NCAR X-Y
plotter. Test runs have shown that the analyses performed compare very
favorably with the actual Tiros cloud photographs.

For those of you in the audience who wish to obtain more detail
about the program, copies of the report prepared by Prof. Baer and me
(1965) are available. Also the computer program itself is available to
anyone who wishes to use it.

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SESSION III

SEVERE LOCAL STORMS (MESO-ANALYSIS)
USE OF SATELLITE DATA IN MESOMETEOROLOGY

Tetsuya Fujita
The University of Chicago

INTRODUCTION

The use of satellite data in severe storms research necessitates accuracy in positions and resolutions in horizontal and vertical directions. In most cases, therefore, individual satellite pictures and radiation data are analyzed in detail in an attempt to depict patterns of mesoscale nephysystems which do not always appear in operational nephanalyses and small-scale grid print maps of radiation data.

This paper presents some results of both radiation and photographic analyses which have potential value in the study of severe storms. Included are discussions of: (1) result of combined analysis of satellite picture and radar echoes photographed simultaneously; (2) the Palm Sunday tornadoes of 1963; (3) the need for radiation data in obtaining vertical resolution; (4) the radiation pattern analysis of Hurricane Anna in 1961; and (5) the basic problems in determining cloud covers and emissivity from radiation data.

COMBINED ANALYSIS OF SATELLITE PHOTOGRAPHS AND RADAR ECHOES

Young thunderstorm cells are known to appear on a satellite picture as very bright spots which can be distinguished from nearby gray areas consisting of scattered or broken cumuli. As clouds get older, their tops are covered with anvil clouds which are sometimes so thick that thunderstorm cells can no longer be identified.
Radar echoes, on the other hand, represent only the areas of precipitation which do not always show a pattern of organization until they form a squall line or precipitation band. If the displacement of echoes is determined from several frames exposed at proper time intervals, the motion of each echo can be computed to show the field of motion (which at the present time cannot be obtained from satellite pictures).

The combined presentation of cloud and echo patterns in Fig. 1 effectively shows the dynamical aspects of the storm. The velocities of echoes are presented in standard wind symbols. The picture includes the northern sector of Typhoon Bess on 7 August 1963 when the storm center south of Shikoku, Japan was moving north at about 6 knots. Just inside the cirrus shield, the echo speed was about 55 knots. Of interest is a localized area of high echo speed near the center of the picture where the echo motion deviates considerably outward from the tangential motion, suggesting that an arc-shaped echo near 32°N and 133°E represents the convergence line along the leading edge of a group of fast-moving echoes. A large deck of cirrus clouds gives an impression that it was left behind by these fast-moving echoes.

Such a combined presentation of echo and cloud patterns is the standard method that is applied to the satellite meteorological study of severe storms producing neph systems.

**DUST STORMS ASSOCIATED WITH THE PALM SUNDAY TORNADOES, 1963**

Satellite pictures are useful not only for the study of severe storms producing neph systems but also for the identification of the air mass characteristics behind the cold front related to the storm development.

A Tiros IX picture (Fig. 2) was taken at about the time when the first storm of the Palm Sunday tornadoes, 11 April 1963, formed in Iowa. Of particular interest is an area of faint cloud near the Kansas-Oklahoma border. At first glance, it looks very much like an image of high clouds with curved streaks. A synoptic chart (Fig. 3) indicates that five air...
masses identified with letters A through E are involved: A, moist, warm air mass; B, dry, cold; C, moist, cold; D, partly dry, cold; and E, remnant of a meso-high. The Kansas-Oklahoma border on this map is in the region of strong westerlies with wind speed as high as 30 knots.

Such strong winds coupled with very low dew point temperature are likely to produce unusually thick dust storms. The visibility chart (Fig. 4) presents this situation. As expected, a region of very high visibility is seen to the west of the Rockies, where descending motion created a clear sky so that one can see mountains some 100 mi away. As the distance from the mountain increases eastward, visibility drops to 10 mi or less over the Oklahoma Panhandle. Further east, a number of stations reported dust and blowing dust with visibility as low as 2 mi at Wichita, Kansas. A large island of low visibility is located at the center of the dry, cold air mass identified with the letter B.

When this area of low visibility and the satellite picture are carefully compared, it is seen that the area of faint clouds corresponds to the region of the duststorm. Because of large areas of plowed fields in the Midwest early in April, the reflectivity of the background is several times higher than that of water. In order to be seen in a satellite picture against such a background, the duststorm must be dense and must extend high. A vertical cross-section made along the isotherm, \( \theta = 279^\circ K \), on the surface (Fig. 5), reveals that a layer of isentropic mixing extends almost 10,000 ft above the surface. Thus the region east of the Rockies and west of the dry, cold front was favorable for the development of the dense, thick duststorms that appeared on the satellite photograph.

It should, therefore, be noted that satellite pictures can be used in identifying situations of severe storm development which are characterized by the rapid movement of dry air from the Rockies in particular.

**RADIATION PATTERNS DEPICTED BY 5-CHANNEL SCANNING RADIOMETERS**

Tiros-borne scanning radiometers with a 5° field of view measure terrestrial radiation in five different spectral ranges. When they are
combined with satellite pictures taken within a few minutes of the scanning time, the radiation patterns give quantitative information about neph systems appearing in the pictures.

As an example, Hurricane Anna of 21 July 1961 was selected. Figure 6 represents the cloud distribution when the storm was located in the Caribbean north of the Colombian coast. It is evident that such a neph analysis gives only limited information on the vertical extent of clouds, no matter how carefully and accurately they are mapped.

Using attitude data from Tiros III, 132 R/O 133, every eighth scan line was drawn in Fig. 7. Black areas denote 20% power scan spots surrounded by isolines of power at 20% increment, with the outermost ellipses indicating 80% power scan spots. The directions of each scan obtained by drawing great circles connecting each scan point with the subsatellite points are shown by arrows, which also represent the major axes of elliptic scan spots. Nadir angles of scan are contoured by dashed lines.

The Channel 1 pattern in Fig. 8, shown by isotherms of equivalent blackbody temperatures, reveals that the region of cloud-free ocean was about $-33^\circ C$. This suggests that the water-vapor channel does not penetrate the deep layers of the atmosphere; instead, its temperature over the cloud-free ocean corresponds to that of 300 mb. When the cloud tops are higher than this surface, the sensor measures the cloud-top temperature instead. The hurricane-top temperature, $-49^\circ C$, corresponding to about a 220-mb temperature, is too high when compared with the window-channel temperature. The effect of stratospheric moisture has been discussed by several researchers in an effort to explain the difference. Relatively small areas of thunderstorms north of Panama and in Venezuela appear as cold spots, suggesting that their vertical growth was beyond the 300-mb surface.

The Channel 2 pattern of equivalent blackbody temperature shows a detailed structure of the hurricane cirrus shield. The coldest spot of
-73°C probably represents a group of penetrative towers forming a rain-band near the eye wall. A scan line identified with scan angles between 130 and 240° was drawn through the storm so that the analog trace presented in Fig. 10 can be examined for the purpose of converting the time changes in radiation into space changes. The scan angles on the analog traces correspond to those in Fig. 9. When the equivalent blackbody temperatures of the thunderstorms in Channel 1 and 2 charts are compared, it will be found that they coincide within an accuracy of 5°C. Over the hurricane area, the Channel 1 temperature is about 10°C warmer. No reasons for such a discrepancy have been found.

The Channel 3 radiometer gives the albedos when the measured effective radiant emittance is divided by the effective solar constant multiplied by the cosine of the solar zenith angle (Fig. 11). The heavy lines labeled with the numbers 700, 710, 720, etc. represent this denominator. Channel 3 effective radiant emittances are contoured at intervals of 10 watts. It should be noted that the albedo of the hurricane top is about 0.5 and more or less uniform, while the temperature varies from -73 to -49°C.

The Channel 4 radiation pattern over the hurricane cirrus shield shows less temperature variation than does Channel 2 (Fig. 12). It is probably because this thermal channel does not penetrate near to the ocean surface as does Channel 2. When the scan spot moves over the thin cirrus deck, the thermal channel is not affected by the underlying warm ocean as much as the window channel. This fact can be shown by comparing the equivalent blackbody temperatures over the cloud-free ocean measured by Channel 2, about +10°C, and by Channel 4, about -5°C. The difference is about 15°C.

Channel 5 patterns should coincide with the vidicon image if their resolutions are identical. Due to a 5° field of view, however, only smoothed patterns of cloud reflectance appear in Fig. 13.
BASIC PROBLEMS OF CLOUD TEMPERATURE, COVER, EMISSIVITY, AND WHITENESS HANDLED FROM SATELLITES

Measured by Tiros Medium Resolution Radiometers (MRR) and Nimbus High Resolution Infrared Radiometers (HRIR) are the combined values of basic parameters designating both clouds and the background inside the radiometer's field of view. These parameters are temperature, areal cover, emissivity, and whiteness.

In the study of mesoscale nephsystems accompanied by severe storms, these basic parameters are of vital importance because the measured effective radiant emittances do not tell us anything about the individual values of these parameters.

The isotherms of equivalent blackbody temperatures measured by Channel 2 MRR of Tiros IV are presented in Fig. 14. A large area of thunderstorms over Italy accompanies a long anvil cloud extending downwind toward the lower right corner of the picture. The equivalent blackbody temperature of the anvil changes from \(-30^\circ C\) near the edge of the parent thunderstorm to \(-5^\circ C\), which certainly is too high for the anvil temperature.

If we assume that the temperature of ice crystals forming the anvil was \(-39^\circ C\), some \(4^\circ C\) warmer than the cumulonimbus top temperature, the measured anvil temperature can be explained by partial filling of the radiometer's field of view and by the emissivity of the thin anvil. Analysis showed that eight scan lines crossed the anvil in question, making it possible to compute the basic parameters when the scan spots were located at the intersections of the anvil axis and the scan lines.

The measured values of Channels 2 and 3 are shown in the top diagram in Fig. 15. From these and the assumed anvil temperature, the emissivity in the figure was computed. It decreased from 1.0 to about 0.4 as the distance from the parent thunderstorm increased.

Other important basic cloud parameters are cloud covers which vary according to the spectral response. The equivalent blackbody cloud
cover, $N_B$, is defined as the amount of blackbody cloud required to replace the real cloud without changing the measured effective radiant emittance. The equivalent white cloud cover, $N_W$, is also defined as the amount of standard white cloud required to replace the real cloud. The third cloud cover, called the particle cloud cover, $N_p$, is the amount obtained by drawing a boundary around the cloud particles. If a cloud shows a distinct boundary in a picture, we may determine the particle cloud cover from the satellite picture. Changes in these three cloud covers are also shown in Fig. 15.

It was found that $N_p$ is the largest, $N_B$ the next, and $N_W$ the smallest. They are interrelated by the ratios called the emissivity $= n_B : n_p$ and the whiteness $= n_W : n_B$. When a cloud consists of small cloud droplets near its top, its whiteness is very close to 1.0. For ice clouds, however, the whiteness is not very high, because large ice crystals do not reflect much sunlight but radiate as blackbodies. Whiteness can be computed from measured short- and long-wave MRR data as a ratio of $\overline{\pi}_{Cr}$, the critical pseudo-radiant emittance, and $\overline{\pi}$, the measured pseudo-radiant emittance. Thus the area of ice-crystal clouds can be identified from MRR data. Both theoretical and experimental work on this subject is now being done at the University of Chicago.

DISCUSSION

Questions and answers went on throughout the talk to clarify minor points since it had been suggested in advance that the speaker wished to entertain questions while the slides were displayed.

Koteswaram: How do you distinguish dust in Fig. 2?

Fujita: Synoptic reports which I obtained from the area of faint clouds indicated "blowing dust, some with less than 2-mi visibility."
Brewer: What direction is the sunlight?

Fujita: From south-southeast when viewed from the area of the dust-storms.

Pallman: Are there any ideas about the route and extent of the dust?

Fujita: The vertical extent will be about 10,000 ft, but I do not know where it came from.

Bates: I can answer that. There are photographs from the Weather Bureau's B-57 with dust at 39,000 ft.

Conover: We have seen dust clouds going westward from Africa.

Nordberg: How could you tell the difference in the movement of ordinary and tornado-producing thunderstorms from the satellite picture?

Fujita: In fact, I could not, but I determined their motion from radar pictures.

Nordberg: How do you propose to tell the difference by satellite?

Fujita: Large storms can be followed at least one or two hours. If we take time-lapse photographs from a synchronous orbiting satellite, they will show that a rotating and a nonrotating thunderstorm move in different directions.

Zipser: Dr. Newton and others have pointed out in the past that this deviation of large from small storms is observed frequently, and certainly not always necessarily associated with rotating systems.

Fujita: The motion of rotating cumulonimbus is different from that of large storms discussed by Dr. Newton and others. I would like to emphasize the fact that small, large, and rotating storms move respectively toward three different directions.

Zipser: Second, you pointed out earlier and quite conclusively, I think, that as the age of storms increases, the anvil will very rapidly cover practically everything, and it would be very difficult to distinguish the large from the small systems, would it not?

Fujita: The anvil may cover the cloud tops, but the overshooting tops are usually brighter than the flat anvil area. If we get radiation data such as Nimbus HRIR, we can also detect the overshooting tops because they are colder than the flat anvil top.
Fig. 1 Radar echoes superimposed upon an enlarged satellite picture gridded with 1° latitudes and longitudes. An area of high-speed echoes near the center of the figure is covered with a large outer cirrus deck. Note the direction of motion of the high-speed echoes, which is about 30° outward compared with the general direction of the echoes in the vicinity.
Fig. 2. Satellite picture showing clear areas behind the dry, cold front. Tiros IX, Orbit 960, Frame 9 exposed at 12h 42.3m CST, 11 April 1963.
Fig. 3 Surface winds and isobars at 1200 CST, 11 April 1963. A long wind barb and a flag were drawn to represent 5 and 25 knots, respectively, in order to emphasize the intensity of surface winds. Air masses involved are: A, moist, warm air; B, dry, cold air; C, moist, colder air; D, moist, cold air; and E, outflow from a dissipating meso-high.
Fig. 4  Visibility chart for 1200 CST, 11 April 1963. Contoured for every 2 mi with thin contours and for every 10 miles with heavy contour lines. Areas of visibility are stippled.
Fig. 5 Vertical cross section along 297°K isentrope and its extension outside the dry, cold air at 1800 CST, 11 April 1963. There was little temperature difference on either side of the dry cold front between Peoria and Flint. Due to a steep lapse rate to the west of the front, significant cold advection is seen near the 700-mb surface located below a stable layer topping the isentropic flow from the west.
Fig. 6  Rectified cloud pattern of Hurricane Anna at 1548 GMT, 21 July 1961.
Fig. 7  Scan spots over the area of Hurricane Anna. Tiros III, 132 R/O 133, 21 July 1961.
Fig. 8 Equivalent blackbody temperature from Channel 1.
Fig. 9  Equivalent blackbody temperature from Channel 2.
Fig. 10  Analog traces showing Channel 2 variation along the scan line in Fig. 9, one recorded by oscillograph, the other by Brush Recorder.
Fig. 11 Effective radiant emittance from Channel 3.
Fig. 12  Equivalent blackbody temperature from Channel 4.
Fig. 13 Effective radiant emittance from Channel 5.
Fig. 14 Isotherms of the equivalent blackbody temperature superimposed upon a satellite picture within 5 min of the time of radiation measurement. Tiros IV, Orbit 99, 15 February 1962. The picture covers the western Mediterranean.
Fig. 15 Cloud parameters computed from long- and short-wave radiation data. Note that the cloud covers, as well as whiteness and emissivity, decrease exponentially as a function of distance from the parent thunderstorm.
USE OF SATELLITE DATA IN SEVERE THUNDERSTORM RESEARCH

L. D. Sanders*
National Severe Storms Laboratory, ESSA

Since up until now we have had no experience in the use of satellite data for severe thunderstorm research, I feel that my presence here is more that of an observer than a qualified participant of this conference.

As a consequence of our lack of knowledge of the potentialities of satellite data applications in severe storm research, I may then perhaps propose questions rather than make definite contributions.

One of our basic questions has been how we can use the data to provide information that is not available by any other means. This would apply to both thunderstorm research and to operational forecasting and forecasting research. In other words, would the satellite data enable us to determine, well in advance of time, the areas of research interest so that we can concentrate our aircraft, surface and upper observations, radar, etc. for optimum research activities. Up until now the observational commitment has been made by anticipating the areas of development of severe thunderstorms perhaps not longer than 1 to 2 hr before the event to be investigated takes place. There appears to be no doubt that the use of the satellite can be of immense help in this respect.

Another question we have posed is whether the data have sufficient resolution to define the mesoscale features we are interested in. From the material I have seen so far the answer appears to be in the affirmative. Here, however, the availability of the data in usable form without

*Leading discussant.
additional laborious processing is of importance. Earlier data have proved to be disappointing in this respect but at present, especially through the efforts of Professor Fujita, things are far more optimistic in this respect. Fujita has also brought out the point, perhaps not too strongly, that radar is still the most important basic tool for operational use for the severe storms forecaster and that satellite photography is probably never going to be a substitute for this. It definitely can be an adjunct to it in providing additional information.

Pertinent topics of interest to the severe storms forecasters are such things as the depth and structure of the moist layer of the maritime tropical air, the precise location of the dry front (dew point front or the moisture discontinuity). The latter can be determined fairly accurately by performing a dew point analysis of the hourly observations.

The depth and slope of the western edge of this moist layer is more difficult to obtain. This is largely due to the inadequate space and time coverage of the upper-air network. From the point of view of the satellite as an observation platform, our enquiry would be what the satellite can do for us in that respect. Present opinion is that little hope can be given us in that direction.

The question of the detection of air flow through clouds and cloud patterns is also important. Other matters coupled with this are the detection of low-level convergent cloud patterns, high-level divergent cloud patterns, the precise location of the jet stream and the jet stream maxima. It would be extremely useful if satellites were capable of observing these parameters.

In regard to the detection of thunderstorms themselves, it would be extremely useful to have the resolution of identifying single storms approaching the severe stage. The same applies to the identification of thunderstorms or lines of thunderstorms in the early stages of development. Apart from the smallest cumuliform development, present materials viewed seem to indicate that the resolution for these purposes is adequate.
The accurate definition of the large-scale cloud pattern is also no less important, and we are aware that the satellite is capable of doing that to a high degree of satisfaction.

Referring to Fujita's presentation of the analyses of cloud-top temperatures, the question may be posed if useful inference could be made regarding the general field of vertical motion over our area of interest in terms of these temperature profiles.

From the discussions so far presented it seems that at present greater use can be made of the satellite photographs to interpret the radar scope pictures. In my opinion the correlations obtained by Fujita between radar and Tiros photographs should be passed on to the radar operators and the operational forecasters, providing them with a better understanding of what the radar scope sees.

In conclusion I would like to say that, based on some preliminary studies, we have come to realize that at present the areal and frequency coverage of satellites is not adequate for any routine scheme of meso-analysis, but it can provide us with a lot of useful data for research purposes. Eventually the coming of the synchronous meteorological satellite will provide us a continuous observation platform to study the evolutions of storms.

Note: Mr. Sanders presented a case study relating radar, satellite and conventional meteorological data over an area around Oklahoma. For technical reasons the case study is not presented here.
SESSION IV

SYNOPTIC ANALYSIS
THE USE OF SATELLITE DATA IN WEATHER ANALYSIS

Vincent J. Oliver*
U. S. Weather Bureau, ESSA

Edward W. Ferguson**
U. S. Weather Bureau, ESSA

During the past ten years, our methods of weather analysis and prediction have undergone some rather fundamental changes. The most important of these has been due to the introduction of the high-speed computer into our analysis procedures. Ten years ago the first operational computer analyses and forecasts were made available to the practicing forecaster. His reaction was, "Now we have another toy of the theoreticians. I wonder if they will ever learn to stop bothering us with their untested brainstorms?" It was about five years later that the field forecaster first began to systematically use some of the machine products and even now, ten years later, we are just beginning to realize that the computer is here to stay and that our tried and true hand methods of analysis and forecasting are not as good or as fast as machine methods.

We are now about to launch the first of a continuous series of operational weather satellites. The ten Tiros satellites and the one Nimbus satellite so far put into orbit have all been designed for research. Some of the operational usefulness of the satellites, such as locating tropical storms (Timchalk et al., 1965; Fett, 1964) (Fig. 1), was recognized immediately and an operational group was organized to detect these storms and distribute messages describing their location and size to those

*Speaker
**Co-author.
services responsible for warning the public of severe weather. Neph-
analyses have also been prepared operationally and distributed worldwide
to make some of the information obtained by these research satellites
available. However, since these were all research satellites, the area
covered, the irregular time of distribution and the form of the mes-
sages could not be adjusted to meet many of the operational needs of
meteorologists.

The operational meteorological satellites which are soon to be
launched will, for the first time, observe the entire world once daily
and centralized machine processing of the data will make possible the
timely issuance of products designed to suit the operational user, as
well as the researcher.

I will discuss here some of the uses we have in mind. The most ob-
vious are locations of fronts and storm centers (Widger, 1964; Wiegman,
et al., 1964) (Fig. 2). Over ocean areas the cloud patterns associated
with cold fronts, warm fronts and occluded fronts are unique enough in
appearance to enable a trained analyst to pick them out and locate them
with an accuracy of a degree or two of latitude. The appearance of storm
centers also is unique enough to permit this same degree of accuracy.

At present, both the computer analyses and hand analyses are as
accurate as this where there is sufficient data. However, over much of
the ocean areas, data is too sparse for this degree of accuracy. In
these areas the satellite data, if used properly, should make possible
more accurate machine analyses as well as more accurate hand analyses.
This improvement in analysis will automatically mean improved forecasts.

Although our first and most obvious use of satellite pictures was
to locate storm centers and fronts, a more careful study of the cloud
pictures has revealed patterns that may turn out to be much more useful
to us as analysts than these. I refer now to the cloud patterns as-
associated with secondary vorticity centers (Oliver), jetstreams (Oliver,
et al., 1964) (Fig. 3), and midtropospheric trough and ridge lines
These may be more important primarily because they are more directly the circulation features used by the computer, and secondarily because these features are more frequently missed entirely or poorly located both by computer techniques and by hand analysis. The remainder of this discussion will be about what to me is our most spectacular discovery -- the secondary vorticity center.

Ever since the classical frontal model for storms was first tried, analysts found that on about 30% of the storms, weather patterns occurred to the rear of storm centers and to the rear of major fronts. This did not fit the classical frontal model. Our first attempt to account for this post-frontal organized weather was by means of a "bent back occlusion." It was supposed that as the storm center re-formed near the junction of the cold and warm fronts the older part of the occlusion was swept southward to the rear of the new storm center and appeared on our weather charts as an elongated area of bad weather to the rear of the main storm. This explanation seemed quite plausible and was strongly advocated by ocean analysts during the period when maps were drawn at 24-hr intervals. Later, when we began to draw surface charts at 12-hr and then 6-hr intervals it became obvious that bent back occlusions were not the answer.

The next solution was the "secondary cold front." A new surge of cold air or a post-frontal zone of convergence so aligned that it would produce a new front was thought to be the mechanism for producing these secondary cold fronts. Many meteorologists still use this explanation for post-frontal bands of weather. However, when use of upper-air charts and thickness charts became widespread it became evident that many of these so-called secondary cold fronts were directly along the upper cold trough line and in the center of the thickness cold dome -- a very poor place for a front.

Satellite TV pictures frequently reveal a concentrated area of convective cloudiness which appears to be directly under the secondary area of vorticity frequently found to the rear of the major storm center. The
vorticity center cloudiness over oceans is primarily convective in origin, but in a rapidly moving system the tops of the convective clouds merge to form a continuous band of middle or high altostratus. These convective clouds form a somewhat circular pattern, but when widespread merging occurs, the pattern becomes more comma-shaped, with the wide part concentrated in advance of the center of curvature of the comma and the cloudiness tapering to a point at the rear end of the comma.

One of the most remarkable features of the vorticity center cloud pattern is the clear area separating the frontal clouds from the vorticity center clouds. Nearly cloudless skies are very rare over oceanic areas—most of the large clear areas which occur in mid-latitudes are observed in an elongated pattern to the rear of those cold fronts which are followed by an active moving secondary vorticity center. We see an example of the cold front, the clear area, the vorticity center comma-shaped cloud band and the isolated clusters of convective cloudiness in Fig. 5 A and the accompanying weather maps in Fig. 5 B. Several examples of how centers of positive vorticity appear in satellite photographs are shown in Figs. 6 through 11.

S. Pettersson and the forecasters at the National Meteorological Center have demonstrated rather clearly during the past ten years that a large majority of the cases of new cyclogenesis and wave formation occur on a front only in those places and at those times when an area of positive vorticity advection is overtaking a portion of the front. The vorticity advection area is concentrated in the portion of the satellite picture covered by the comma-shaped cloud band. Therefore, the early detection of those vorticity centers and the advection accompanying them should make a very important contribution to our forecasting ability—by more accurately locating areas of vorticity maxima, and also by making more accurate the prediction of cyclogenesis in those regions where the computer advepts this vorticity into a frontal zone.

In conclusion, although we have now had five years of satellite observations, these have been R & D satellites and have had only intermittent
effects on analysis and forecasting. The operational satellites to be launched in the winter and spring of 1966 will make possible complete daily coverage of the entire sunlit portion of the earth. All types of synoptic-scale storms can be clearly seen and accurately located, but in my opinion the secondary vorticity center cloudiness will be the most useful of all of the observations in improving our middle latitude forecasting ability.

REFERENCES


Oliver, V. J.: "The Use of Satellite Pictures in Weather Analysis and Forecasting" (tentative title), WMO Technical Note, in preparation, National Environmental Satellite Center.


Fig. 1 Typhoon Ruth, southeast of Japan. Dashed line encloses the major overcast area. Concentric cloud bands and the eye of the typhoon are visible. (0351 GMT, 18 August 1962, Tiros V, Pass 0855T)
Fig. 2 Cloud pattern associated with a mid-latitude storm system showing the storm center, A, and the cold frontal band, C. (Atlantic Ocean, 1707 GMT, 16 February 1965, Tiros IX, Pass 307T)
Fig. 3 Cirriform cloud formation associated with the jet stream. The anticyclonically curved bands, as at A, which are oriented normal to the upper level flow are frequently seen in jet associated cloud masses. (Western Mexico, 1919 GMT, 7 January 1964, Tiros VII, Pass 2992T)
Fig. 4 Position of a cloud mass, A, relative to the 500-mb flow, dashed lines. The western edge is located along the 500-mb trough and the eastern edge coincides with the position of the ridge line. (North Atlantic, 1641 GMT, 6 February 1965, Tiros IX, Pass 186T)
Fig. 5 A. Comma-shaped cloud mass, A, associated with a vorticity maximum, H, located to the rear of a cold front, E-G-B-F; the clear area, K, separates the frontal clouds from the vorticity center clouds. Isolated clusters of convective type clouds, as at M, appear in the cold, unstable air. (North Pacific, 2155 GMT, 25 February 1965, Tiros IX, Pass 418T)
Fig. 5 B. Surface and 500-mb analyses for the same time as the satellite picture shown in Fig. 5 A. Shaded areas correspond to significant cloud areas in the picture. Dashed lines are 500-mb contour lines labeled in tens of meters. Continuous lines are 1000-mb contour lines labeled in tens of meters.
Fig. 6 The well developed comma-shaped cloud mass, A, is in a region of positive vorticity advection in advance of a center of positive vorticity at B. (Pacific Ocean, 2000 GMT, 25 February 1964, Tiros VIII, Pass 0964T)
Fig. 7  The mirror image of a southern hemisphere picture. A well defined comma-shaped cloud mass, A, behind a frontal system in an area of positive vorticity advection. The major storm center can be seen at B. (South Pacific Ocean, 2228 GMT, 8 February 1965, Tiros IX, Pass 203)
Fig. 8  The cloud mass, A (outlined for clarity), is the comma-shaped cloud pattern associated with a positive vorticity center, B. (North Pacific, 1934 GMT, 4 November 1963, Tiros VII, Pass 2047T)
Fig. 9 The comma-shaped cloud pattern, A, represents the area of positive vorticity advection in advance of a vorticity center at B. (North Pacific, 2348 GMT, 15 January 1964, Tiros VII, Pass 3113T)
Fig. 10 The concentration of convective type clouds at A forms a comma-shaped pattern in advance of a vorticity center at B. (Eastern Pacific, 2015 GMT, 14 April 1963, Tiros VI, Pass 3041D)
Fig. 11 An area of positive vorticity advection, A, behind a cold front, C. The comma-shaped pattern is associated with a center of positive vorticity at B. (Central Atlantic, 1716 GMT, 20 February 1963, Tiros V, Pass 3530T)
LARGE-SCALE CLOUD SYSTEMS

Frederick Sanders*
Massachusetts Institute of Technology

We have only recently entered into satellite work and consequently I have only nine diagrams to show. Under a contract with the National Weather Satellite Center, the project has as its aims to obtain an insight into the production and maintenance of large-scale cloud systems, to enhance the diagnostic value of Tiros cloud photography by calculation of large-scale vertical motions, and to compare some of the results with the cloud photographs.

Two important and not unrelated questions to which attention is directed here are: (1) what is the relative importance of the roles of horizontal and vertical motion in accounting for the observed configuration of cloud systems, and (2) what is the degree to which the presence or absence of cloud can be taken as an indication of local ascent or subsidence. In case of positive results we may perhaps go from cloud picture to vertical motion and from vertical motion to horizontal motion which is the primary input to numerical weather prediction.

The main avenue of approach to these questions is a study of the relationships between calculated fields of large-scale vertical motion and cloud patterns observed by satellite for selected cases in middle latitudes. The criteria for selection of cases were the clear-cut synoptic characters of both continental and maritime environments subject to limitations imposed by availability of both upper-level soundings

*Leading Discussant
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data and Tiros observations. The former criterion was aimed at providing generality of application of results, while the latter was designed to obtain an appraisal of the effects of limited availability of water vapor over continents.

A suitable equation for calculating the vertical motion is obtained by combining hydrodynamic and thermodynamic equations for large-scale quasi-geostrophic flow so as to eliminate local time-derivatives. An appropriate form of the vorticity equation in the \((x,y,p,t)\)-system is

\[
\nabla^2 \frac{\partial \varphi}{\partial t} = - \int \nabla \cdot \nabla \eta + \int \frac{\partial \omega}{\partial p}
\]

(1)

where the symbols have the customary meanings. An appropriate form of the thermodynamic energy equation is

\[
\frac{\partial}{\partial p} \left( \frac{\partial \Phi}{\partial t} \right) = - \nabla \cdot \nabla \varphi - \sigma \omega + \mathcal{H}
\]

(2)

where \(\sigma = \frac{\partial \Phi}{\partial p} \frac{\partial \rho}{\partial p} (\ln \Theta)\) is a measure of the static stability and is taken to be a function of pressure, and \(\mathcal{H} = \frac{R \mathcal{C}_p \rho}{d \mathcal{C}_p / dt}\) represents the effect of diabatic heating.

Combining Eqs. (1) and (2) will eliminate the local time derivative and the familiar "omega" equation results

\[
\left[ \nabla^2 + \frac{\sigma}{\sigma} \frac{\partial^2}{\partial p^2} \right] \omega = - \frac{\sigma}{\sigma} \frac{\partial}{\partial p} \left( - \nabla \cdot \nabla \eta \right) - \frac{R}{\sigma} \left( \nabla^2 \left( \nabla \cdot \nabla \eta \right) \right) - \frac{1}{\sigma} \nabla^2 \mathcal{H}
\]

(3)
Here it suffices to note that the left side of Eq. (3) is negatively correlated with \( \omega \) itself. From examination of the right-hand side it is found that ascent (subsidence) tends to occur in regions of upward increase of cyclonic (anticyclonic) vorticity advection, near maxima of warm (cold) temperature advection, and near maxima of diabatic heating (cooling).

As boundary conditions, \( \omega \) is assumed to vanish at the top of the atmosphere and also at the lateral boundaries of the volume over which the calculations are performed. At the 1000-mb surface, which is taken to represent the base of the atmosphere, it is assumed that

\[
\omega_{10} = \rho_{10} \left( \frac{\partial \varphi}{\partial z} \right)_{10} - \psi_0 \nabla \cdot \mathbf{\varphi} - g K S_{10} \cdots
\]

Here \( \varphi \) represents the geopotential at the surface of the earth and \( \psi_0 \) and \( S_{10} \) the geostrophic wind and its relative vorticity at the 1000-mb pressure surface. The contribution from the first term on the right is relatively small. The second term is due to the orographic effects and the third term expresses the contribution to the vertical motion due to frictional divergence within the layer. The constant \( K \) was chosen so that a 1000-mb geostrophic relative vorticity of \( 10^{-4} \) \( \text{sec}^{-1} \) generates a frictional vertical velocity of 2 cm sec\(^{-1}\).

The input data are values of ground elevations obtained from Berkofsky and Bertoni (1955), and the contour heights of the mandatory pressure surface (excluding 200 and 100 mb) up to 50 mb. The rectangular grid contained 32 by 24 points with a mesh length of 165 km in middle latitudes. The stability factor \( \sigma \) was obtained from an estimated sounding representing a horizontal average over the grid area. In this way \( \sigma \) is a function of pressure only. Diabatic heating and cooling were ignored.

Some remarks regarding the interpretation of the results are not
out of place. According to Phillips (1963) the geostrophic theory on which the $\omega$ equation is based is most appropriate for synoptic-scale circulations of hemispheric wave number four to eight. The small mesh length used in these computations permits vertical motion patterns more than an order of magnitude smaller in scale. Identifiable cloud elements observed on Tiros cloud photographs are at least two orders of magnitude smaller. We are therefore entitled to relate the calculated vertical motions only to the broader aspects of the observed cloud systems, which are usually adequately portrayed in the nephanalysis prepared from the original cloud picture. The quantitative accuracy of the details of the computed field of vertical motion must be viewed with reservation. Hopefully, in fact, the degree of success in explaining the observed cloud patterns by use of the vertical motions may be a measure of their accuracy. Lest this state of affairs should seem like the blind leading the blind, however, we offer the impression, based upon experience, that the geostrophic model yields qualitatively correct vertical motions even on scales considerably smaller than those for which the results can be rigorously defended a priori.

No attempt has been made in our calculations to incorporate the effect of the release of latent heat of condensation in the cloud systems, though Phillips (1963) suggests that it would be appropriate to do so within the framework of the geostrophic model provided the precipitation rate does not exceed about 0.03 in./hr. Qualitatively, it is apparent from the $\omega$ equation that the effect of this heating would be to increase the rate of ascent in the precipitation areas and perhaps shrink the size a little.

Regarding the method of analysis it may be said in summary that we simply did everything we could to insure that there were no glaring inconsistencies of hydrostatic or thermodynamic nature even over regions of sparse data.

Three cases were selected for study of which one will be described
here. This case occurred during the period 4 to 7 September 1962 over the United States and Southern Canada. It illustrates the formation and evolution of a spectacular long-lived cloud vortex, associated with an occluding though weak surface system. Its geographic location justified the preparation of a ten-level synoptic model computation.

The synoptic situation on 4 September at 1200 GMT is presented in Fig. 1. The dashed lines portray the surface pattern, the full lines the pattern at 300 mb. A leading edge of cold air is clearly shown to run from James Bay through the Great Lakes, the Texas Panhandle and back to the Rockies. A wave cyclone is shown to develop just north of Chicago. Figure 2 shows the Tiros V cloud picture close to the time of the synoptic map. It shows a major cloud band associated with the front. A slight oscillation on the west side of the band indicates the incipient wave disturbance. On the north side of the wave is an area of broken stratocumulus and altocumulus. To the west the skies are clear.

Figure 3 shows a composite of three nephanalyses obtained from Tiros V. Superimposed is the computed 700-mb vertical motion field in units of $10^{-4}$ mb sec$^{-1}$. Directing the attention to the westernmost one of the composite nephanalyses, it will be seen that qualitatively the vertical motions correspond well with the major cloud features. Maximum ascent is northeast of the surface wave position and, in general, there is ascent along a major part of the main cloud band. The area of clear skies is characterized by rather strong descent.

During the course of the next 48 hr the surface wave developed and moved to a point just southeast of James Bay (Fig. 4). The front has now passed down through Maine and off the coast of New England, south of which a secondary wave is forming. A pronounced vortex is shown at the 200-mb pressure surface. The contour interval of 12 decameters indicates this is a rather substantial closed vortex and more prominent at this level than at the surface. A large cold high at the surface
underlies a major part of the 300-mb trough over the United States. Figure 5 is the associated Tiros V cloud picture and Fig. 6 again presents a composite of nephanalyses with the vertical motion field superimposed. A striking feature is the noticeable intrusion of dry air into the eastern quadrant of the upper-level center.

The vertical motion field indicates well documented ascent in the vicinity of the cloud vortex and in a region extending southwestward and northeastward. The developing secondary south of the south coast of New England shows a separate ascent maximum. To the southwest of that forming center one may notice an interruption of the ascent band by a region in descent. Perusal of the original cloud photograph seems to indicate a thinning out of the frontal cloud band over the same region.

The subsidence maximum occurs just northeast of the surface high and tends by and large to correspond to a clear region. There is even a sort of an indication perhaps for the computed downward vertical velocities to show the northeastward intrusion of the dry air, although it looks as if the leading edge of the dry air intrusion penetrates the region of ascent. This must be attributed to the horizontal advection of dry air.

Twenty-four hours later on 7 September at 1200 GMT (Fig. 7) a pronounced 300-mb cut-off low is situated just southeast of the Hudson Bay region. At the surface, the secondary low off the New England coast has moved eastwards. An extensive surface high pressure system is still located below the 300-mb trough line.

This time the satellite photographs cover the whole region to just northeast of the 300-mb low. Figure 8 shows the classic dry air intrusion behind the cold front and around the vortex. The main cloud mass is centered around the vortex. The nephanalysis and the superimposed computed vertical velocities are given in Fig. 9. On the whole the vertical velocity field is rather complex. This may be due to the
way in which the vorticity advection in the forcing function was handled on the boundary. In the near future it is hoped to extend the grid and repeat the computations in order to see if the complex features persist.

Figure 9 clearly shows a region of descending motion wrapping itself around the north and northwest side of the vortex. This area is almost free of cloud. The descending motion is associated with cold air advection. As in the previous case, it is again noted that the tongue of clear air penetrates the area of ascending motion. The main area of ascending motion near the vortex center shows a rather spectacular agreement with the region of extreme cloudiness, although again it must be noted that some of the cloud mass associated with the vortex extends over the region of descent. This cloud, however, did have a recent history of upward motion.

I wish to emphasize that the case study just described was the best in showing the relationship between upward motion and cloudiness. I do not want to suggest that the presence of a cloud vortex enables one to deduce that there is an ascent maximum near it. In another investigation there was actual descending motion in the cloud center. Even manipulation of the original analysis failed to change the sign of the vertical motion. However, in this particular case it appeared that the vortex was in the process of being swept up by an approaching trough, justifying slight descent in the vortex center.

Based on the foregoing case, and two others not described here, one may venture some conclusions. It is found that both horizontal and vertical motions are important factors in determining the configuration of large-scale cloud systems. Indeed, when these factors are both considered, the evolution of cloud structure is successfully accounted for. There is some suggestion that the coincidence between ascent and presence of cloud is best in the earliest phase of storm evolution after sufficient moisture has become available, but before rotary circulation has developed. After closed cyclonic circulation
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has developed, clear dry air flowing to the eastern quadrant of the storm encroaches upon ascent, while moist cloud-bearing air circulates around the top of the storm and intrudes upon descent in the western quadrants. Thus in middle latitudes the large-scale vertical motion cannot generally be deduced with confidence from the cloud pictures.

In this respect no differences were observed between continental and maritime situations. In the latter case dry air intrusions extended into ascent regions even after long over-water trajectories. Nothing can be said about generally dry continental storms. Lack of interesting Tiros pictures would have precluded their consideration in this study. Of course, they offer little possibility for diagnostic use of Tiros data.

The cloud vortex per se is seen under two rather different motion environments, one which I showed you in detail, the other to which I referred in passing, being picked up by a trough. The vortex is not seen in the case where the large-scale motions dominate. The situation which favors a clear-cut long-lived vortex is one in which an extensive cut-off cyclone remains relatively isolated from the rest of the general circulation for a period of days. The diagnostic significance of the Tiros vortex per se seems limited.

On the positive side we have gained some understanding of the dynamics of large-scale cloud systems. It does not seem necessary to invoke the release of latent heat of condensation in the calculation of vertical motions sufficient to explain them.

Finally we are left with a feeling of confidence in the application of quasi-geostrophic theory, at least qualitatively, to scales smaller than those which can be defended as suitable a priori.
REFERENCES


Fig. 1. Synoptic chart 4 September 1962. 1200 GMT. Dashed lines 1000-mb and full lines 300-mb contours.
Fig. 2. Tiros V orbit 1105 D, frame 3. 1430 GMT for 4 September 1962.
Fig. 3. Tiros V nephanalysis composites 4 September 1962 and superimposed computed 700-mb vertical velocities. Units 10^{-4} cm sec^{-1}. 
Fig. 4. Same as Fig. 1 but for 6 September 1962, 1200 GMT.
Fig. 5. Tiros V orbit 1133 D, frame 10. 6 September 1962, 1325 GMT.
Fig. 6. Same as Fig. 3 but for 6 September 1962.
Fig. 7. Same as Fig. 1 for 7 September 1962, 1200 GMT.
Fig. 8. Tiros V orbit 1147 D, frame 15, 1250 GMT, 7 September 1962.
Fig. 9. Same as Fig. 3 for 7 September 1962.
SYNOPTIC APPLICATIONS OF SATELLITE-BORNE INFRARED WINDOW MEASUREMENTS

Guenther Warnecke*
Institute for Meteorology and Geophysics,
Free University of Berlin

Some brief comments are presented on the potentiality of satellite radiation data for synoptic analysis.

Figure 1 shows the 8 to 12 μm band equivalent blackbody temperature analysis over the United States for 16 July 1961 as obtained from the Tiros III satellite. The analysis was based on a computer print-out. Many interesting features may be noted, but attention is especially directed to the elongated zone of very low equivalent blackbody temperatures (T_{BB}) over the southern and eastern parts of the United States. Figure 2 provides the corresponding synoptic nephanalysis based on conventional cloud observations. A very good correspondence is recognized between the low temperature belt and the observed cloud. The zone of low T_{BB} values represents the cloud mass associated with a cold front. Also it can be observed that the cloud mass over Lake Michigan, Wisconsin, and Minnesota does not show a similar response in the pattern of radiation data. This is because the cloud observed was generally of the low stratus type and hence was associated with warmer temperatures. The frontal cloud, on the other hand, penetrated deep into the higher layers of the troposphere and consequently exhibited cold cloud tops. Therefore the radiation data, in combination with surface cloud observations or satellite TV pictures, can help to identify the vertical extent of the observed cloud patterns.

* Discussant
An attempt was also made to verify the window radiation data in a vertical cross section from Cuba to British Columbia, cutting almost at right angles through the frontal system and heavy cloud area (shown in Fig. 1). This cross section is depicted in Fig. 3, on which the isotherms and fronts are presented in the conventional manner. The moisture and cloud analyses were performed using the radiosonde moisture measurements, as well as surface cloud observations from synoptic reports. The heavy black line marks the level where the air temperature equals the measured equivalent blackbody temperature ($T_{BB}$) along the cross section. On the whole, a good correspondence between the observed clouds and the radiation measurements is obtained. However, there are also some regions where the interpretation of the radiation measurements is difficult, and this indicates a disadvantage of the medium resolution radiometer (MRIR) for detailed synoptic analysis. If there are many different cloud layers, as for example in the Prince George–Edmonton regions, the MRIR sensor responds to radiation emissions mixed from various altitudes and integrated over a heterogeneous field of view. In such a case, it is not possible to interpret the measurements as representing a distinct radiating surface (cloud top or earth's surface) at all. However, if there are widely homogeneous cloud decks filling the field of view of the radiometer, as one finds approximately realized in the dense high-reaching cloud systems associated with the frontal bands, a very good correspondence is obtained between the cloud-top heights and the so-called radiation level.

From the above remarks and the existing literature it may be concluded that the 8 to 12 $\mu m$ radiation band will generally provide the gross pattern of cloudiness rather than details. Details may be derived where there are restricted areas of homogeneous dense cloud decks filling the field of view of the radiometer.

Another point to be emphasized is the potentiality of this type of information if it is available on a quasi-global (in the case of Tiros)
or a global scale (in the case of the Nimbus satellite). Particularly over areas where we do not have any weather information, one can at least infer the gross features of the horizontal and vertical cloud distributions and thus locate significant weather systems such as cyclones and fronts. Figure 4 shows the global distribution of all available synoptic surface weather observations for a single day (21 January 1964 at 1200 GMT). Needless to say, a period of several months was involved in collecting these data. Despite the effort required to collect such information, it will be noticed that large areas, especially in the southern hemisphere, are void of data, and a conventional synoptic analysis is impossible. It is just in such areas that satellite information is of extremely high value.

Figure 5 shows two quasi-global analyses of the 8 to 12 μm band of Tiros III and Tiros VII; the latter matches the date of Fig. 3. In the case of the satellite observations, the data coverage is very complete and is particularly useful over the southern hemisphere. The striking feature of both maps is the equatorial belt of high cloudiness associated with the intertropical convergence zone (ITC), but there are differences in the ITC structure in both these seasons. For example, in the northern hemisphere winter the ITC appears to be much better organized around the globe than during the northern hemisphere summer. From these pictures one can also derive that the southern hemisphere circulation does not change drastically in character between winter and summer with regard to the general distribution of cloudiness. The latter remarks are to be construed on the limited basis of the two maps available.

In the northern hemisphere large seasonal differences may be observed. On the winter map one finds heavy cloudiness over Siberia and northern Europe extending far into the middle latitudes; conversely, during the northern hemisphere summer only a few relatively small areas of heavy cloudiness are to be observed.
Another interesting feature to be mentioned is that in the particular case study of 21-22 January 1964 the mid-latitude frontal cloud zones in nearly all instances merge with the ITC. This is particularly noticeable for the frontal band crossing the southeast Indian Ocean and passing Australia into the ITC. A band of heavy cloudiness is also observed east of Australia, and others in the Central Pacific and south of Africa. In the latter case, the band extends into Brazil where it merges with the ITC. A second zone is indicated farther south. From this one may be inclined to infer the existence of quite different behavior in the two hemispheres of the zonal and meridional circulation systems and their relations with the ITC.

As a closing remark, it must again be stressed that satellite measurement of terrestrial infrared radiation has a high potential of information for synoptic purposes; once available on an operational basis, it should become an extremely useful and indispensable tool for synoptic meteorologists.
Fig. 1  Isopleths of equivalent blackbody temperature as derived from Tiros III, 8 to 12 μ radiometric measurements on 16 July, 1961 (°K) and conventional observations of cloudiness.
Fig. 2 Nephanalysis based on conventional synoptic observations on 16 July, 1961 (octa).
Fig. 3  Vertical cross section across the United States on 16 July, 1961 based on conventional radiosonde and cloud observations. Isotherms, thermal discontinuities are represented in the conventional manner; the moisture field is represented by dewpoint depression. Clouds are schematically indicated by dark grey shade.
Fig. 4  Distribution of synoptic surface observations on 21 January 1964, 1200 GMT.
Fig. 5-A  Quasi-global distribution of terrestrial and atmospheric emission within the 8 to 12 μm window spectral region as measured by Tiros VII on 21-22 January 1964.
Fig. 5-B  Quasi-global distribution of terrestrial and atmospheric emission within the 8 to 12 μ window spectral region as measured by Tiros III on 16 July, 1961 (T_{BB} in °K).
Mr. C. W. C. Rogers of the ARACON Geophysics Company presented a series of slides showing some of the work he and Mr. P. E. Sherr had been doing on television and infrared satellite data. In addition to indicating the usefulness of infrared pictures for synoptic purposes, Mr. Rogers also emphasized the value of the data in making physical interpretations of the clouds in terms of advection and vertical motions.

Dr. W. K. Widger, Jr., of the ARACON Geophysics Company presented some of the work done on cloud transport in non-convective situations. The results mainly seem to show that the direction of transport is essentially that of the 500-mb wind and that the speed is somewhat less than the 500-mb wind.

Some examples of studies exhibiting the use of HRIR data were also discussed.
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LOCAL CIRCULATIONS
This paper will emphasize three different types of local circulation patterns seen in satellite pictures: the so-called cellular pattern, the mountain wave pattern, and some interesting spiral cloud patterns seen in the lee of certain islands.

CELLULAR PATTERNS

Before considering some examples, it may be useful to discuss laboratory experiments and theoretical aspects of cellular patterns. It is well known that if a plate is heated carefully and slowly from below, a thin layer of air above the plate will produce Benard cells. These cells, when viewed from above, sometimes display hexagonal patterns, although it is not easy to achieve this geometrical shape most of the time. In such a case, the motion within the pattern is downward in the middle of the cell.

There has been some discussion about why this motion in the center is downward rather than upward; for example, with most liquids, the motion happens to be upward in the center of the cell rather than downward. And the reason that has been advanced for this is somewhat as follows (Graham, 1933). The Rayleigh number is proportional to the reciprocal of viscosity, if we keep the temperature gradient, the thickness of the layer, etc., constant. In air, viscosity increases as the temperature increases, so that if we have a flat plate which is hot at the bottom, the temperature will be high near the plate, which means that we have a high viscosity. Farther from the plate the temperature will be
lower and accordingly, the Rayleigh number will be higher. In Bénard cell theory there is a critical Rayleigh number, and if this value is exceeded, motion begins. The place in the fluid where the high Rayleigh number first appears would be the place where the instability would occur first. Presumably, then, for air heated from below, the instability would occur first near the top of the fluid. According to Graham's view the motion would start away from the boundary where the instability first occurs, and the cell would form in such a way that the motion would be down in the middle. In liquids, the viscosity generally decreases with increasing temperature; therefore the motions would be reversed; i.e., upward in the middle.

Moreover, there are liquids in which the viscosity decreases with increasing temperature below a certain temperature; and then the viscosity increases with further increase in temperature. Experiments have been performed (Tippelskirsch, 1956) which verify that when we are in the regime of increasing viscosity with temperature the motion is downward in the middle; when the liquid changes over to the opposite regime the motion starts upward in the centers of the cells.

So in air, if we have laminar type motions, we may expect downward motion in the middle. But, of course, in the atmosphere we generally have a much more turbulent motion, so that molecular viscosities or heat conductions cannot be used. Therefore, the concept of the eddy viscosity, which is not fully understood, is used. If the reasoning cited above for laminar flow is applied for turbulent flow, we can get cells with the motion downward in the middle, for we find that the eddy coefficient of viscosity in the region above about 200 m decreases with height (Haurwitz, 1941, p. 212). Thus Rayleigh type instability would occur near the top of air layer, i.e., near the base of an inversion, especially when heating from below occurs. In that case then the motion would be downward in the middle of the cell. At any rate, we do find cloud patterns in satellite pictures that have a ring of clouds with a clear area in the middle, suggesting that we have a downward motion in
the middle of the cell and upward motion around the edges where the cloud occurs. These we have observed on many occasions when there is a fairly intense heating from below, as for example during a flow of cold air from the north.

On the other hand, we sometimes see a pattern where the cloud seems to be in the middle, and with a clearer area around the cloud; however, this pattern is seen much less frequently over large areas. In this latter case, it has been suggested (Hubert and Krueger, 1962) that there would be a different distribution with height of the eddy coefficient. It may be a cloud layer, which has instability created near the top rather than near the bottom, e.g., by radiative cooling of the top of an extensive cloud area. Perhaps in this case the largest coefficients of eddy viscosity would be near the cloud top and consequently these eddy coefficients would increase with height in the cloud layer. The greatest Rayleigh instability would occur near the bottom of the layer according to the argument presented earlier. The motion would be upward in the middle of the cell and downward around the edges in agreement with the appearance of the cloud in the middle of the cell.

Another point should be mentioned regarding laboratory results. In the laboratory the size of the cells is such that the width tends to be three times the height; but in the atmosphere, if we consider the height as being determined by the height of an inversion, provided one exists, then the width/height ratio is much larger, and there have been discussions about the reason for that. But before discussing those, it may be useful to look at a few of the pictures of cloud patterns observed by the various satellites.

Figure 1 is a picture taken from Graham's (1933) article in which he shows the kind of pattern obtained in the laboratory. One can get a hexagonal pattern, with convergence into the middle near the top; and the motion is downward in the middle, as in the case of the air type of motion. In Fig. 2, also taken from his article and which shows one
of the better formed patterns, the type of pattern which is sometimes achieved in the laboratories may be seen. The dark spots in the middle of the individual patterns supposedly indicate the downward motion. Concerning the scale of one cell, it may be mentioned that the depth of the fluid was something like 7 mm and if we take the width 3 times that, something like 20 mm is obtained.

Figure 3 is an early picture taken from Tiros I. The left one is a wide-angle picture; in the area of the cells in the center and bottom part of the picture the cellular patterns are apparent. The right-hand picture is a narrow-angle one taken in the middle of the first picture. The dark area in the middle is due to a camera defect. Often the patterns are more irregular, but still the pattern suggests an array of cells with holes in the middle.

Explanations have been advanced for the difference in size that one finds in the atmosphere by comparison with the size in the laboratory. The argument that was advanced by Priestley (1962) was rather simple. Priestley's argument is as follows: in an inversion with convection below, the clouds penetrate slightly through the inversion. If a cellular motion is formed, the eddy coefficients would not vary much in the horizontal direction. However, the transfer across the inversion would restrict the vertical transfer so that the eddy coefficients, which are involved in the transferring of heat and momentum, would have a lower value in the vertical than they would in the horizontal. He showed that if we have a horizontal coefficient which is 100 times greater than the vertical coefficient, and if one put that into the standard equations, this would give a ten-fold increase in the ratio of width to height. Thus in the atmosphere the ratio could be 30 to 1 by comparison with the 3 to 1 in the laboratory. Ray and Scorer (1963) also allowed the coefficients to vary with position in the cell and found that certain distributions of coefficients could explain a 30 to 1 ratio of width to height. Thus the variation of eddy coefficients in the cells serves as one explanation of the fact that the atmospheric cells seem to be flat.
In Fig. 4, we see an example of the cloud patterns with a hole in the middle, near the top. At the bottom left the pattern is quite different, perhaps suggesting a pattern with the cloud in the middle. In the center of the figure is a star-like pattern which may be a transition between the other two types. In the top of the picture there may be heating from below; whereas in the bottom part of the picture, cooling at the top may predominate. However, this is still only speculation in this case. Sasaki (1965) has also found that the relationship between the horizontal and vertical eddy coefficients is important in determining the cell size. Among the parameters he considers, heat transfer from the surface is also important.

It may furthermore be mentioned that the diameter of the cellular phenomena as shown on the Tiros pictures ranges from about 50 mi for the largest to about 5 to 10 mi for the smaller ones. Most of the patterns are found over the oceans but in rare cases may also be found over land.

Now let us introduce a shear. In the laboratory, according to Graham, you go from pattern to pattern as shown in Fig. 5. According to him, this depends to a certain extent upon the shape of the hexagon. Cells are obtained which are still more or less circular or hexagonal but become distorted in the direction of the shear. If the shear becomes strong enough, we get lines parallel to the wind flow. However, with very low wind shear, they have also been able to get lines which are perpendicular to the wind shear.

Figure 6 is a picture which we have studied rather intensively, and we know something about the temperature and wind structure in this case (Krueger and Fritz, 1961). The wide-angle picture, in the lower part of the figure, has some pronounced arcs. The narrow-angle picture in the upper left shows the area corresponding to the square shown on the wide-angle picture. From the narrow-angle picture, with its higher resolution, it is evident that the arcs are actually made up of individ-
ual convective cloud elements. If the wind soundings at Bermuda and ship "E" are considered, it is possible to relate the bowing-out of the cells to the direction of the shear, although the effect is not pronounced.

Figure 7 shows the position where the cellular clouds in Fig. 6 had occurred (i.e., within the dashed-line area). We note that they occurred in an anticyclonic region, where there is some heating from below. Sasaki (1962) has considered the effect of the heating gradient, in this case on the cell size. Probably both the heating gradient and the distribution of the eddy coefficients are important in the final explanation.

In this region we also had, luckily, many soundings. We see in Fig. 8 that there are pronounced inversions throughout the area and we can reasonably say that the height of the top of the cloud system was the height of the inversion. The width of the cells could be obtained from measurements of the diameters on the pictures. And the ratio of width to height could therefore be obtained.

In Fig. 9 the dots show the width and pressure at the inversion which we obtained from the last few figures. But we had a few other cases near Madeira Island in which we were able to get cell size. These are indicated by the lines labeled A and B. The cells were much smaller, averaging about 10 mi in diameter. A line was drawn through all the data by eye; this shows a ratio of about 30 to 1 between the width and the height.

These are all anticyclonic cases; that is, when there is anticyclonic flow with heating from below and perhaps there is a relationship between the height of the inversion (i.e., the depth of the cell) and the horizontal diameter as shown in Fig. 9. Other investigators who have studied these phenomena in Europe and elsewhere, have found that when you have cyclonic flow this may not work at all. Then there are also patterns that look like cellular patterns, but one cannot always find any
pronounced inversion and the reasoning cited above may, therefore, not hold. Moreover, the cyclonic situation is a very frequent occurrence.

Figure 10 shows a cyclonic pattern. There is strong suggestion of flow from the north, with heating from below, and we find a broken cumulus type of cloud field. This type of pattern, with some closed cells in it and many cloud arcs with lots of clear spaces, appears frequently behind cold fronts and in the flow of cold air over the warmer water.

There may be an interesting point in relation to these things, and that is the following: consider the scale at which moisture and heat are transferred into the atmosphere from the ocean. Figures 9 and 10 suggest that this transfer occurs on the scale of about 5 to 50 mi in cold air which is being heated by the underlying warm ocean. Thus, with the aid of cloud pictures, it may be possible to select those geographic areas in which sufficient turbulent heat is being transferred to the atmosphere to create the cloud patterns just described.

MOUNTAIN WAVES

Next I would like to discuss the question of mountain waves, which are also on a scale of about 10 mi, and which are discernible in the satellite pictures. If we refer to some of the theory and empiricism, one finds that Scorer's parameter is fairly prominent: \( F(z) \), equal to \( \left( \frac{gS}{U^2} \right) \), where \( S \) is the stability, \( U \) is the horizontal wind, and \( g \) is gravity. Scorer (see Alaka, 1960) found that this parameter has to decrease in height for mountain waves to appear. Thus, if we have a rapid increase of horizontal wind with height the criterion would be satisfied. But stability would also be a factor: if an inversion is present the stability is high at the inversion, and above that the stability will decrease, i.e., the decrease of stability above an inversion also helps to give a decrease in Scorer's parameter. Actually, mountain waves are found mostly in areas where an inversion exists not too far from the mountain top that is producing the waves.
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Scorer's parameter is related to the wave length, \( \lambda \), and theoreticians have determined that a good value for \( \lambda \) could be determined from the mean value of \( U \) and \( S \), averaged over the troposphere after certain simplifications. The relationship (Fritz, 1965) would be

\[
\frac{aL}{G} = \left( \frac{S}{G} \right)^{1/2} \lambda
\]

where \( G \) is a constant. Thus it is possible to determine the wind from the wave length, if the stability is known. Corby (1957) in England, using radiosondes, determined the average wind speed in the troposphere empirically and found a linear relationship with the lee wave length. To justify the linear relation, one must assume that the stability is more or less constant.

Figure 11 is a satellite picture taken over the Appalachians. The east coast of the United States can be seen in the picture, and Washington (W), Pittsburgh (P), and Huntington (H) are identified. Some of the wind and temperature data are shown on the figure. Note that the wind increases rapidly with height and an inversion is present, too. The wind is blowing perpendicular to the mountain, and gives rise to this wave pattern with a wave length of 13 mi. We can, moreover, examine the wind speed distribution and the stability as it determines the wave length and see if it agrees or disagrees with Corby's results. (Several additional examples of satellite lee wave cloud patterns were shown (see Fritz, 1965)). Figure 12 shows a summary of the relation of the mean wind speed to the wave length. The dots are Corby's observations obtained with balloons, and a linear relationship seems to be present. This figure also shows, with other symbols, mean wind speeds for several other cases. In most cases the results agree rather well with Corby's results, but the Washington case did not. On investigating stability versus height, it turned out that the mean stability for three cases were very close to each other, but for Washington, D. C. and Huntington, West Virginia, the stability was substantially lower than the others. If the stability is lower, for a given wave length, a lower wind speed can be expected and this actually was found to be the case. When the numbers for stability were inserted a better agreement for the observed
wind was obtained. However, uncertainties still remain.

In Fig. 13, four cases of waves in various parts of the world have been assembled. The one in the Middle East begins near the coast of Lebanon and Israel which is on the left-hand side of that picture. The people at the Hebrew University in Jerusalem are studying this particular case and the winds are much too low to agree with Corby's results, even if they take the stability into account. I hope they will find the explanation. If the radar wind measurements are correct, then something else will have to be found to explain the discrepancy.

Other factors may also need to be considered such as, for example, the mountain itself. According to theory, the mountain details, if you get far enough away from it, are not important. But at the Pennsylvania State University, perhaps for a smaller scale than this, they seem to think that the individual ridges may determine the wave length. So there are questions of interaction between the real, complicated mountain terrain with the wind. Of course, in Corby's results one obtains a variation of wave length with the wind speed, and I think all his cases were with the same mountain; so terrain details of the mountain cannot be the dominant factor. The newer theories being developed will eventually have to be consistent with the available observations, some of which come from satellite pictures.

SPIRAL CLOUD PATTERNS IN THE LEE OF ISLANDS

There are several explanations of the spiral patterns that we find in the lee of certain islands. These look very much like von Kármán vortices. If air blows past an obstacle, and if the flow is essentially horizontal, after the Reynolds number gets high enough, eddies form in the lee of the obstacle. The eddies are said to be shed by the obstacle and to move downstream.

In Fig. 14, which is from a paper by Chopra and Hubert (1965), the mean wind blows past an island whose diameter is "d." Eddies are shed by the island; the spacing between the centers of the eddies on opposite
sides of the island is "h," and the distance between the centers of eddies on one side is "a." An example of such eddies, as seen from Tiros, is shown in Fig. 15. These are seen in the form of cloud spirals, and are presumably formed by flow past Madeira Island. In Fig. 16, the picture is shown together with a sketch. The distances "h" were measured, or at least estimated, as shown. An inversion was located below 3000 ft, so that the mountains protruded well above the inversion, suppressing vertical displacements. However, some vertical motions were evidently present, judging by the cloudy and clear areas.

One can now compare various quantities as observed in the laboratory with the measurements in this particular case, and Table I does that.

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<th>Laboratory</th>
<th>Atmosphere</th>
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<tr>
<td>h/a</td>
<td>0.28 to 0.52</td>
<td>0.43</td>
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<tr>
<td>h/d ( \approx (C_d) )</td>
<td>cylinder, 1.25</td>
<td>1.8 to 2.1</td>
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<td></td>
<td>rectangle, 2.0</td>
<td>(depending on value of &quot;d&quot; chosen)</td>
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<tr>
<td>( U_e/U_o )</td>
<td>0.85</td>
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</table>

Chopra and Hubert could measure h/a. They could measure h/d, which is approximately equal to the drag coefficient. They have a quantity \( U_e/U_o \), where \( U_o \) = 10 knots was the mean wind, upwind from the island, and \( U_e \) is supposed to be the speed of the eddies, once they are formed in the lee of the islands. They determined \( U_e \) from an equation involving h/a. They also could calculate the period at which these eddies are formed in the lee of the island. They estimated that one was formed ev-
ery seven hours. They also were able to use another parameter (Lin's) measured in the laboratory to estimate the eddy coefficient of viscosity and got about $10^7$, which agrees more or less with other estimates for the atmosphere. Therefore, because the values in the table do agree with what is found in the laboratory for von Kármán vortices, the spirals observed from Tiros are probably also von Kármán vortices. However, in an earlier study, Hubert and Krueger (1962) attributed much significance to surface friction and its influence on the vorticity whereas Chopra and Hubert neglect surface friction. So that a complete theory would doubtless have to take both of those into account; that is, the depletion of vorticity due to the vertical eddy coefficient, and also the spreading of the vorticity due to the horizontal eddy diffusion. Additional examples of Tiros views of spirals and of a cycloidal cloud pattern in the lee of Madeira Island were shown; see Hubert and Krueger (1962), Fritz (1963).

So far we have been assuming that the wind spirals around also, either parallel to the cloud lines, or at least that the wind pattern has a spiral shape, even if it were displaced from the cloud lines. There are, however, other ways to produce cloud patterns like that, and a computational experiment was performed by Hama (1962). He assumed a sinusoidal mathematical flow. A shear was assumed perpendicular to the sinusoidal flow pattern and from that the streaklines were computed, as in Fig. 17. A streakline is produced as follows: given a fluid and a source in the fluid such as a point where dye may be emitted continuously. At some later instant in time, a picture is taken of all those particles. A pattern will be observed. It may not have anything to do with the particular streamline, the particular motion at that instant, or even with the trajectory; it is just the pattern that one observes. Of course, that is almost what Tiros did: it looked down and saw a picture. And one can produce a spiral cloud pattern, without any spiral wind motion. However, if the cloud spirals were streaklines, there would be some point sources for the cloud elements. One can speculate
about such point sources, but they may be difficult to locate, if they exist.

REFERENCES


Fig. 1. Schematic representation of the cellular pattern (after Graham 1933).
Fig. 2. Hexagonal pattern as obtained in a laboratory experiment (Graham).
Fig. 3. Tiros photograph, wide-angle picture on left, narrow-angle on the right.
Fig. 4. Cloud patterns as revealed by satellite photographs.
Fig. 5. Schematic representation of cloud patterns and their movements in wind-shear zones.
Fig. 6. Anti-cyclonic convective cloud elements as revealed by Tiros.
Fig. 7. Surface pressure map relevant to Fig. 6.
Fig. 8. Dropsonde and radiosonde data associated with Fig. 6.
Fig. 9. Graph showing the relationship between diameter of the cellular cloud and the pressure at the base of the inversion.
Fig. 10. Tiros cloud picture of a cyclonic vortex, showing cumulus-type cloud in the northerly flow with heating from below.
Fig. 11. Left, Tiros V showing lee waves west of the Appalachians. Right, upper-air data at Pittsburgh (P) and Washington, D.C. near the time of occurrence of the lee-wave cloud.
Fig. 12. Graph showing relationship of the average zonal wind speed ($\bar{U}$) between 850 and 200 mb and the wavelength of the lee waves.
Fig. 13. Further examples of wave-cloud patterns.
Fig. 14. Schematic representation of movement of spiral eddies in the lee of islands (after Chopra and Hubert).

Fig. 15. Spiral cloud patterns as revealed by Tiros VI.
Fig. 16. On the left, same as Fig. 15. On the right, sketched outline of the same pattern.

Fig. 17. Streaklines in perturbed shear flow.
LOCAL CIRCULATIONS AS SEEN FROM SATELLITE DATA

Studies conducted at CSU under the sponsorship of the Weather Bureau Satellite Laboratory are grouped around two subjects, orographic effects and jet stream structure. The following is a short summary of results obtained up to this time.

Orographic Effects

From recent investigations into the structure of atmospheric turbulence conducted by aircraft several interesting aspects of the atmospheric meso- and microstructure could be found. These flight measurements were mainly conducted over Australia, the Soviet Union, and the United States. A summary of the findings is presently in publication (Pinus, Reiter, Shur, and Vinnichenko, 1966).

Figure 1 shows schematically power spectra obtained by either gust probes mounted on an aircraft nose boom or by accelerometer measurements in aircraft, between wave lengths of approximately 50 m and about 5 km. Measurements from about 6 to 600 km were obtained by doppler radar carried by aircraft. Table 1 gives the references to the simplified power spectra shown in Fig. 1.

Quite surprisingly atmospheric turbulence from very large scales (order of magnitude of several hundred kilometers) to small scales (order of magnitude of tens of meters) has a tendency to follow the
"-5/3" law of turbulent energy dissipation, meaning that the magnitude of turbulent energy is proportional to the -5/3 power of wave number.

Inspecting Fig. 1 we find that the decay of turbulent energy from long waves to short waves in the atmosphere is not quite uniform, however. First of all, we must expect an input region of kinetic energy at wave lengths exceeding perhaps 1000 km, corresponding to baroclinic disturbances in the general circulation, such as cyclones or anticyclones.

A second input region of turbulent energy may be expected somewhere between 600 and 100 km, as is substantiated by the rather shallow slope of the spectrum curves measured by Kao and Woods (1964). This wave length region corresponds to gravity-inertia waves, presently under investigation at CSU. Such gravity-inertia waves seem to be generated by flow over the Rocky Mountains and may be detected from satellite photographs. As a matter of fact, aircraft data as shown in Fig. 1 and satellite pictures seem to be the only data sources from which the existence of such gravity-inertia waves may be deduced. From preliminary studies it appears that a wave length of approximately 500 km indicates the order of magnitude of this type of flow disturbance. Such a wave length is difficult to detect from the current radiosonde network because the spacing of radiosonde stations over the continental United States corresponds approximately to the half-wave lengths of this wave phenomenon. Thus, even if from careful radiosonde analyses such wave disturbances would appear, one would have to discredit them as possible instrument or analysis errors. For the oceanic regions and other areas where the radiosonde network is less dense than over the United States such disturbances become lost altogether.

The theoretical treatment of gravity-inertia waves encounters considerable difficulty because the integration of the equations of motion cannot rely on certain simplifications that may be made in order to obtain pure inertial waves on the one hand, or pure gravity waves on the other hand. Some attempts at theoretical wave solutions in this wave
Returning to Fig. 1 we find that near a wave length of 6 km there seems to be a discontinuity between the slope of spectra obtained by doppler wind measurements and spectra obtained by gust probes or accelerometers. Spectrum No. 11, for instance, which has been obtained under severe clear air turbulence conditions, cannot be connected smoothly with Spectrum No. 1 which, again, corresponds to occurrence of severe clear air turbulence. If we were to interpolate across this data gap at around 6 km we may suspect a wave length region in the spectrum of atmospheric disturbances which does not follow the \(-5/3\) law but which contains an input of kinetic energy into turbulent eddies of smaller scales. Such waves, as is well known, correspond to the long gravity and lee waves frequently observed over and to the lee of mountain ranges and prominent hills. Such waves may be adequately observed from the ground, at least as far as their structure and existence is concerned. For a survey of such long gravity waves over a larger area, again, satellite data are of great value. It appears that a wave length of 1 to 10 km marks the lower resolution capabilities of Tiros photographs. Numerous cases are on record, however, where such waves have been observed and described in connection with lee wave theory as, for instance, postulated by Scorer (1949) and other authors.

Currently attempts are being made at CSU to correlate constant level balloon and special radiosonde observations with the existence of such gravity wave phenomena. These special measurements will then be compared with ground-based cloud photogrammetry and with satellite data.

Again returning to Fig. 1 we find a third region of discontinuities in the spectrum curves, namely at wave lengths between approximately 1 km and 300 m. These wave lengths correspond to short gravity waves observed at stable interfaces in the atmosphere which are associated with vertical wind shears. From aircraft measurements taken over
Australia it has been concluded that the breakdown of such short gravity waves into homogeneous and isotropic turbulence leads to the phenomenon of clear air turbulence which seems to affect jet aircraft of present design between wave lengths of tens of meters to perhaps 200 m. Since the decay of turbulent energy measured at wave lengths of about 300 or 400 m down to very small wave lengths of the order of meters, or perhaps even centimeters, seems to follow rather accurately the \(-5/3\) law of turbulence theory, we may consider the short gravity waves as one of the immediate reservoirs of turbulent energy for the generation of clear air turbulence.

The large spread of spectra shown in Fig. 1, which occurs at wave lengths of approximately 3000 to 4000 m also suggests that turbulent energy generated in lee waves and long gravity waves may constitute a reservoir of turbulent energy which allows this energy to cascade downwards to smaller eddy sizes. This would explain the well known fact that clear air turbulence has a frequency maximum of occurrence over mountain ranges and hills where lee waves are known to exist quite frequently.

Although certain correlations between the occurrence of clear air turbulence have been found with large-scale flow patterns (Colson, 1963), the evidence presented in Fig. 1 would suggest that such correlations (see, for instance, the vertical spread between Spectra 11, 12, and 13), should not be expected to be perfect: too many things may happen at wave lengths of around 10 km or several hundred meters to destroy a clear-cut relationship between clear air turbulence and large-scale flow patterns.

The present capabilities of satellite data indicate a valuable input for the study of atmospheric disturbances in the two important break regions appearing in Fig. 1: between 200 and 600 km and around 4 to 10 km. Satellite data, therefore, could be used to accumulate statistical evidence on such disturbances on a much more complete scale than
would be possible either from ground-based observations or from aircraft measurements.

**Jet Stream Structure**

Although jet streams have been explored to some detail as far as their large-scale flow aspects are concerned, some of their mesostructure, especially with respect to vertical motions around the jet core, still presents a number of open questions. In a recent publication, Oliver, et al. (1964) have documented a case where a bank of cirrus clouds located at approximately 35,000 ft cast a shadow on lower stratus clouds at approximately 7000 to 8000 ft altitude. The edge of the cirrus cloud bank shifted from the axis of the subtropical jet stream in the south to the polar front jet stream in the north.

A re-analysis of this very interesting case showed that actually the flow from the warm subtropical jet stream crossed over and continued in the branch of the polar front jet stream to the east of the upper-air trough. Thus, the edge of the cirrus cloud bank marks the edge of the ascending motion associated with this flow in the subtropical jet stream. The air motion in the branch of the polar front jet to the west of the trough undergoes a subsidence and slides in underneath the cirrus cloud bank which, from there on, obscures its existence. From this peculiar flow behavior in regions where polar front and subtropical jet streams seem to merge we may draw the conclusion that the nomenclature of jet streams, distinguishing between polar front and subtropical jets, may have to be used a little more cautiously. A more detailed description of this case, as well as of the conclusions from the analysis, is contained in a technical report by Reiter and Whitney (1965).

**REFERENCES**

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Fig. 1. Schematic presentation of power spectra obtained by means of gust probes or by accelerometer measurements.
Table I

Power Spectra of Turbulence in the Free Atmosphere

<table>
<thead>
<tr>
<th>Spectrum No.</th>
<th>Source</th>
<th>Characteristics (turbulence components given with respect to course of aircraft)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>Shur, 1964</td>
<td>w-component, severe CAT, near jet stream level, stable stratification</td>
</tr>
<tr>
<td>2</td>
<td>Reiter and Burns, 1965</td>
<td>u, v-components, moderate CAT, jet stream level, stable stratification</td>
</tr>
<tr>
<td>3</td>
<td>Reiter and Burns, 1965</td>
<td>w-component, flight parallel to wind, moderate CAT, jet stream level, stable stratification</td>
</tr>
<tr>
<td>4</td>
<td>Reiter and Burns, 1965</td>
<td>w-component, flight nearly normal to wind, moderate CAT, jet stream level, stable stratification</td>
</tr>
<tr>
<td>5</td>
<td>Vinnichenko, Pinus, Shur, 1965</td>
<td>u-component, no CAT, near jet stream level, stable stratification</td>
</tr>
<tr>
<td>6</td>
<td>Reiter and Burns, 1965</td>
<td>u, v, w-components, light turbulence at 100 m altitude, unstable stratification</td>
</tr>
<tr>
<td>7</td>
<td>Vinnichenko, Pinus, Shur, 1965</td>
<td>u-component, light turbulence at 1000 m altitude, unstable stratification</td>
</tr>
<tr>
<td>8</td>
<td>Kao and Woods, 1964</td>
<td>u-component, at jet stream level, flight parallel to jet stream</td>
</tr>
<tr>
<td>9</td>
<td>Kao and Woods, 1964</td>
<td>v-component, at jet stream level, flight parallel to jet stream</td>
</tr>
<tr>
<td>10</td>
<td>Kao and Woods, 1964</td>
<td>u, v-components, at jet stream level, flight normal to jet stream</td>
</tr>
<tr>
<td>11</td>
<td>Pinus, 1963</td>
<td>u-component, severe CAT, under core of jet stream, flight normal to jet stream</td>
</tr>
<tr>
<td>12</td>
<td>Pinus, 1963</td>
<td>u-component, moderate CAT, under core of jet stream, flight normal to jet stream</td>
</tr>
<tr>
<td>13</td>
<td>Pinus, 1963</td>
<td>u-component, light CAT, over core of jet stream, flight normal to jet stream</td>
</tr>
<tr>
<td>14</td>
<td>Shur, 1962</td>
<td>w-component, moderate CAT, at jet stream level, stable stratification</td>
</tr>
<tr>
<td>15</td>
<td>Shur, 1962</td>
<td>w-component, moderate CAT, at jet stream level, stable stratification</td>
</tr>
</tbody>
</table>
SESSION VI

GENERAL CIRCULATION
INTRODUCTION, GENERAL CIRCULATION

Philip D. Thompson*
National Center for Atmospheric Research

The theme of this morning's discussion will be concentrated on the usefulness of meteorological satellite observations to the study of the general circulation problem. We will also discuss the question of what the satellite data have told us or what satellite data can tell us about the general circulation problems that we did not know before. In my capacity as Chairman I do not presume to answer these questions but I would like to put them into the perspective of a dynamic meteorologist.

It may be useful to look at the question in reverse and ask what conventional measurements do not tell us. Most notably they do not tell us very much about the heat balance, which, as we know, is an all-important factor in the general circulation problem. Most of what we know about the radiative balance is inferred by looking upward from the bottom, by observing what radiation is received at the surface of the earth, making some assumptions about the composition of the intervening atmosphere and trying to calculate about how much radiation has been absorbed and emitted by various atmospheric constituents.

The satellite is in a unique position of having virtually no atmosphere above it so we know pretty well what is coming in at the top of the atmosphere. What is coming in at the top is what is impinging on the atmosphere, and conversely, what we see looking downward from the satellite is exactly what is received from the earth and the intervening atmosphere.

* Chairman
Another very important factor is the transfer of heat by convection, notably in the tropical regions. One can again ask why conventional observations have not told us what we need to know about this phenomena. One problem is that, looking upward from a single point, things that are near loom large while things that are far away seem small with the result that one tends to accentuate things on a small scale. Likewise, when making observations at a number of widely scattered points one tends to accentuate the large scale. This leads to the added difficulty of not being able to obtain an accurate picture of the intervening scale. The satellite is uniquely able to do this. To date the satellite has given us a great deal more about the scale, structure, convective circulations and the degree of organization about certain atmospheric phenomena. The past few sessions have already raised certain questions regarding the interaction between mesoscale and large-scale motions that have never been raised before and which may call for some considerable re-examination of our notions about the dynamics of the atmosphere.

With this setting I call upon Professor Verner Suomi of the University of Wisconsin, who, as we all know, is one of the pioneers in satellite meteorology, to lead off the subject under discussion.
In discussing the general circulation it occurred to me that when, some ten or twenty years from now, the history of meteorology will be written, there may be a comparison between the contribution made by the satellite and that made by the computer. I do believe that when that history is written it will be the numerical model that will be the biggest discovery or invention.

The discovery or invention of that numerical model is like the invention of the wheel, in this case a hydrodynamic wheel. It is an amazing revelation to see how well the simplified models of the atmosphere perform. They really have no business to do that well.

If I may be permitted to use the analogy that the large-scale motion is like an hydrodynamic wheel I would like to deliberate this morning on the "hill" down which this hydrodynamic wheel is allowed to roll. The heat budget of the earth is the furnace and condenser of this giant atmospheric engine and the models so far designed work very well in terms of mean values; that is, if we insert the correct size of the "hill." I would now like to discuss some aspects of the variations of this "hill."

Scarcely any models incorporate the effect of cloudiness, although we do know that cloudiness affects the albedo of the earth and also affects the escape of heat from the earth. Experiments have indicated that cloudiness is far more effective in controlling the outgoing long-wave radiation than the temperature and the moisture structure. Cloudi-
ness is therefore the important variable. I will briefly discuss aspects of its importance in the global heat budget as measured by Tiros satellites, where one is mainly concerned with the generation of zonal available potential energy. At this stage I wish to acknowledge that the following discussion is the result of research work by many persons.

It is well known that solar energy reaches the earth at a more favorable angle in the tropics than at the poles. On the other hand, the outgoing radiation is more uniformly distributed with respect to latitude. Therefore the main interest is centered on the difference between the incoming short-wave radiation and the outgoing long-wave radiation. In general, the difference is positive in the tropics, that is, more heat is received than lost into space, and over the polar regions it is negative. However, it must be realized that we are measuring the difference between two large quantities and a small error in one measurement will cause a large change in the difference between the two. Therefore, in practice, the net radiation is difficult to measure.

Figure 1 shows the mean radiation balance of the earth as measured by Tiros IV. Curve 1 is the absorbed short-wave radiation in \( \text{cal cm}^{-2} \text{ min}^{-1} \) as a function of latitude. Curve 2 depicts the emitted long-wave radiation. The dots indicate the mean long-wave radiation as measured by Explorer VII. The latitude of radiation balance is shown to be 33°.

A comparison of the meridional variation of the outgoing long-wave radiation for two precessional cycles is shown in Fig. 2. Both curves show minimum values in equatorial latitudes, maxima around the latitudes of the subtropical highs and low values towards the poles. The equatorial minimum clearly indicates the effects of moisture and clouds as opposed to the drier and less cloudy skies of the subtropical highs causing the maxima.

The albedo measurement in per cent as a function of latitude for the two cycles is presented in Fig. 3 and the net radiation computed from
Figs. 2 and 3 is shown in Fig. 4. Inspection of the related equations, as presented by House (1965), shows this measurement to be the most accurate of all. The diagram shows a large latitudinal shift of net radiation from one cycle to the next. This is partially explained by the small variations in albedo and small variations in long-wave radiation, but it is mainly explained by the difference in position of the sun. This measurement of the net radiation is really then the forcing function which provides the "hill" for the hydrodynamic wheel.

It is naturally of interest to compare the above measurements with estimates made by previous research workers. In Fig. 5 the latitudinal variation of the long-wave radiation measured by Tiros IV for March, April and May is compared with estimates made by London (1957) for a similar period. Because of the precessional cycle it was possible to match the two periods very well and London must be credited for his work, considering the sparsity of data at his disposal. The stippled lines in Fig. 5 indicate the uncertainty in the values as discussed by House (1965). The agreement of London's values is very good in the northern hemisphere. However, in the southern hemisphere, his values are lower than those of the satellite. It must not be forgotten that London's calculations are for the northern hemisphere. One can argue that the satellite observations should be higher because of the lag in the seasons for ocean areas. Consequently, for the southern hemisphere with its large ocean expanse, one can expect warmer temperatures and consequently a larger long-wave radiation loss to space.

In comparing the meridional variation of albedo as presented in Fig. 6, London's estimates appear to be too high, particularly for the tropics. Although this difference is of the nature of about 5%, it actually means a change in heat transport of about 40% between the latitudes 20°N and 20°S. The lower albedo values measured by the satellite in the tropics indicate a greater absorption of energy than provided for by London's values.
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In Fig. 7 a comparison is made of the mean annual meridional variations of albedo as computed by Houghton (1954), London (1957) and House (1965). The comparisons should be qualified as follows. Houghton and London derived their results by considering the annual climatological means of the various parameters involved. The satellite measurements, however, did not cover a full year. They more or less covered the northern hemisphere spring and the fall season for the opposite hemisphere. While one could argue that they really covered about eight months, they did not cover the extreme winter and summer seasons, which may play an important part in the evaluation of the annual curves. Nevertheless, Fig. 7 again shows an approximate 30 to 40% difference in heat transport between the satellite curve and the other two in the equatorial latitudes for the annual curves.

Figure 8 presents the total heat transport required by the planetary radiation balance as obtained by the various research workers. Good general agreement is shown poleward of 50°, but equatorward of 40° the Tiros measurements indicate a larger heat transport. Equatorward of 20° this excess reaches 40% as a result of the larger input of energy in tropical latitudes, due to the measured lower albedo.

Finally, it is of interest to consider the variations in radiation from year to year. Unfortunately, this is difficult because satellites do not at present last for many years and, even if they did, one would not be sure of the calibration of the sensors during such a long lifetime. In the future, it may be possible to retrieve the instruments for recalibration or perhaps recalibration in space will become feasible. Figure 9, however, does compare two periods which are very nearly the same in terms of the time of year. One measurement is made by House and the other by Bandeen, et al. (1964) for Tiros VII. In comparing the two sets of curves it must not be forgotten that Tiros IV data did not have the geographical resolution obtained by Tiros VII. On the other hand, one could argue that even if the absolute values of calibration were different, which they can be, in certain regions they do have the
same relative value, but no matter what form of smoothing is applied one obtains a substantial difference in value (of the order of 10%), which again for the tropics indicates an enormous effect on the heat transport. The differences between the curves may be partly explained by the fact that the times are not exactly the same; this needs further careful investigation.

In conclusion, I will summarize by saying that we have latitudinal, longitudinal and time variations of radiation which are variations of the forcing function of the atmospheric "hill." They should be investigated in the future and our chances for looking at them more carefully are definitely good.

REFERENCES


Note: Due to non-availability of figures, the remainder of Dr. Suomi's talk has been omitted.
Fig. 1 The mean radiation balance of the earth from Tiros IV satellite:

Curve 1 - Absorbed Short-wave Radiation
Curve 2 - Emitted Long-wave Radiation
Dots - Explorer VII Mean Long-wave Radiation.
Fig. 2 Comparison of the meridional variation of long-wave radiation for two precessional cycles.
Fig. 3 Comparison of the meridional variation of albedo for two precessional cycles.
Fig. 4  Comparison of the meridional variation of net radiation for two precessional cycles.
Fig. 5 Comparison of the meridional variation of long-wave radiation for March, April, and May.
Fig. 6  Comparison of the meridional variation of albedo for March, April, and May.
Fig. 7 Comparison of the annual meridional variations of albedo.
Fig. 8. The flux of total heat as required by the radiation balance.
Fig. 9  Tiros VII data for March, April and May, 1964.
RADIATIVE HEATING

Jay S. Winston

Meteorological Satellite Laboratory, ESSA

I have some material to present on the variations of radiation with large-scale circulation and the possible feedback of this heating on the circulation. One of the important tasks we have is to study radiative heating measured by satellites relative to basic variations in the circulations over various time periods (i.e., weeks, seasons, years). This work has only barely begun with some of the Tiros radiation data.

Latitudinal distributions of the albedo and long-wave radiation from Tiros IV measurements have been examined in relation to the large-scale flow pattern over the Pacific Ocean area for several 5-day periods in the spring of 1962. Appreciable differences in latitudinal profiles of short-wave and long-wave radiation are found for differing states of the circulation pattern. An example of this is shown in Fig. 1 where it may be seen that the period 29 March to 2 April 1962 is one of strong westerly flow between latitudes 30 and $45^\circ$N, whereas the following period has considerably diminished zonal wind speeds in these same latitudes. In the case of the strong westerly flow the albedo shows a marked decrease from latitude 40 to $20^\circ$N, much of this gradient coinciding with the strong anticyclonic shear on the south side of the westerlies. The long-wave radiation between 35 and $20^\circ$ shows a similar but inverse relationship. These profiles are indicative of a marked variation from considerable major cloudiness in the westerlies to relatively little cloudiness on the south edge of the westerlies. In the period with diminished westerly flow (the mid-tropospheric map, which is not shown

* Discussant
here, consisted of a broken-up flow pattern with several cyclonic and anticyclonic cells) the albedo gradient is displaced southward from the preceding period, the main gradient now located between $30^\circ N$, at which there is a maximum of albedo, and $10^\circ N$. The long-wave radiation shows a somewhat similar, but inverted latitudinal variation, southward of $30^\circ$. This indicates that there is considerably more cloudiness extending farther southward in the second period and that the major area of little or no cloudiness is displaced about 5 to $10^\circ$ latitude southward also.

This variation in flow patterns is by no means the most extreme that is observed, since the weakened westerly state in this case is really not the typical low index situation where the westerlies split with maxima of westerly flow at high and low latitudes and a relative minimum of westerlies in middle latitudes.

A schematic illustration of variations in circulation of the type associated with an index or energy cycle and the accompanying major cloudiness distributions (shaded) is shown in Fig. 2. Also shown are profiles of absorbed solar radiation, derived from the mean annual solar energy available at the top of the atmosphere and a hypothetical distribution of albedo based on the types of distributions found in the cases described above. It will be noted that the high index, or flat westerly, stage A has a very sharp gradient of absorbed solar radiation northward from $20^\circ$. The lower index stage C demonstrates a marked weakening of the north-south heating gradient between latitudes 30 and $45^\circ$, and there is a strong gradient between 30 and $10^\circ N$, which is markedly different from the gradient of the high index stage. Northward of $45^\circ$, the gradient is also strong, similar to stage A in these latitudes. The contributions of these heating profiles to the generation of zonal available potential energy have been calculated, assuming a normal north-south temperature profile, and it is seen that energy generation associated with this component of heating alone is about 25% greater in the high index stage than in the low index stage. Thus it would appear that the varying cloud distributions associated with the variations
in the flow patterns can have some significant influence on the generation of zonal available potential energy and therefore affect future developments of the circulation pattern.

To test for the quantitative influence of these heating variations on the circulation, an experiment was devised utilizing a series of variable heating profiles in the Mark I numerical generation model of Smagorinsky (1963). Smagorinsky's model is a primitive equation model using two levels, 750 and 250 mb. Smagorinsky used Houghton's (1954) calculations of the mean annual short-wave radiation absorbed by the earth-atmosphere system and expressed long-wave radiation as a linear function of temperature. Since the latter has a very small variability in his experiment, he essentially used a constant north-south heating gradient. In the variation tried here, five separate stages of north-south solar heating profile have been constructed. The average stage is the same as Smagorinsky used in his original experiment; and the others are variations about this heating profile based on the relation between the radiation and circulation described above. The extremes are the same as shown in Fig. 2 for high index and low index heating profiles and the other two stages are intermediate between the average and the two extremes.

In this experiment the zonal available potential energy was used as an index of the state of the circulation and the heating profiles were varied according to changes occurring in the zonal available potential energy. Every 24 hr a test was made to see how far the available potential energy had changed from the latest maximum or minimum. For each change greater than 1.5% and multiples thereof up to a maximum of 6% the heating profile stage was changed upward or downward, depending upon the direction in which the available potential energy was changing. This heating input is essentially indicated by the daily values of the generation of zonal available potential energy in the experiment (lower half of Fig. 3). The actual start of the experiment occurred on day 184 at a time when the available energy had just exceeded a value.
4.5% below the previous maximum near day 174. Thus proceeding toward lower values of zonal potential energy, on day 184 the heating profile was changed to one weaker than the mean which immediately produced less energy generation. Later on, when the available energy started increasing again, the heating profiles were shifted upward toward the stronger north-south heating of the high index period (Stage 1) and considerably more energy generation ensued in that period.

The basic results are indicated in the upper half of Fig. 3, where the zonal available potential energy for the two experiments is shown. The zonal available potential energy in the first several days decreased more than in the original experiment and took longer to recover to high values; but then as a greater north-south heating contrast was applied, the available energy climbed to considerably higher values than were observed in the original experiment. The net results of these developments are that the length of the energy cycle increased from 26 to 41 days and that the amplitude of the variability of the zonal available energy increased considerably over the original experiment. Also it is to be noted that the two curves are no longer correlated after about day 200. Thus the imposition of variable heating markedly changes the character of the variations of the circulation in this model. (This is also indicated by other parameters in addition to the zonal available energy.)

These results suggest, then, that the differing distributions of cloudiness which accompany various circulation states may have substantial feedback on the future states of the circulation through their influence on the basic energy source for the general circulation. In this case these effects would be of importance for periods of several weeks and it is possible that such effects would still be of major importance for even longer periods such as months or seasons.
REFERENCES


Fig. 1  Relationship between long-wave radiation, albedo and zonal wind speed. Upper figure for strong westerly flow, lower figure for weak zonal flow.
Fig. 2 Schematic illustration of variations in circulation of the type associated with an energy cycle and the accompanying major cloudiness distribution (shaded).
Fig. 3  **Lower half:** daily values of the generation of zonal available potential energy as obtained in both experiments (see text).

**Upper half:** resulting profiles of the zonal available potential energy.
SESSION VII

WATER VAPOR, TEMPERATURE STRUCTURE
AND HEAT BUDGET
Earlier in the meeting we talked primarily about cloud formations, cloud and earth-surface temperatures, and the broad scale of reflected and emitted radiation. So, in essence, we have covered satellite radiation measurements in the "window" region of the infrared spectrum, regions where one could "see" through the atmosphere. In fact, we discussed at great length the interference by the atmosphere in interpreting these measurements meteorologically. Today, we shall talk about radiation measurements in spectral regions where the atmosphere is the primary emitter and where we really do not want to see through to the ground. These regions are obviously the carbon dioxide and water vapor bands. Mr. Bandeen will talk about the Tiros measurements in the 6.3 \( \mu \) water vapor band later. I shall concentrate on the 15 \( \mu \) carbon dioxide band. I shall report on the method by which stratospheric temperatures were derived from radiance measurements made by Tiros VII between roughly 14.5 and 15.2 \( \mu \). In this spectral range, radiation is almost entirely emitted by carbon dioxide in the altitude range of about 15 to 35 km.

In order to interpret the stratospheric temperature patterns one must know the limitations inherent in the method of deriving these temperatures. A brief description of the method, I think, will demonstrate these limitations to you. Then the discussion will hopefully show a few newer things; namely, what it has been possible to get out of these measurements meteorologically. Figure 1 shows the radiometer's spectral
response which is primarily determined by an interference filter. You see that the filter is not terribly transparent -- at the peak, the transparency is only 20%. For Tiros it is the best we could do, making the sensitivity of this particular channel somewhat poor, which resulted in a poor signal-to-noise ratio and a poor accuracy. We hope to improve this situation in Nimbus. Because of the poor signal-to-noise ratio of the individual measurements (50 by 50 km) and because of the relative constancy of stratospheric temperatures, we decided to present these data in weekly averages. This is a compromise between the accuracy that we could get out of this particular measurement and the variations of temperature structure in the stratosphere that we might expect. We feel that if the stratosphere temperatures are relatively stable over periods of a few days, then we can compose worldwide weekly averages with confidence. It also happens that over a period of one week the spatial coverage from Tiros is such that a sufficient population of measurements is accumulated within one grid element (2.5 by 2.5° of latitude and longitude), that one can achieve an accuracy of about 1°K in the average temperatures from one grid element over one week. For individual measurements, this accuracy becomes much poorer. It is in the order of 5°K at the very best, perhaps even poorer than that.

I am talking strictly about a random error. The random error in the individual points is in the order of between, roughly, 5 and 10°K. However, this random error reduces very sharply as the measurements are averaged. Of course, you could not reduce the systematic error with a greater number of measurements. Talking about the random error here is important to justify the averaging process for the form in which the data will be available (weekly averages over the portion of the globe which the satellite covers, roughly from about 65°N to 65°S). If the random errors in the individual measurements were less, one would not need to go to weekly averages and one could look for phenomena in the stratosphere on a small time scale. Because of the necessary averaging it will not pay for the Tiros VII measurements to look for stratospheric
events with time scales substantially smaller than a week or geographic scales smaller than 2.5 by 2.5°. In some cases a few days will still be acceptable, but certainly nothing less than that.

Winston: Why would not the spatial averaging of something in the order of a few degrees latitude and longitude take care of this?

Nordberg: We are already averaging over something like 2° of latitude and longitude. Now, if you want to go to a larger spatial average you will have to trade space averages for time averages. For example, you can determine the average stratospheric temperature of the entire globe with extremely great accuracy in one instant of time (12 hr is the shortest time during which the satellite covers that large an area). But, I do not know what good it might do to have a very precise temperature on so coarse a geographic scale.

Now, how do we get stratospheric temperatures from measurements of intensities of emitted radiation in the carbon dioxide band? To some of you this might not be as obvious as it might be to others. In Fig. 2(b) the solid line depicts the amount of radiation received by the satellite within the stated spectral interval from altitude layers 1 km thick as a function of altitude. We call this the weighting function. It is expressed in units of watts per square meter per steradian per kilometer. This quantity, of course, is a function of many things, including atmospheric temperature. So it is a cyclic thing: we try to derive the temperature of the stratosphere from the radiation measurement, but you have to know the temperature distribution with height in order to assign the derived temperature to an appropriate height level. Fortunately, temperature enters into the determination of the weighting function only in second order. So, one postulates a temperature distribution on the basis of climatology or on the basis of whatever one knows from radiosondes. This is similar to the derivation of cloud heights from temperature measurements in the window region.
If one postulates the temperature profile (Fig. 2a), the specific absorption coefficient for carbon dioxide, the type of radiative transfer model to be used, the mixing ratio of carbon dioxide to air, etc., one can draw the weighting curves shown in Figs. 2(b) and 2(c). The solid curves, for example, show that the maximum contribution to the satellite-measured radiation comes from an altitude of about 25 km, with substantial contributions within the heights of 15 to 35 km. Practically no radiation comes from the ground and very little radiation (less than 10%) comes from below 10 km.

In addition to temperature, the weighting functions depend also on the viewing angle. The curves shown in Fig. 2(b) are for a viewing angle (nadir angle) of 0°, which means the satellite is viewing straight down. In Fig. 2(c) they are for nadir angles of 52 to 58°. You see that in an absolute sense the weighting curves do change; however, the height range does not change very appreciably with temperature or viewing angles. The maximum contribution for all four assumed temperature profiles still comes from between 20 and 25 km, the contribution from below 10 km is still negligible and contributions from above 35 and 40 km are also relatively small. At nadir angles of 58°, the steepest viewing angle that is possible from Tiros, the maxima in the curve shift upward somewhat. In the interpretation of the data attempts were made to compare the temperature maps obtained at low viewing angles to temperature maps at high viewing angles and to obtain some vertical structure. This was possible in a few individual cases. For example, we have studied stratospheric warmings which have shown warmer temperatures at nadir angles of 58° than at the vertical because, apparently, the most intense warming occurred at the higher altitudes, closer to the maximum in the weighting curves for the larger nadir angles. However, this method reveals the height structure of the warming only very crudely, since a change in nadir angle from 0 to 58° changes the height level of the maximum in the weighting curve by only about 2 km. In general, then, the satellite observes a radiation intensity originating in
varying amounts between about 15 and 35 km. If one derives temperatures from these radiation intensities strictly on the basis of Planck's Law, the temperatures will be weighted averages within that height interval, weighted according to the curves shown in Fig. 2.

Figure 3 illustrates this point further. It relates quantitatively the effect of a given change in temperature within a given height interval to the change in radiance observed by the satellite and to the derived weighted average temperatures. On the left is a repeat of the weighting curve and temperature profile for the tropical atmosphere shown in Fig. 2. On the right the same weighting curve is drawn for a temperature distribution where temperatures between 25 and 35 km were increased arbitrarily by $10^\circ$ over the temperatures shown on the left. Then the radiation intensity, or the contribution of radiation to the satellite measurements, increases by the amount illustrated by the shaded area. The shaded area thus represents the change in the weighting curve. The integral under the weighting curve corresponds to the total radiation seen by the satellite. This total radiation can be interpreted in terms of the "weighted average temperature" of the stratosphere weighted with height according to the curves. For the temperature profile and weighting curve shown on the left of Fig. 3 (tropics), that average temperature is 228$^\circ$K. And, indeed, in Figs. 4 and 5 you will see that this is what the satellite observed, in general, over the tropics. For the temperature distribution shown on the right of Fig. 3 (as stated previously, a temperature increase of $10^\circ$ over a 10-km height interval from 25 to 35 km) the derived average temperature is 231$^\circ$K. If the same temperature increase of $10^\circ$K occurred not between 25 and 35 km, but between 33 and 43 km, then the weighting function would change by a much smaller amount and the derived average temperature would only be 228.5$^\circ$K. This is only one-half a degree higher than for the original temperature profile shown on the left of Fig. 3, a change not noticeable in the measurements. This illustrates that any temperature changes, either with time or location, seen by the satellite re-
reflect the actual temperature change in greatly varying amounts depending on where the actual temperature change occurs in relation to the weighting curve maximum. An actual temperature increase of 10°K at the height of the maximum in the weighting curve (15 to 25 km) will result in an increase of 5 to 6°K in the derived average temperature.

Figures 4 and 5 show the derived average temperatures on a worldwide scale. Figure 4 illustrates a situation well known from climatological patterns expected for northern hemisphere winter. Such patterns have in the past been obtained from radiosondes, but now we have a continuous picture over what we call the quasi-globe, from latitudes of 65°N to 65°S. The isotherms in the southern (summer) hemisphere show the rather weak and uniform temperature gradient from the summer pole to the equator: 240°K at about 63°S, and 220 to 225°K in the broad zone of the tropics. When this belt of 220 to 225°K extends into the winter hemisphere to about 30° latitude, the isotherms become tightly packed toward the winter pole, much more tightly packed than at equivalent latitudes in the summer hemisphere. The interesting feature is the nearly perfect zonal picture in the southern hemisphere versus the not-at-all zonal structure in the winter hemisphere. The warm center of about 230°K average temperature over the Gulf of Alaska obviously reflects the Aleutian anticyclone. The very cold core over the North Atlantic, with temperatures reaching down to 210°K just above 60°N, indicates the location of the polar vortex. A stratospheric warming is indicated by a warm center over the Middle East.

In August (Fig. 5) one sees the essentially zonal structure of the isotherms in the northern hemisphere with maximum temperatures of 236°K at 60°N. You remember, in the southern hemisphere it was 240°K at 62 or 63°S. But, in the southern hemisphere, we now see a significant departure from the zonal pattern: a warm region prevails over southern Australia, the southwestern Pacific Ocean and the eastern Indian Ocean with temperatures around 230°, while at the same latitude over the Eastern Pacific ocean the temperature is between 220 and 225°. This situation has
now been observed in two seasons. I should point out that these data have been available for a total of two years from Tiros VII, with the first year (June 1963 to June 1964) affording fairly good geographic coverage. For June 1964 to June 1965, the interrogation of the satellite became more sporadic and the data were not as continuous as in the previous year. In both years the region over the west Pacific and the east Indian Ocean was much warmer in winter than the south Atlantic and the west Indian Ocean regions.

DISCUSSION

Koteswaram: At what layer is this, or what angle?

Nordberg: This is at a nadir angle from 0 to 40°, essentially looking straight down. The weighting curve maximum is near 22 km. There has been the usual systematic error that is inherent in all Tiros sensors which is a time degradation. The sensor becomes less and less sensitive as time progresses. An empirical correction is made for this degradation. I believe that the magnitude for this systematic correction is in the order of about 5°.

van de Boogaard: You have assumed here clear atmospheres all the time. Could you comment on the effect of low clouds, below 10 km? Then could you comment on whether it is worthwhile to make any corrections for high cloud systems, above 10 km?

Nordberg: Well, of course we have assumed a clear atmosphere, and we have made a study to show that lower clouds, below 5 km or so, have practically no effect. This can be seen easily from the weighting curves. High clouds have a pronounced effect and, particularly, clouds over severe storms. The largest effects, as one could expect, can be found over hurricanes and severe tropical storms. Temperature changes of as much as 5 or 10° can be caused by such clouds. Now, part of this change is indeed due to the radiating cold cloud surface. The other part is due to the cold air on top of the clouds brought up by convection. But, since the temperatures shown in the last two figures were averaged over a week and over large geographic areas, we expect that the effect of clouds is pretty well smeared out. Thus any temperature dis-
continuity in the pattern shown would not be due to storms but, rather, due to large-scale variations in the stratospheric temperatures, because of the large averaging. Individual points when not averaged, however, will be affected strongly by storms. In summary then, any high altitude cloudiness, particularly if the clouds go above 10 km, would make the temperatures colder by as much as 5° and possibly by 10°; that may not be due to the cloud alone, but to the effect of cold air.

Fritz: I wonder if it is worth making a slight clarification about how these temperatures are obtained -- not what the meaning is, but how it is obtained. Is it true to say that before the flight, before the satellite is launched, you took the instrument and looked with that at some black bodies and then got some calibrations of the electrical emission (voltage or something like that) against the blackbody temperatures which you were looking at? And these numbers result from the fact that the satellite has sent down a signal which you interpret, say, in voltage or some way, and taking your previous calibration you then find what the equivalent blackbody temperature was? Now the interpretation in terms of the atmospheric properties would depend on the things you discussed earlier. But these numbers are merely the equivalent blackbody temperature according to calibration.

Nordberg: Exactly. Equivalent blackbody temperatures.

Singer: Would it be a good idea to present the data in a slightly different form? You have taken certain atmospheres, which you showed in the first slide -- typical climatological atmospheres -- which have in your interpretation certain blackbody temperatures. Would it make sense to present your data in reference to these standard atmospheres, in the tropics and in the arctic, as being colder or warmer than these standard atmospheres rather than to use the blackbody temperatures, which are artificial anyway?

Nordberg: Well, this would be the next step. But there are so many different kinds of interpretations that one could make. If one is interested only in a gross change (how the temperature pattern changes longitudinally, for example), it does not really make too much difference
what weighting curve you use. But now, if somebody wants to know exactly where the maximum of the weighting curve is, then one has to make a pretty good assumption of the temperature profile.

Singer: Well, take the tropical atmosphere you had before and assume that is the standard. Then I would like to know whether what you would mark as cold air would be very much colder than this standard atmosphere.

Nordberg: No. The weighting curve does not do anything but tell you roughly what the height level is to which these averages correspond. The temperature itself will not change. The temperature is truly a measurement. The only question is: what does it represent meteorologically? It represents, in general, a weighted average between 15 and 35 km. Just how exactly that average is weighted then depends on what altitude the radiation really comes from, which then depends on the vertical temperature profile. I do not think that in most cases it is worthwhile to assume that profile exactly a priori.

Renard: Is there any significance in taking a one-week period rather than a two-week period or a single day?

Nordberg: The significance, as I tried to state at the outset, is that we feel that within one week we get sufficiently dense coverage within grid elements of 2.5 by 2.5° over the entire quasi-globe. Any temperature differences which show up in these averages will be meteorologically significant rather than just random errors.

Renard: What about deviations from day to day?

Nordberg: They could not be seen, and as I said before, that is what you cannot get out of it.

Kindler: The primary reason for the errors are calibration problems or your uncertainty of your influence here?

Nordberg: What do you mean by "influence"? The weighting curve? (Gets affirmative answer.) No. The weighting curve is the least problem. The calibration of the instrument before flight is of course no problem either; it is just a matter of doing it right. But the biggest problem in the systematic errors is the instability of the sensor after it is in orbit -- the change of calibration. The instrument is built in such a
way that you do not have an adequate check of the calibration during flight. This is the biggest source of the systematic error. The biggest sources of the random error are simply the narrowness of the filter, the inefficiency of the filter, and the narrowness of the spectral interval that you have to work with.
Fig. 1 Effective spectral response of Tiros VII. 15 $\mu$ channel as a function of wavelength.
Fig. 2 a) Typical temperature profiles based on proposed supplements to the U.S. Standard Atmosphere 1962 for 60° North summer, 60° North winter (warm and cold) and 15° North. The "warm" and "cold" temperature profiles for 60°N (high latitude winter) can be considered typical at these latitudes depending on the state of the stratosphere in these regions.

b, c) Weighting functions $\Psi (h)$, applying to the measured outgoing radiance $N$; nadir angle = 0°, rep. 58°.
Fig. 3  Tropical standard atmosphere and weighting function for 15 $\mu m$ carbon dioxide channel of Tiros radiometer vs. height.
Fig. 4 Quasi-global map of 15 μ equivalent blackbody temperatures averaged over the week of 15 to 22 Jan. 1964; nadir angle 0° to 40°.
Fig. 5  Quasi-global map of 15 $\mu$ equivalent blackbody temperatures averaged over the period 5 to 13 August 1963; nadir angle 0° to 40°.
When the satellite radiometric measurements of the 15μ emission band became available, one of the first questions asked was what one could do with the data from the point of view of synoptic representation of temperature patterns in the stratosphere. The first few studies were performed with practically uncorrected data; that is, no account was taken of instrumental degradation. Later it was proved that if one was just interested in the pattern, data of this type were sufficient and applying the correction did not basically change the pattern. Only absolute values were affected and in case the degradation was nonlinear some change in the gradients was observed.

Figure 1 shows for 21-22 January 1964 the global distribution of radiosonde stations capable of measuring temperatures at the 30-mb pressure surface. It is readily observed that at that level the coverage is particularly poor over the southern hemisphere in general, and also over the higher and lower latitudes and the oceans of the northern hemisphere. The observations for both 0000 and 1200 GMT are depicted. For a single observation time the observational network could be classified as extremely poor.

As a first step, a global temperature analysis was performed for 21-22 January 1964 using radiosonde data only; the result is presented

* Discussant
in Fig. 2. Although observational data were virtually non-existent in the southern hemisphere, it was fortunate that the data chosen corresponded to the summer for that region. During this time it may be confidently assumed that the isotherms run parallel to the latitudes. Consequently, with observations available in the Australia, New Zealand and antarctic regions, the analysis as shown in Fig. 2 was performed.

For the northern hemisphere the details analyzed are well substantiated by actual observations. The figure shows the general pattern of mid-stratospheric temperature distribution in winter, prominently displaying the existence of the Aleutian anticyclone, the polar vortex over Greenland and the European sector of the arctic, the warm belt over the lower middle latitudes, the cold belt over the tropics, and from there the steady temperature increase towards the South Pole.

Figure 3 shows the equivalent blackbody temperature of the 15μ carbon dioxide channel of Tiros VII for the same day. Comparison with Fig. 2 shows the overall pattern to be similar. Using the two maps, a linear correlation coefficient was computed for the northern hemisphere by taking the temperature values at designated geographical gridpoints. Figure 4 is a scatter diagram of these temperature readings with the radiosonde values plotted along the ordinate and the satellite values along the abscissa. If the correlation had been perfect, all points would have been on one straight line. The actual result shows that the values are generally well distributed parallel to a line through the origin with a slope equal to 1. This indicates a systematic difference between the two sets of data. The correlation coefficient obtained is $r = 0.90$. In judging these results one must be reminded that the satellite equivalent temperatures are the averages of a certain stratospheric layer of air and are not the temperatures of the 30-mb pressure surface.

Similar computations for the southern hemisphere are portrayed in Figure 5. As this is the summer hemisphere, low temperatures are
The high correlation coefficients obtained in both cases indicate that one can interpret the satellite remote-sensing radiation measurements of the 15μ band as an equivalent of the 30-mb temperature field. One may be reminded from discussions by previous speakers that, from the point of view of the weighting functions, the 30-mb pressure surface provided the maximum contribution to the emission signal. It must also be pointed out that the profile of the weighting function has a different shape for different atmospheres. The colder the atmosphere, the lower the sensor is looking. Consequently, it is obvious that if one were considering temperatures at constant height levels this effect would produce difficulties. In the case of constant pressure surfaces, a particular pressure surface would be lower down if the atmosphere were cold or higher up if the atmosphere were warm. The change of height of the pressure surface and the maximum of the weighting would therefore be more-or-less in phase, and consequently would result in an improved correlation. In view of this, application of remote sensing temperatures at constant pressure surfaces is recommended.

It must also be remarked that it should be possible to perform similar investigations with regard to the thickness pattern of say the 100- to 10-mb layer which provides the major contribution to the 15μ radiation band. Having obtained such a thickness pattern, graphical addition to the 100-mb contour pattern would provide a 10-mb contour chart. As the 100-mb pressure analyses are more readily obtainable, global 10-mb pressure analyses could be obtained.

The second part of my presentation will concern the details of the changes with respect to time of stratospheric temperature changes observed by the satellite. Figure 6 shows the analysis of the satellite-measured temperatures (T_{BB}) averaged for the period 15-22 January 1964. No corrections were applied to the measurements. The figure
Session VII displays a typical winter pattern with the polar vortex located over northern Europe and no noticeable temperature contrasts between 20°S and 20°N.

During the following week, 22-29 January 1964, the temperature field underwent remarkable changes, as indicated in Fig. 7. A warm area developed near the Caspian Sea. Radiosonde observations over central Europe towards the end of the month indicated that a stratospheric warming had occurred. Figure 7 shows that this warming was also detected by the satellite. Over central Europe the warming did not penetrate lower than the 10-mb (30-km) level, but at the 38-mb surface the temperature rose to an observed maximum of +1°C over Berlin. Judging from the large temperature increase detected by the satellite over the Near East, and keeping in mind the profile of the weighting function, one can deduce that the warming penetrated much farther down than to 30 km, indicating a magnitude of about 30 to 50°C at about the 25- or 10-mb pressure surfaces. A temperature increase of 49.3°C was observed at 37 km over Berlin from 2-31 January 1964.

Figure 8 displays the stratospheric temperature change (°K) between the two periods under discussion. The area around the Black Sea and the Caspian Sea indicates a temperature rise of about 10° of equivalent blackbody temperature (T_{BB}). As explained by the main speaker at this session, an air temperature change of 10° results in a 3° change in the T_{BB} value. Consequently, the change indicated around the Near East must be caused by an upper stratospheric warming of more than 30°.

The conclusion to be drawn from the above, and from other case studies not presented here, is that the satellite-borne radiometer measurements in the 15/μ carbon dioxide emission band have a very high potentiality for synoptic surveys of the temporal and spatial thermal behavior of the middle stratosphere. Furthermore, whereas the lack of conventional radiosonde measurements over strategic regions made it impossible to detect the areas of origin of the stratospheric
warmings, the satellite measurements now seem to indicate that these may be found somewhere in the subtropics or even the tropics proper. This aspect of the stratospheric warming phenomenon seems to be the task to be undertaken in the near future.
Fig. 1 Distribution of stations reporting 30-mb temperatures at 1200 GMT on 21 January, and 0000 GMT on 22 January 1964.
Fig. 2  Quasi-global analysis of the temperature field at the 30-mb surface on 22 January 1964 at 0000 GMT based on radiosonde measurements (°K).
Fig. 3 Quasi-global analysis of Tiros VII, 15/μ channel radiometer measurements ($T_{BB}$) in a 24-hr period 21-22 January 1964.
Fig. 4 Scatter diagram of mid-stratospheric temperatures on 22 January 1964 over the northern hemisphere gathered from isopleths analysis of radiosonde data and satellite-borne radiometer measurements (°K).
Fig. 5 Scatter diagram of mid-stratospheric temperatures on 22 January 1964 over the southern hemisphere gathered from isopleths analysis of radiosonde data and satellite-borne radiometer measurements (°K).
Fig. 6 Quasi-global analysis of Tiros VII, 15 μm channel radiometer measurements ($T_{BB}$) in a weekly average from 15-22 January 1964.
Fig. 7  Quasi-global analysis of Tiros VII, 15μ channel radiometer measurements ($T_{BB}$) in a weekly average from 22-29 January 1964.
Fig. 8 Stratospheric temperature change (°K) from 15-22 January 1964 to 22-29 January 1964 as derived from weekly means of Tiros VII, 15 μ channel radiometer measurements.
ATMOSPHERIC WATER VAPOR CONTENT FROM SATELLITE RADIATION MEASUREMENTS

W. R. Bandeen
Goddard Space Flight Center, NASA

The Tiros II, III, and IV satellites carried a five-channel, medium resolution radiometer, one channel of which was sensitive to radiation with the 6.3 μm absorption band of water vapor. We shall discuss how data from this channel, together with data from another channel sensitive in the 8 to 12 μm atmospheric window, have been used to infer atmospheric water vapor content.

The equation of radiative transfer expressing the effective radiance to which the water vapor channel responds, \( \bar{N} \), can be written

\[
\bar{N} = \int \int B(\lambda, H) \phi(\lambda) \frac{\partial T(\lambda, H, \Theta)}{\partial H} dH d\lambda 
\]  

(1)

where \( B \) is the Planck radiance, \( \phi \) the effective spectral response of the instrument (extending between about 6.0 and 6.5 μm), \( T' \) the transmittance from level \( H \) to the satellite, \( \lambda \) the wavelength of radiation, \( H \) the vertical coordinate, and \( \Theta \) the zenith angle of radiation. Implicit in Eq. (1) is a knowledge of the temperature as a function of \( H \) for determining \( B \) and a knowledge of the temperature, pressure and water vapor concentration as a function of \( H \) for determining \( T' \). If one integrates over \( \lambda \) and considers only normally emerging radiation (i.e., \( \Theta = 0^\circ \)), Eq. (1) simplifies to
The bar in \( \bar{B} \) refers to the integration over \( \phi (\lambda) \), namely the quantity of radiation to which the water vapor channel responds between about 6.0 and 6.5 \( \mu \). \( \bar{T} \) is the effective normal transmittance of radiation to which the channel responds from level \( H \). Because the spectral width of the water vapor channel is quite narrow (about 0.5 \( \mu \) ), \( \bar{T} \) is essentially independent of \( \bar{B} \) in Eq. (2). (This would not be true for the broader 8 to 12 or 8 to 30 \( \mu \) channels.)

The nature of the weighting function, \( d\bar{T}/dH \), is shown in Fig. 1. On the left a standard atmosphere and two representative water vapor distributions are plotted. The long-dashed distribution results in two precipitable centimeters of water vapor in the atmosphere and the short-dashed distribution half that much. Because water vapor is a variable gas, one sees in this example that there are two weighting functions, depending upon the two water vapor distributions. For the wetter of the two atmospheres, the weighting function, shown by the long-dashed curve, peaks at about 350 mb. For the drier atmosphere the weighting function, shown by the short-dashed curve, drops about one kilometer, peaking near 400 mb. Obviously, from a consideration of Fig. 1, a satellite radiometer would "see" a colder temperature from the 2-precipitable-centimeter model than from the 1-precipitable-centimeter model.

One sees from the right-hand part of Fig. 1 that, for any reasonable amount of water vapor, the atmosphere is optically thick to the 6.3 \( \mu \) channel, meaning that radiation from the ground does not reach the satellite. Therefore, if there were no clouds, the equivalent blackbody temperature measured by the 6.3 \( \mu \) channel would be a good measure of the water vapor content of the atmosphere, provided some assumption were made as to the vertical distribution of water vapor (for example, that
the relative humidity were constant everywhere). However, there are clouds in the atmosphere and if a colder temperature is measured by the satellite over one location and a warmer temperature over another, the colder measurement could be either (1) caused by an increase in water vapor, thus pushing the weighting function upward into colder temperatures, or (2) due to the introduction of a cloud layer without any increase in water vapor above it. The cloud would have the effect of substituting an infinite isothermal layer of atmosphere having the cloud-top temperature, in place of the actual atmosphere with its normally increasing temperatures toward the ground. To resolve this ambiguity, one must invoke the use of data from the 8 to 12 μ window channel. The window channel measurement basically allows one to determine whether or not there is a cloud in the field of view and if so, what its top surface temperature is. And by a suitable interpretation of both the water vapor and window channel measurements, the mean relative humidity of the atmosphere can be inferred with certain assumptions. Professor Fritz Möller of the University of Munich was the pioneer in this method and has published a number of papers on it (1961, 1962) together with his student Dr. Raschke (1964).

Professor Möller’s thesis is illustrated in Fig. 2 where several model atmospheres are shown. Since the saturation vapor pressure of water vapor is a function of temperature alone and since the lapse rate of all atmospheres is approximately the same (about 6.5°K/km), it follows that the same water vapor mass exists above the same temperature in all atmospheres having the same relative humidity. The water vapor mass, u, above height, z, can be calculated by Eq. (a) in Fig. 2, where $e_s$ is the saturation water vapor pressure, $T$ the temperature, $r$ the relative humidity, $R^*$ the universal gas constant, and $m_w$ the molecular weight of water vapor. Thus in Fig. 2, Eq. (a) would calculate approximately the same water vapor mass above height "A" in the tropical atmosphere as it would above height "B" in the ICAO standard atmosphere for the same relative humidity. The reduced mass of water vapor, $u^*$, which is
required for the radiative transfer problem is approximated by Eq. (b) in Fig. 2 where \( p \) is the ambient pressure, \( p_o \) a standard pressure, and \( n \) an exponent. The term \((p/p_0)^n\) takes into account the pressure broadening of the spectral lines in the absorption band. (There is also a slight temperature dependence, not illustrated.) Because of the term \((p/p_0)^n\) the reduced masses of water vapor above heights A and B in the previous illustration are slightly different. But this is a small effect, and substantially one may say that the same optical depth exists in all atmospheres of equal relative humidity to that point in each atmosphere having the same temperature.

This concept is expressed in a Moller-type evaluation diagram shown in Fig. 3. Here the water vapor channel (dashed lines) and window channel (solid lines) measurements combine to determine tropospheric mean relative humidity along the ordinate and, as a by-product, the temperature of the surface of the earth or of clouds along the abscissa. One can see that in the region where the surface temperature is high, the window channel measurement makes no difference in the determination of relative humidity because the atmosphere is optically thick and surface radiation in the 6.0 to 6.5 \( \mu \) interval does not reach the satellite. However, as clouds are introduced, the window channel measurement becomes lower and lower until at surface temperatures below about 280\(^\circ\)K the dashed lines begin to depart from the horizontal, and measurements from the two channels must now be combined to determine the relative humidity. Again, because of Möller's thesis, an evaluation diagram for one atmosphere may be applied to a good approximation to all atmospheres.

We shall discuss briefly a synoptic study carried out by Allison and Warnecke (1965). In Fig. 4 the equivalent blackbody temperatures measured by the window channel of Tiros III on 16 July 1961 are shown. Areas where the equivalent blackbody temperatures were less than 270\(^\circ\)K are shaded. Also shown are cloud cover circles taken from conventional synoptic data. The situation involved a dissipating front from Ohio to Texas, with a pre-frontal squall line passing from Virginia to Louisiana,
a warm front extending laterally across Maryland, and a dying occlusion extending up to a low south of James Bay. A second frontal system was located along the U.S.-Canadian border and extended south along the coast of Washington and Oregon. Figure 5 shows the relative humidities derived from satellite data using the evaluation diagram in Fig. 3. Areas having relative humidities greater than 70% have been dark-shaded, and areas having relative humidities less than 40% have been light-shaded. Note that high relative humidities coincide with the frontal and pre-frontal squall line system. There is a dry tongue in Texas, corresponding to the lack of clouds that was in evidence in the window channel map. Also there are high relative humidities over the low in Canada and over the northwest frontal system, and there is a very dry center over Colorado and New Mexico. It is important to note that is relatively dry over Wisconsin.

Figure 6 shows the conventional analysis of relative humidity at 400 mb, and the agreement on a broad scale with Fig. 5 is quite good. Figure 7 shows the conventional analysis of mean relative humidity between 1000 and 500 mb. Here again, there is general agreement in moist and dry areas with the two previous figures, with a notable exception: the area over Wisconsin is quite moist. This figure pointedly illustrates that the satellite-derived relative humidity essentially pertains to the upper troposphere, as indicated by the weighting functions in Fig. 1, and that the region between the surface and 500 mb scarcely affects the satellite determination. Therefore, even though the lower atmosphere over Wisconsin is quite moist, the satellite-determined low relative humidity pertains to the upper troposphere over Wisconsin and, hence, should be expected to compare more favorably with the 400-mb data in Fig. 6.

Figure 8 shows the U.S. Weather Bureau conventional map of vertical motion centered at 650 mb. There is a rather good large-scale positive correlation between vertical motion and relative humidity (cf. Fig. 5). A qualitative estimate of the large-scale vertical motion field could
probably be made from Fig. 5 if the generalized relation between ascending motion, saturated air aloft, deep vertical cloudiness, and precipitation, on the one hand, and descending motion, dry air aloft, and suppressed vertical cloudiness, on the other, were used as a basis.

In addition to synoptic applications, global studies of water vapor distribution have been made (Bandeen, et al., 1965). In Fig. 9 quasi-global relative humidities determined from Tiros IV data and averaged over the period 11 to 18 April 1962 are shown. The dark shading pertains to relative humidities greater than 40% and the hatched gray refers to very low relative humidities of less than 10%. There are areas of high relative humidity over Indonesia, the central Pacific south of the equator, the Pacific west of Central America, South America, Equatorial Africa, and the eastern United States and Canada. However, in discussing relative humidities, one can be misled if he is interested in total water vapor content, because in the arctic a high relative humidity may be associated with a very low mass of water vapor, and, on the other hand, in the tropics a very low relative humidity may be associated with considerable water vapor mass. For this reason the Möller method of determining relative humidities has been extended to infer total water vapor mass above an arbitrary level, 500 mb. The 500-mb lower boundary was chosen for two reasons: (1) the weighting function of the water vapor channel, as seen in Fig. 1, is sensitive to radiation centered around 350 to 400 mb, viz., to radiation emanating from the atmosphere above 500 mb, and (2) we wished to compare our first satellite analysis on a global scale with a conventional analysis made by Bannon and Steele several years ago (1960).

Figure 10 illustrates the technique by which total water vapor mass above 500 mb is inferred. On the left, seven model atmospheres are plotted. On the right, there is a plot of tropospheric temperature (in the vicinity of 500 mb) vs. water vapor mass, u, above the indicated tropospheric temperature (for 100% relative humidities in all atmospheres). Saturation was taken with respect to liquid water for ambient
temperatures greater than \(-20^\circ\text{C}\) \((253.2^\circ\text{K})\) and with respect to ice for temperatures less than \(-20^\circ\text{C}\). The solid line indicates the water vapor mass above the corresponding temperature in the saturated 15°N mean annual model atmosphere. For example, from Fig. 10 the mass of water vapor above the 250°K level in the saturated tropical atmosphere is 0.10 gm cm\(^{-2}\). The curve consists virtually of two straight-line segments in this semi-logarithmic plot, inflecting at the 253.2°K transition point between liquid water and ice. Calculations of the water vapor mass above the 500-mb temperatures in all of the other atmospheres (assuming saturation) were made and plotted in Fig. 10. In every case the plotted point virtually lies on the solid curve drawn for the tropical atmosphere. Therefore, the solid curve was used to determine the water vapor mass above the 500-mb temperature level for all saturated atmospheres, and this quantity, multiplied by the satellite-derived relative humidity, yielded the actual water vapor mass above the 500-mb level. The 500-mb temperature field, required for this evaluation, was taken from conventional synoptic and, where necessary, climatological data.

Figure 11 shows the results of the water vapor mass analysis applied to the data in Fig. 9. Areas having a water vapor mass above 500 mb greater than 3 decigrams cm\(^{-2}\) are shaded in gray and areas having masses less than 0.3 decigrams cm\(^{-2}\) are hatched. There are three noticeable large concentrations of moisture: one over Indonesia, another over South America, and a third system of three pockets over Africa. Also, there are two other smaller moist regions: one over the mid-Pacific south of the equator and a second west of Central America. These two pockets correspond very closely to the mean position of the ITC at this time of year. Also, as expected, there is a marked decrease in water vapor mass at the higher latitudes in both hemispheres. Noticeably along 20°N latitude there are pockets of reduced moisture with moisture increasing to the north before decreasing again. This dry zone compares favorably with a marked maximum in long-wave emission from the northern hemisphere tropics for the same time of the year resulting from studies
Session VII of the planetary heat balance using Tiros VII data (Bandeen, et al.).

Figure 12 shows the average water vapor content above 500 mb during April of the years 1951 to 1955 as determined by Bannon and Steel from conventional data (1960). Bannon and Steel's work shows the same three major centers of moisture over Indonesia, South America, and Africa as did the satellite-derived data in the previous figure, but does not indicate the two other pockets of moisture in the Pacific Ocean or the dry zone near 20°N. Because of the total lack of conventional moisture data over much of the world, we feel that the satellite measurements promise to improve our knowledge of the spatial and seasonal distribution of atmospheric water vapor. With this goal in mind, we are now undertaking a program to analyze the quasi-global water vapor distribution from Tiros IV data over the entire lifetime of that satellite (four and one-half months).

DISCUSSION

Winston: At one point in showing one of your maps, I think you said that these were data derived from water vapor channels. Of course, they were not derived from water vapor channels only. And this brings up the big point I would like to question you about. Most of your weekly maps of what you showed of the water vapor naturally strongly resemble the maps you get from Channel 2 or the window radiation alone. I would really like to see how much additional information you are getting out of your water vapor channels.

Nordberg: May I answer this? I have not yet seen anybody who has given 3 decigrams per square centimeter or a half of a decigram per square centimeter of water vapor from a window channel map.

Winston: No, I do not really care about the actual amount. I think you could find ways of deriving the amounts of water vapor. But I am interested in maybe the relative humidity and the general distribution of high humidity and low humidity. There are lots of studies (Smagorinsky
made some ten years or so ago) that definitely related cloudiness to percentages of relative humidity in the troposphere.

Bandeen: If one compares the relative humidity map with either the Channel 2 map or the Channel 1 map, he finds a high negative correlation: high temperatures meaning low relative humidity, and vice versa. One could infer certain aspects of the water vapor content of the air merely by looking at one or the other map, Channel 1 or Channel 2 alone. However, I think there is certainly more information from both channels together than there is from either channel separately.

Winston: I think more from Channel 2 than from Channel 1.

Hanel: In high temperature ranges, you will recall from the third figure, the window channel data are not important at all, only the Channel 1 data. With very low temperatures, the curves for both channels are almost parallel, and you cannot use either one.

REFERENCES


Fig. 1  The weighting function $\frac{dT}{dH}$, as would be derived from Tiros IV 6.3 μ channel data, corresponding to two representative water vapor distributions.
Fig. 2. Schematic diagram illustrating that radiation of a given temperature is emitted from approximately the same optical depth in all atmospheres with the same relative humidity (after the method of Möller).
Fig. 3. Tiros III evaluation diagram. Equivalent blackbody temperatures (°K).
Channel 1 (6 to 6.5 μ) --- Channel 2 (8 to 12 μ)
Fig. 4. Channel 2 radiation analysis (TBB; °K) over the United States recorded during orbits 57, 58, and 60 on 16 July 1961, from 1000 GMT to 1500 GMT.
Fig. 5. Analysis of satellite-derived mean relative humidity (%) for orbits 57, 58, and 60 on 16 July 1961.
Fig. 6. Analysis of conventional 400-mb relative humidity (%) at 1200 GMT, 16 July 1961, over the United States.
Fig. 7. Analysis of conventional 1000- to 500-mb mean relative humidity (%) at 1200 GMT, 16 July 1961, over the United States.
Fig. 8. Analysis of vertical motion (650-mb level) in cm/sec at 1200 GMT, 16 July 1961.
Fig. 9. Mean tropospheric relative humidities determined from simultaneous 6.0 to 6.5 μ and 8 to 12 μ measurements by Tiros IV during the period 11-18 April 1962, in percent.
Fig. 10. Method of calculating total water vapor mass above 500 mb.
Fig. 11. Average water vapor content above 500 mb determined from simultaneous 6.0 to 6.5 μ and 8 to 12 μ measurements by TIROS IV and 500 mb temperatures from radiosonde data during the period 11 to 18 April 1962, in decigrams cm⁻².
Fig. 12. Average water vapor content above 500 mb, April 1951-55, in decigrams cm$^{-2}$. (After Bannon and Steele)
In the time at my disposal I would like to take up the question of how temperatures measured by the Tiros VII 15\(\mu\) band radiometer compare with those obtained from conventional radiosonde data. In particular, I wish to discuss the aspects of how temperature fluctuations match up, since it is easy enough to adjust absolute values. Another point of interest is to investigate the satellite's capability to follow systems.

Since my present particular interest is directed to the southern hemisphere, I will take up two case studies from that area. With the 15\(\mu\) band temperatures applying to heights of about 15 to 30 km an observation station has to be found with the capability of measuring radiosonde temperatures to these heights.

The first case discussed is that of Campbell Island, in the vicinity of New Zealand (53\(^{\circ}\)S, 169\(^{\circ}\)E). In addition to having good data, this station is well situated within the orbital measuring capabilities of the satellite. Figure 1 presents a time cross-section of R/S temperatures (in \(^{\circ}\)Celsius) for the pressure surfaces 100 to 10 mb, for the period 15 August to 15 November 1963. The full lines indicate intervals of 10\(^{\circ}\) and the dotted lines represent 5\(^{\circ}\) intervals. The satellite temperatures, averaged over periods of 7 to 8 days, are indicated at the top of the diagram. It should be noted that the overall period is from about the end of winter (August) to the beginning of summer (November).

*Leading discussant*
Figure 1 shows a very cold air incursion \((-70^\circ C\), presumably from the Antarctic, during late August; unfortunately, no satellite data were available at that time. From the end of August onwards there is a series of warm-cold-warm-cold cells all the way to the end of the period -- oddly enough, with an almost regular 7-day periodicity. This means that the satellite 7-day averages include one cold and one warm period. It is also important to observe that the satellite-measured temperatures very distinctly show the warmings and coolings for the layer under discussion.

A further point of interest is that maps displaying satellite data in the horizontal showed a warming trend in the mid-south Indian Ocean around 1 September. During the week or so following, this center of warming gradually moved eastwards and, just before reaching Campbell Island, veered off towards equatorial latitudes and died out. This warming trend is clearly indicated from about 12 to 25 September, after which the warm-cold regime is re-established until the final warming towards the end of October. Another notable fact shown in Fig. 1 is that the warmings and coolings generally occurred throughout the entire layer with very little slope in the isotherms. This is very favorable if one wants to use satellite data of this type to enhance the analyses. It is clear, however, that a 3- or 4-day average is to be preferred.

The second case discussed is that of Wilkes Station, one of the most equatorward observation stations \((66^0 S, 111^0 E)\) on Antarctica. This station has first-class equipment but, unfortunately, it is situated in the marginal poleward limit of the satellite observing capabilities.

Compared with the previous case, Fig. 2 shows glaring contrasts in many respects. Little, if anything, is seen of the 7-day periodicity, and temperatures range from -80 to \(-10^\circ C\). On the whole, the stratospheric circulation pattern appears to be different from the previous middle-latitude case.

At the 30-mb pressure surface, temperatures at the beginning of
the period are as low as -80°C and, with increase in time, rise by about 20°C. Temperature fluctuations are more intense at high altitudes. The satellite data also clearly indicate the temperature fluctuations shown by the radiosondes. Two periods of intense warming (-47°C and -41°C), separated by a cooling of -62°C, were accurately measured. In this case, the final warming (with the temperature leveling off to about -35°C) is clearly evident. It is interesting to note how the final warming penetrates to lower levels with increase in time.

In summarizing the two cases, it may be said with confidence that it has been shown that the satellite measurements obtained by the 15μ band radiometer are capable of detecting the stratospheric temperature oscillations and also of following their progress in time and space.

In conclusion, I would like to list under various headings a number of points which I have noted in using the satellite data and about which I would caution the analyst.

PROPERTIES OF A RADIATION- DERIVED LAYER MEAN TEMPERATURE

1. Large changes at the top or bottom of the layer will produce smaller changes in the layer mean.

2. The layer represented will tend towards the warmer (away from colder) temperatures due to the fact that the amount of radiation from a layer varies with the fourth power of the temperature.

3. At high satellite nadir angles a higher layer is represented.

4. Clouds at a very cold tropical tropopause level cut off radiation from below by several per cent of the CO₂ radiation sensed by the satellite. This effect should be relatively unimportant in extra-tropical regions where clouds do not build up higher than the 200-mb pressure surface.

5. Degradation of filters due to effects of high vacuum may allow water vapor radiation to enter the CO₂ channel filter in objectionable
FULL ORBIT INTERROGATION CAPABILITY IS ESSENTIAL

Geographical gaps now exist due to:
1. Lack of polar coverage.
2. Unfavorable modes of sensor sweep that (a) miss the earth entirely, (b) involve only high nadir angles (limb darkening), and (c) keep the earth in constant view and thus lack intermittent calibration.
3. Lack of read-out stations in South America and Asia.
4. Outages during interrogation.
5. Slipping data tapes.

ADVANTAGES OF SATELLITE MEASUREMENTS OF STRATOSPHERIC TEMPERATURES

1. Vast stationless areas are observed in detail.
2. Mean layer temperature permits build up of circulation pattern.
3. Sudden warming effects can be detected in early stages and followed.
4. Thermal aspects of seasonal reversals of circulation can be followed in both hemispheres.
5. Equatorial temperature changes associated with the 26-month cycle are most important in the layer sounded by means of the CO₂ channel. In this respect, however, there will be the problem of compatibility of data for different years and different satellites.

ADVANTAGES OF THE NIMBUS SATELLITE SYSTEM

1. Polar orbit providing (a) complete global coverage, especially the polar regions where, for the stratosphere, most of the interest is vested, (b) it is within range for interrogation of each orbit, and (c) daily maps will allow the tracing of transient systems.

*Objected to by W. Norberg
2. The earth orientation.
3. New and better equipment.

OPTIMUM APPLICATION OF DATA

1. Comparisons with rawinsonde data to investigate (a) level represented and its variation, (b) temperature bias, and (c) time and space scale of systems represented.

2. Development of refining corrections: (a) possible use of window channel to detect high cloud areas and correct for their effect, and (b) use of 50-mb temperature charts (or 50-to 100-mb layer mean temperatures) to correct satellite values.
Fig. 1. Time cross-section of R/S temperatures (in °C) for the pressure surfaces 100 to 10 mb, for the period 15 August to 15 November 1963.
Fig. 2. Time cross-section of R/S temperatures (in °C) for the pressure surfaces 100 to 5 mb, for the period 15 August to 15 November 1963.
SESSION VIII

RADIATION PHYSICS
The meteorological satellite has been characterized as a radiation meteorologist's dream, and indeed it is. In contrast to the ground and lower atmosphere, where a multitude of sensors permits us to probe the environment, the satellite has access only to the electromagnetic radiation arising from atmosphere-scattered sunlight, and to the thermal or self-emission of the earth-atmosphere system in the far infrared.

Let us now perform the following experiment. Imagine yourself a newly-arrived meteorologist from Mars, familiar with the adiabatic lapse rate, hydrostatic equation, perfect gas law, etc. Imagine further that instead of re-entry, you have decided to orbit, say, at 500 km. What could you tell about the atmosphere by indirect inferences solely from radiometry? And, I might add, it is only by pushing to extremes the inference possibilities of remote radiometry that we can sense the limits of the technique.

Because of the strong coupling of the temperature to the far infrared emission through Planck's intensity law, the temperature structure is perhaps the easiest to infer. It appears then also that this problem has been solved: namely, that the vertical thermal structure of the atmosphere can be determined by remote radiation soundings alone. Until a year ago there was a question whether this could be done even with observations of good quality.

What is really surprising is that the problem has been solved by two quite different methods, one a refinement of a linear technique and the
second a non-linear method. In 1956 the suggestion was made that the thermal structure of the atmosphere could be determined from the variation of the upwelling intensity as a meteorological satellite scanned from nadir to limb. This is the so-called limb-darkening effect, long familiar to solar astronomers who have used it to roughly determine photosphere temperature structure. Kaplan (1959) suggested a similar technique in an atmospheric absorption band for the purpose of determining the vertical thermal structure of the atmosphere. This idea formed the basis for the design of a multi-channel high resolution radiometer developed by ESSA and to be flown on the Nimbus satellite. This instrument, which consists of some seven channels scattered across the 15μ carbon dioxide band, was successfully test-flown on a balloon platform in September 1964 at the NCAR facility at Palestine, Texas. Data obtained on that flight were of good quality and as such provided the first worthwhile test of "inversion" procedures.

The upper portion of Fig. 1 shows schematically the distribution of carbon dioxide as a function of wave number. Looking down at the wing near the window, the eye will intercept photons near the surface giving information of the surface temperature. Going towards the opaque center band, one finally reaches the point at the Q branch in the center where, because of the high opacity, almost all photons are rising from high in the atmosphere.

The mathematical relationship connecting the intensity I sensed by the satellite and Planck's intensity B(u), and the final equation to be used, are presented in the lower part of the figure. This is a solution of the radiative transfer equation and standard notations are used. Given measurements of the upwelling intensity I in different channels having known absorption coefficients, the problem thus becomes one of "inverting" this equation and subsequently solving for the slope, i.e., the temperature distribution with depth.

The problem itself can be viewed in several ways. It may be considered as a problem of solving either an integral equation inverting a LaPlace transform, or constructing a quadrature formula, or matching up movements of this function, or constructing an interpolation formula. All these interpretations are identical; just different ways of viewing the same mathematical problem.

However, two difficulties are encountered. First, there is the question of the finite channel number, which generally is less than ten channels. This provides, instead of a unique solution, the possibility of an infinity of solutions, because any temperature profile, which upon averaging passes through these end points, is an equally valid mathematical solution. Most of these solutions are, however, completely inadmissible, being oscillatory, etc. The problem therefore becomes one of discriminating among a variety of possible solutions.

The second difficulty is not obvious, but is the more serious of the two and concerns the effect of noise. Any noise in the data that is of upwelling intensity, upon inversion is invariably magnified into a lapse rate that can be described only as a meteorological monstrosity. This is because the process of inversion is akin to numerical differentiation, so that the slightest bit of noise in the original smeared-out data, upon inversion, becomes amplified wildly into a completely non-physical solution.

The usual procedure is that of linearizing the set of equations by forming a linear simultaneous matrix set. We have to solve

\[ \Delta I(0, K_i) = \int_0^\infty \frac{dB(u)}{du} e^{K_i u} du \]

Assuming a polynomial set

\[ \frac{dB}{du} = \sum_{j=1}^N b_j P_j(u) \]
we obtain by substitution into the equation

$$\Delta I(\theta, k) = \sum_{j=1}^{N} b_j P_j(k)$$

where \(P_j(k)\) is the Laplace transform known for each of the polynomial terms. Inverting this by routine computational methods, \(b_j\) can be found.

The disadvantages of the uncritical linear method make it completely unsuitable for inversion. One of the reasons is that there is no criterion for the sensing channels. Another, and by far the most deleterious property is its extreme sensitivity to noise. This rules out the straight application of the linear methods. It is, however, possible by a refinement of linear methods, which has been developed by Wark, Twomey and Fleming (1966),* to surmount these disadvantages and to infer the temperature profile.

Conscious of this subjective element involved in the proper choice of a polynomial set, we sought a method in which the data itself would determine the fitting of the polynomials. Commencing with the same equation

$$\Delta I(\theta, k) = \int_0^\infty \frac{dB(u)}{du} e^{k_i u} du$$

the integral is replaced by its appropriate sum

$$\Delta I(\theta, k) = \sum_{j=1}^{N} \Delta B_j e^{-k_i u_j} = \sum_{j=1}^{N} \Delta B_j x_j k_i / k_{\text{min}}$$

where the substitution $x_j = e^{-\kappa_{\min} u_j}$ has been made. The above procedure is nothing else but the reverse application of the fundamental theorem of integral calculus. In other words, we are trying to construct a quadrature formula to fit the integral. This equation now has a unique solution for $\kappa_i / \kappa_{\min} = 0, 1, 2 \ldots 2N-1$, where the $x_j$'s are the roots of the orthogonal polynomial set. The advantages of the above method are that (1) the data generate a unique polynomial set, (2) it has a rationale for determining the channels, i.e., the spacing we should view in order to obtain this unique solution, and (3) it is insensitive to noise.

Figure 2 provides a test of the approach by taking a synthetic atmosphere obeying the simple law

$$\beta(w) = 1 - e^{-\frac{w}{w_0}}$$

and using the ten intensities which formed for this simple model the sequence

1, 1/2, 1/3, 1/4, ......... 1/9

with computational accuracy to eight places to infer five thickness slabs and five widths. It will be observed that the shape of the profile has been very faithfully reproduced and also that the slab thickness is smallest in the region of the greatest slope, thus minimizing the "cornering" error. That this is not an altogether trivial result is shown in Table 1, which represents the results of using a linear method in which 100-mb intervals were chosen and coupled with the ten intensities used previously. As can be seen, the inferred thermal structure for this model is completely unrealistic.
In view of this encouraging result, it was of interest to determine the response to this algorithm by purposely degrading (i.e., adding noise to) the data. This was simply done by rounding off the intensities to four-digit accuracy (Fig. 3 shows the configuration). The remarkable thing that happened in this case was that instead of obtaining five roots from the fifth degree polynomial, i.e., five slabs, only four slabs were obtained, the remaining slab being of value -1. The latter is an inadmissible root and has no physical interpretation in terms of the thermal slope. Although it may not be immediately obvious, it indicates that the algorithm was, in fact, in a nice way responding to noise, since the weight associated with this root is of the order of $10^{-3}$.

Therefore, as a result of introducing noise into the data, it is possible to infer only four slabs instead of five, and consequently some resolution is lost. However, it is lost in a harmless way, namely by one of the roots becoming inadmissible but absorbing the noise in the data, and, in association with its small weight, causing a minor convection and essentially leaving the inferred profile unimpaired.

Figure 4 shows the case for rounding off to three-digit accuracy. Here, two inadmissible roots are obtained, one a negative root and the other extending a foot into negative space. Again, the weights associated with these roots are negligible and, consequently, the thermal pro-
file is again faithfully reproduced.

Rounding off to two-digit accuracy introduced an added novelty of producing paired complex roots in addition to a root going into negative space. Again, despite this, a surprisingly faithful reproduction of the trend of the profile is obtained (as shown in Fig. 5).

Since synthetic models, as described above, may be subject to suspicion because of the danger of subconsciously selecting the answers, an actual case (from a balloon flight at Palestine, Texas on the morning of 11 September 1964) was used to put the algorithm method to test. The data furnished consisted solely of intensity readings in the various channels of the radiometer. The actual temperature profile which was obtained simultaneously was purposely withheld in order not to prejudice the application of the method. Since the thermal profile of the atmosphere is more ramp-like than step-like, the word "ramp" has been substituted for "slab" in the case of the atmosphere.

Without smoothing the data, it was applied to the algorithm to producing the roots \( x_j \) and weights \( \Delta B_j \) as indicated in Fig. 6. Once again, it is to be noticed that one of the weights or one of the roots is bad, indicating the presence of noise. Fortunately, the weight associated with the bad root is quite small, resulting in the thermal profile as shown. Figure 7 depicts the same profile in a more meteorological coordinate system. The dots represent the five intensity data points used to perform the inversion.

A temperature versus pressure plot is shown in Fig. 8 where the solid line is the ramp solution and the dots indicate the actual temperature measurements. On the whole, it may be conceded that a faithful reproduction of thermal atmospheric structure is given.

One of the main discrepancies of the algorithm is a tropopause temperature \( 10^0 \) too low at a pressure which is about 20 mb too high. This may be due to a number of factors, and the prime suspect is simply a scaling error in the model used to represent the transmittance.
An extension of the atmospheric case was provided by the observations at noon on the same day. At that time, a cirrus cloud deck obligingly formed as a result of convective activity. This meant that the radiometer looked down on the deck and one would naturally ask if it were possible to infer the height and temperature of the clouds.

The result of applying this set of data to the non-linear algorithm is illustrated in Fig. 9, from which it is immediately apparent that the profiles present an abrupt change from the morning case. At one part of the profile two values are indicated, showing that the Q-branch channel was in error and that it was necessary to make some assumptions about the temperature at the 100,000-ft level. However, comfortingly enough, varying the assumptions at the top of the atmosphere does not make a great deal of difference to the inferred profile. In this case, there are again two good roots, one root attributed to noise and the negative root again associated with a very low order of magnitude weight factor.

The temperature-pressure plot is shown in Fig. 10. The inversion algorithm has provided the unambiguous interpretation that below the cloud deck it is looking down on an isothermal layer, which (since an opaque surface is the equivalent to an infinite isothermal medium) also indicates the height of the top of the cloud at the point of discontinuity. It is of interest to observe that the indicated cloud height deviates from the actual cloud height in a systematic fashion in the same direction as the tropopause, indicating that this is a scaling error.

In conclusion, it may be summarized that the cases shown indicate that inversion is possible and that it should be performed routinely once the Nimbus-C satellite with its multi-channel radiometer is in orbit. In addition, it will also be possible by this method to infer flux divergences and, in principle, one can infer anything that is coupled with the radiation field.

Note: The second part of Dr. King's presentation, dealing with the non-linear inversion technique to the problem of microscopic radiative transfer, is for technical reasons not reproduced here.
**Inversion**

$$I(0, \mu) = \int_{0}^{\infty} B(u) e^{-\mu u} \, du$$

$$I(0, \mu) - B(0) = \int_{0}^{\infty} \frac{dB}{du} e^{-\mu u} \, du$$

**Problem:**

$$\Delta I(0, \mu_i) = \int_{0}^{\infty} \frac{dB(u)}{du} e^{-\mu_i u} \, du \quad (i = 1, 2, \ldots, N)$$

**Difficulties:**

1. Finite channel no. ⇒ Infinitude of solns.
2. Noise ⇒ Meteorological monstrosities

---

**Fig. 1**
\[ B(u) = 1 - \exp\left(-\frac{u}{u_o}\right) \]

Fig. 2
$B(u) = 1 - \exp\left(-u/u_0\right)$

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Fig. 3
The graph represents the function

\[ B(u) = 1 - \exp\left(-\frac{u}{u_0}\right) \]

and includes the following data points:

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<th>( x_j )</th>
<th>( u_j / u_0 )</th>
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<tr>
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Fig. 4
$B(u) = 1 - \exp\left(-\frac{u}{u_0}\right)$

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<tr>
<td>5.1 + 0.0004i</td>
<td>0.979 - 1.8i</td>
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Fig. 5
AM RAMP SOLUTION
11 SEPTEMBER 1964
PALESTINE, TEXAS

PLANCK INTENSITY

WING OPTICAL DEPTH

Fig. 6
AM RAMP SOLUTION
11 SEPTEMBER 1964
PALESTINE, TEXAS

Fig. 8
### Noon Ramp Solution
#### September 1964

**Palestine, Texas**

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**Planck Intensity**

**Wing Optical Depth**

**Fig. 9**
Session VIII

Fig. 10

NOON RAMP SOLUTION
11 SEPTEMBER 1964
PALESTINE, TEXAS
INVERSION PROBLEMS

David Wark*
National Weather Satellite Center, ESSA

Our approach to the inversion problem has an entirely different philosophy from that of Dr. King. Originally, we have a series of measured values, determined from calibration of the instrument and its output. The measured values of radiance are a function of a number of variables. However, we are only interested in the variable $f$ (frequency). If we are looking through an infinite atmosphere, the relation between the measured radiances and the optical and thermodynamic properties of the atmosphere can be written

$$I(f) = \int_{t_0}^{t_{oo}} B \left[ T(t_0, t), T(t) \right] \frac{d\tau(f, t)}{dt} dt$$

where $\nu_c$ is the central frequency in an interval, $B$ is the Planck radiance, $T$ is temperature as a function of the independent variable $t$ (in our work this is usually the logarithm of the pressure) and $\tau$ is fractional transmittance in the interval from level $t$ to the satellite ($t_o$). Strictly speaking, this is an identity, rather than an equation.

Equation (1) simply states that the observed radiance is the integral of the Planck radiance at different atmospheric levels, $t$, weighted by the derivative of the transmittance. The weighting function in a spectral is determined by the instrumental characteristics (spectrometer slit

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function or a filter function) and the detailed transmittance of the continuous variable $\phi$.

The term on the left is discrete, whereas the individual terms on the right are continuous functions of the variable $t$. The solution for the unknown Planck radiance cannot be described analytically, or even uniquely, in view of the finite number of measured radiances. Thus the problem becomes one of numerical analysis, rather than of mathematical analysis.

The equation must be linearized in order to achieve a solution. This is done by the relation

$$\beta(\nu_x) = \alpha(\nu_c) \beta(\nu_x) + \beta(\nu_c)$$

where $\nu_c$ is a reference frequency within the relatively narrow frequency range of the 15 $\mu$ carbon dioxide band. The $\alpha$'s and $\beta$'s are evaluated at 270 and 2300 K.

Now Eq. (1) can be written

$$g(\nu) = \frac{\pi(\nu) - \beta(\nu)}{\alpha(\nu)} = \int_{\nu_c}^{\nu} \beta(\nu, t) \frac{d\tau(\nu, t)}{dt} dt$$

or

$$g(\nu, \nu_c) = \int f(\nu) K(\nu, \nu_c) d\nu$$

where $i = 1, \ldots, N$.

Given $N$ measurements of $g$, one can determine $N$ quantities in the indicial function; this function is not analytic because $g(y_i)$ is not analytic, but is only a set of numbers. Thus, we are not in the realm of pure
The solution of the set of linear equations (Eq. 4) is inherently unstable because of the large degree of interdependence of the components of the kernel function \( K(x, y_i) \), as shown in Fig. 1. These are six weighting functions for the spectral intervals employed in the instrument which has been flown from a balloon, although Fig. 1 would be for the same instrument in a satellite. The upper curve is in the Q-branch at 669 cm\(^{-1}\), and the remaining five are in the R-branch out to 709 cm\(^{-1}\).

It is obvious that there can be no unique solution for a temperature sounding when only six radiances are measured. One must therefore approximate the indicial function, either as six points distributed over the range \( a \) to \( b \), or as a series with coefficients.

Figure 2 shows the results from a balloon flight test in September 1964 at Palestine, Texas. The dotted line is the sounding made during ascent, and the other two curves are two of the infinity of possible solutions. In one case we used a sine series, but because we had only five good measurements because of electronic troubles with the Q-branch channel, we had only three terms to the sine series. It obviously cannot make the sharp bend at the tropopause. So we cheated a little bit and we said, let's put the tropopause where we know it is, at 98 mb, and instead solve for five points and connect them with line segments. Well, we get a pretty solution when we do that. In the case shown in Fig. 3, when the balloon passed over a cumulonimbus, we assumed that we did know where the tropopause was. The sine series was used to solve for the inflection point, and, indeed, we came out at exactly the same point, which is almost too good to believe; the solution was then made for a series of line segments. When we were looking down on the cloud, which severely affected certain channels which "saw" down into the cloud, three of the six channels were almost unaffected because the weighting functions were nearly zero there. There was a small change in the 677.5 cm\(^{-1}\) channel, which affected the deduced sounding, which I now think may have been partly real. The temperature was slightly different.
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over the cumulonimbus area from that in the area of the early morning ascent. There are still some problems to be straightened out to get the solution for the cloudy case to be as good as the clear case. It is probably not quite so good, but it is fairly good, and may indeed be an accurate sounding.

One of the problems that remains with an instrument of this type is to know how many pieces of independent information one can get. This depends upon the conditioning of the matrix set up from Eq. (3). When this is inverted, using a smoothing technique, the contribution by the last terms of a series solution can be very small, which means that there is no significant contribution made by having so many measurements. However, there is some redundancy. Therefore, we will probably wind up doing a type of least squares solution, in which we will use our six (now seven) pieces of information and probably get four or five coefficients of the series expansion of the indicial function.

Recently we have been trying empirical orthogonal functions, based upon radiosonde observations, in the solution of the inversion problem. These have an obvious advantage over analytic functions because they represent the physical nature of the atmosphere. This type of series representation converges rapidly, so that one can usually employ about four functions, solve for their coefficients, and obtain temperature soundings superior to those employing other functions.

An entirely different problem involves only absorption by the atmosphere. The forbidden oxygen "A" band shown in Fig. 4 absorbs sunlight reflected from cloud tops. The amount of absorption is a measure of the optical path and therefore of the pressure height of the cloud. The band has a head at 7593 Å, a strongly absorbing R-branch between there and the band center at 7619 Å, and a less absorbing P-branch at greater wavelengths. The Mt. Wilson solar telescope was used for the spectra in this figure; the resolution was about 0.05 Å (one can distinguish lines in the original plates which are not even listed in the Charlotte Moore catalogue).
The instrument used to test this principle is a small hand-held spectrograph with a resolution of 5 Å. It was carried in the Gemini-5 manned spaceflight, from which we obtained a large number of fine spectra. Figure 5 shows a typical observation with the photograph of the observed area below and its spectrum displayed above. Figure 6 shows a photograph of the instrument; a Leica camera back was attached to the main body of the compact spectrograph-camera.

Figure 7 shows densitometer traces for three of the Gemini-5 observations: low stratus; middle clouds in the ITC; and the top of hurricane "Doreen." In these, one can barely see that the pairs of lines in the P-branch are resolved.

By measuring the absorptance of the atmosphere at the peak of either branch, one can, from the geometry of the sun and the observer, deduce the cloud-top heights. The region around 7632 Å is used for lower clouds, and that at 7607 Å for higher clouds. In Fig. 8 are shown the absorptances at the two wavelengths and two angles from the local vertical (θ is the solar angle and φ is the observer's angle). It is apparent from this figure that the two wavelengths are needed to cover the range from the surface to 100 mb. The ability to distinguish the height of a cloud depends upon the noise level of the observation and the slope of the absorptance curve. If, for example, the errors are 4%, one can discriminate to better than 1 km in the cloud-top height if the absorptance is between 31 and 93% (the limits shown by the vertical lines in the figure). With 1% accuracy one can discriminate to 250 m between these absorptance limits.

Unfortunately, as everyone knows, the light does not reflect off the cloud top but it scatters through the cloud and then finally comes out the top. So we now have a program to calculate the added path length in typical clouds. One must then make a correction, because you get a false cloud height from the observations. This correction is anything but trivial, amounting to about 100 to 150 mb for very thick clouds and as little as about 30 mb for thin clouds such as stratus. But one
does not and cannot get a unique answer because the exact nature of the cloud is unknown. However, results so far have shown that assumptions on cloud-droplet size, size distribution, and density lead to reasonable results.
Fig. 1 Sine weighting functions for the spectral intervals used in the instrument flown from a balloon.
Fig. 2 Temperature profile of a balloon flight test at Palestine, Texas in September 1964. The dotted line is the sounding made during the ascent.
Fig. 3 Similar to Fig. 2, but with balloon passing over cumulonimbus cloud.
Fig. 4  The spectrum of the oxygen "A" band.
Fig. 5 Photograph taken from Gemini-5 with a hand-held spectrograph. The lower part is a photograph of the observed area; the upper part is the corresponding spectrum.
Fig. 6 Instrument used to obtain Fig. 5.
Fig. 7  Densitometer traces taken during the Gemini-5 flight for low stratus (upper), middle cloud in ITC (middle), and top of hurricane "Doreen" (lower).
Fig. 8 Profiles of absorptance versus pressure at two wavelengths and two angles from local vertical.
PROBLEMS RELATED TO THE USE OF
SATELLITE OBSERVATIONS

Julius London*
University of Colorado

It has been amply demonstrated during the course of these discus-
sions that satellite observations can be used to complement dynamic
modeling techniques in the general understanding and prediction of var-
ious scales of atmospheric motion. It has also been shown that differ-
ent types of satellite observations, when used together, could aid in a
more complete analysis of the structure of the atmosphere. A number of
papers dealt with information derived from one set of observations or
another, e.g., the carbon dioxide experiment of Tiros VII, the water
vapor 6.3μ results that were reported by Bandeen, the inversion tech-
niques just discussed by both Wark and King, etc. The results of each
experiment give us a little different type of atmospheric information.
Observations of upwelling radiation in the transparent region (8 to 12μ)
give us information about the temperature of the underlying emitting
surface, ground or cloud. Observations in the 15μ band give us inform-
ation about the mean stratospheric temperature in the layer from about
20 to 30 km. In addition, observations in the 6.3μ band give us some
idea about the water vapor content in the upper troposphere. This in-
formation could be used as feedback to further correct the 15μ band
where there is an overlap between carbon dioxide and water vapor absorp-
tion. Thus, in using the existing observations, one can get the most
amount of information by judiciously combining the observations rather
than simply looking at each one by itself.

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It is well known that thermal emission from the atmosphere is degraded as a result of high-level absorbing and emitting sources. Although these high-level emitting sources are not always visible, they could represent significant parameters for the thermal budget of the stratosphere. One of the present day problems, therefore, in the use of satellite information, is development of the ability to discriminate between aerosol layers and cirrus clouds in the upper troposphere and lower stratosphere, and to understand the different emission properties of these radiating sources.

One of the most important single satellite observations, certainly, is the observation of the average total solar radiation received at the top of the atmosphere, the solar constant, and in particular, the spectral distribution of this extra-terrestrial solar radiation. As far as I know, there is still no flight time assigned to this important observation, although it would provide basic meteorological information. It is necessary that the entire spectrum of solar radiation received at the top of the atmosphere and time variations of that spectrum be carefully determined. Because of the direct role that solar ultraviolet radiation plays in the photochemistry and heat budget of the upper atmosphere, it is of particular importance to observe the ultraviolet spectrum. Absolute intensity measurements as well as possible time variations could indicate the extent to which one might expect the upper stratosphere and mesosphere to react to various manifestations of solar activity. Solar radiation in the region of the spectrum from about 1800 Å up to about 3000 Å is rather important for the energy budget of the stratosphere, and radiation in the region of the spectrum from about 1500 Å up to 3000 Å is important for the energy budget of the entire mesosphere.

We know from rocket observations that the energy of the solar spectrum in the ultraviolet region departs from a 5000K blackbody curve at around 2100 Å. Strong absorption and photodissociation by O\textsubscript{2} takes place below 2100 Å, whereas strong absorption by O\textsubscript{3} occurs in the
Hartley bands just beyond 2100 Å. Accurate observations of the solar spectrum right in this portion of the spectrum are needed to study the photochemistry and budget for the mesosphere and lower thermosphere. A number of satellite experiments have recently been designed for this purpose and I hope they will provide us with the desired information within a reasonable amount of time.

Finally, I would like to comment on a problem of application of radiative transfer theory. This problem came up in a previous discussion in terms of the type of observations that are necessary for studies of the dynamics of the atmosphere. As recently discussed by Manabe and Strickler, the relaxation time in approaching a radiative equilibrium temperature distribution in the troposphere and lower stratosphere is of the order of about thirty days. We can show, however, that small-scale perturbations to the mean vertical temperature distribution are radiatively damped at a faster rate depending on the height and vertical wavelength of the initial perturbation. This information indicates the minimum period of time radiative changes in the atmosphere are significant. If the wavelength of the temperature pulse is very small (of the order of a few hundred meters) the damping takes place rapidly, but if the wavelength is large (of the order of 1 to 10 km) the damping takes place rather slowly. In the lower troposphere, with the perturbation wave length 1 km, the relaxation time is of the order of one day. In the upper troposphere and lower stratosphere with a wavelength of 10 km, the relaxation time is of the order of about 12 to 15 days. The normal temperature oscillations, such as often develop in mid-troposphere, would have a relaxation time of the order of a few days. The radiative relaxation time depends inversely on the local gas concentration and therefore tends to increase with increasing height.
DISCUSSION

**Hanel:** How important are convective effects?

**London:** In the stratosphere, if one assumes a simple form for the diffusion equation, then a value of the exchange coefficient that one needs in order to get the equivalent of radiative damping would be 

\[ K_z = 10^5 \text{ cm}^2/\text{sec}, \]

indicating that radiation is certainly at least as important, if not more so, than turbulent mixing at these heights.

**Neiburger:** I need some clarification concerning the physical meaning of the radiative relaxation time.

**London:** If we assume a mean temperature distribution in the atmosphere upon which we superimpose some sort of temperature pulse so that we have a difference between the mean and observed temperature distribution, we want to know how long it will take for the disturbed temperature to return to the mean distribution if we consider only radiative processes. This time interval is called the radiative relaxation time.

**Neiburger:** What is the process that brings the observed temperature distribution back to the normal or mean distribution?

**London:** There are, of course, many processes, but if we consider just radiation, then it is the radiative exchange between the layer in question and the atmospheric layers below or above it.

**Singer:** I suppose the reason the stratosphere relaxes more slowly is because of the lower water vapor mixing ratio.

**London:** Actually, in the stratosphere, it is the carbon dioxide that plays the more important role in radiative damping. It turns out that ozone is relatively unimportant in this region of the atmosphere. Water vapor is the most important in the troposphere.

**Dütsch:** I would like to put some emphasis on what was said about the satellite experiment in the ultraviolet region of the spectrum. Observations of ozone by Umkehr technique for the top level which is in more or less equilibrium indicate more and more that there are relatively slow but large variations, possibly of the order of 20%, which seem to go with the solar cycle.
Wark: About this measurement in the ultraviolet — were you planning to extend the measurements into the visible region, beyond 3000 Å?
London: No, just to 3000 Å.
Wark: That is good enough, although I would prefer 3200 Å.
London: We plan to use the measurement at 3000 Å as a calibration check, so we will not be able to measure variations at 3000 Å.
I hope that I will be able to say a few words about the aerosol and dust in the atmosphere, but the main topic of my talk is inversion methods in the ultraviolet and visible regions. The use of the visible and adjacent regions offers three basic advantages. First, this is the region where the sun's radiation has a broad maximum and thus the emergent radiation from the atmosphere has sufficient energy for a sophisticated measuring technique. Secondly, this is a region where little or no absorption occurs; hence the scattering can be considered as the only physical process on which the inversion can be based. Finally, this is a region where we can obtain some information about the content of the particulate matter in the atmosphere from the relative magnitude of molecular and aerosol scattering. At the short-wave end of this region we have predominantly molecular scattering, while at the long-wave end, aerosol scattering predominates.

The radiation emerging from the atmosphere, the measurement of which is to be used for the inversion, results from the atmospheric scattering of the sun's radiation. Therefore, the inversion method will give us the elements that cause this physical process. The inversion will not give us any information about the temperature distribution in the atmosphere, for example, because the scattering is completely independent of the temperature itself. The element on which the scattering depends very critically is the number of scattering particles. If we
can consider only the molecules as scattering particles, the quantity that can be obtained from the inversion will depend on, and thus give some indication of, the pressure at the lower boundary of the atmosphere. If we have a considerable amount of particulate matter in the atmosphere, then, since the particulate matter scatters the light quite differently than the molecules, the measurements, and finally the inversion, will give quite different values for the basic quantities. From the difference between the measured values and the theoretical ones, valid for a molecular atmosphere, we expect to get some idea about the aerosol content in the atmosphere and eventually about the location of the aerosol concentration.

The air molecules and a large number of aerosol particles are of sizes much smaller than the radiation wavelengths, and therefore the scattering produces considerable polarization. Thus we have the possibility of determining not only the intensity (the radiant energy flux) but also the so-called polarization parameters. By using polarization parameters for the inversion we can gain three times more information than we would if the scattering did not produce polarization, or if our measurement and inversion were restricted to the intensity of the scattered light only. The idea of using the polarization of the light scattered by the atmosphere to determine the aerosol content of the atmosphere, is 150 years old. The use of radiation emerging upward from the atmosphere for this purpose is, of course, very recent. Since Coulson (1959) made the first computation of the polarization characteristics of the light emerging upward for the molecular atmosphere in 1956-1957, most of the studies of polarization characteristics have been concerned with their determination for different optical thicknesses and ground reflections. In all these studies it was always assumed that the optical thickness and ground reflection laws are known. Nowadays we are concerned with the inversion problem, i.e., we assume that we know all the optical characteristics of the emerging radiation, and we want to determine the optical thickness and the ground reflection
characteristics from the measurement. Unfortunately, ground reflection plays a very important role in this problem, even for a thick atmosphere.

Because we have only recently begun to investigate the problem of inversion in the visible and adjacent regions, I am able to give only a progress report and some ideas of what we intend to do, rather than to discuss results. I will omit completely the experimental details of the inversion method and assume that we know the intensity of the emerging radiation and its polarization in any direction of the viewing solid angle, as well as the direction of the sun's radiation at the point of observation.

In previous studies of skylight polarization, the polarization was -- for simplicity -- always studied in a particular plane: namely, the vertical plane containing the direction of the sun's radiation (the so-called "sun's vertical"). The quantity measured was the degree of polarization -- the ratio of the intensity of the polarized component to the total intensity. If we measure the degree of polarization of the light emerging upward from the atmosphere, we find that in the sun's vertical there is a point of maximum polarization close to 90° from the sun. We also find two "neutral points" where the polarization disappears. For a pure molecular atmosphere without any particulate matter, the measurements of the position of the neutral points and of the maximum degree of polarization will be sufficient to determine the optical thickness (τ) and the reflectivity of the ground, provided the ground reflection is governed by Lambert's law of isotropic and unpolarized reflection. This is apparent from Figs. 1 and 2 where the angular distance of the neutral points* from the antisun (Fig. 1) and the degree of polarization in the nadir (Fig. 2) are plotted as functions of the optical thickness for different zenith distances of the sun (Zθ).

* The neutral points are named in a complete analogy to the neutral points of skylight (Sekera, 1957): the point above the antisun is called the Brewster point; that below the antisun, the Babinet point; and the point in the opposite part of the sun's vertical, the Arago point.
It can be seen that from the measured position of the neutral points we can determine the optical thickness, although for larger values of optical thickness ($\tau > 0.8$) the accuracy of such determinations is decreased, or eventually there are two values of the optical thickness for which the neutral points have the same position with respect to the antisun. This ambiguity can be partially resolved from the measured values of the degree of polarization in the nadir in Fig. 2. The striking feature of this figure is the dependence of the degree of polarization on ground reflectivity. Knowing the optical thickness from the position of the neutral point, we can, for a given zenith distance of the sun, determine the reflectivity $A$. For low reflectivity ($A \rightarrow 0$), however, the curves are flat and therefore the determination of the optical thickness from the measured degree of polarization in nadir is not accurate, especially for low zenith distance of the sun.

Although the measurements of the position of neutral points and, for example, of the degree of polarization in the nadir, could be used to determine optical thickness and ground reflectivity in a pure molecular atmosphere, this method fails if the atmosphere contains particulate matter, dust or haze. It may be possible to find out how the neutral points are shifted and the curves in Fig. 2 modified for different types and size distributions of aerosol particles, but there will not be enough measured quantities to resolve the increased number of ambiguities in the method. It is therefore necessary to modify the method of observation, and the clue to this modification can be obtained from the theory of radiative transfer. From the latter theory we know that, even for a very general type of scattering, the Stokes polarization parameters can be expanded into harmonic series of the azimuth difference between the planes containing the direction of observation (given by the parameter $\mu = \cos \theta$, and the azimuth $\phi$, $\theta$ being the zenith distance) and the direction of the sun's radiation ($\mu_0$, $\phi_0$). Thus we have for the intensity component parallel to the vertical plane through the zenith:
for the intensity component perpendicular to the vertical plane through
the zenith:

\[ I_x(\tau; \mu, \varphi) = I_x^{(0)}(\tau; \mu, \mu_0) + I_x^{(1)}(\tau; \mu, \mu_0) \cos (\varphi - \varphi_0) \]

\[ + I_x^{(2)}(\tau; \mu, \mu_0) \cos 2(\varphi - \varphi_0) + \ldots + I_x^* \]

and for the polarization parameter \( \mathcal{U} \), determining the position of the
plane of polarization,

\[ \mathcal{U}(\tau; \mu, \varphi) = \mathcal{U}_{(0)}(\tau; \mu, \mu_0) + \mathcal{U}_{(1)}(\tau; \mu, \mu_0) \sin (\varphi - \varphi_0) \]

\[ + \mathcal{U}_{(2)}(\tau; \mu, \mu_0) \sin 2(\varphi - \varphi_0) + \ldots + \mathcal{U}^* \]

The fourth polarization parameter can be omitted since its value under
normal conditions is negligible with respect to the others. The last
terms in these equations (marked with an asterisk) indicate the effect
of ground reflection. If we measure these parameters directly, at a
sufficient number of points in the lower hemisphere, we can perform the
harmonic analysis with respect to \( \varphi - \varphi_0 \) and thus obtain the measured
values for the coefficients in this harmonic series that are only func-
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tions of the optical thickness $\mathcal{T}$, and the directional parameters, $\mu, \mu_0$. It is quite clear that in this way we can obtain many of the quantities to be used for the inversion.

For pure molecular atmosphere (with Rayleigh scattering only) all coefficients of higher harmonics should be zero, and also

$$I_r^{(s)} = \mathcal{L}^{(s)} = 0$$

(4)

The remaining seven coefficients are, however, not independent, since the following relations exist:

$$I_r^{(c)} = \mu \mathcal{L}^{(c)}$$

(5)

$$I_r^{(z)} = -\mu^2 I_r^{(s)} = \frac{1}{2} \mu \mathcal{L}^{(z)}$$

(6)

The conditions in Eqs. (4, 5 and 6) are necessary conditions for Rayleigh scattering; if they are not satisfied, then the atmosphere contains non-Rayleigh scatterers and the degree to which the relations are not satisfied can be used as the measure of the atmospheric turbidity.

If these relations are satisfied, then we have at our disposal four functions that can be used for the inversion. Two of them, the coefficients at the first and second harmonic, are not affected by Lambert's reflection. By proper combinations of the measured values of these two coefficients for different values of $\mu$, we can obtain a function that depends only on $\mu_0$ and $\mathcal{T}$; since such a function (see Fig. 3) is a monotonic function with increasing $\mathcal{T}$, we can, for a known $\mu_0$ uniquely determine the optical thickness. For $A = 0$, we get four
such functions, and hence four independent determinations of the optical thickness. If $A \neq 0$, we get two independent determinations of $\tau$ and $A$. The consistency of these independent determinations of $\tau$ and $A$ can be again used as criteria for the atmospheric purity.

This method could be extended for a turbid atmosphere if we knew how to solve the problem of radiative transfer for different models of the turbid atmosphere. Since we do not yet know this, we shall have to wait until more progress is made in that direction. As we are now on the right track to the solution of this problem, we hope the waiting period will not be long. The theory should provide the values of the differences of the coefficients in the series in Eqs. (1, 2 and 3) from those for Rayleigh scattering for different models of the turbid atmosphere. Since we will have a large number of functions to be compared, we will be able to obtain a fairly accurate determination of the parameters which define the aerosol content of a turbid atmosphere.

There are several advantages of this inversion method that should be mentioned. First, from the studies of skylight polarization we know that under normal conditions the molecular scattering is always predominant. Therefore, some kind of perturbation method could be introduced and is justified from the analogy to skylight polarization. Furthermore, since the upward and downward emerging radiations are very closely related, there is the possibility of testing the inversion method on the skylight, and thus estimating the accuracy of the method from ground observations. Finally, if we take measurements at different wavelengths throughout the visible region, we obtain numerous sets of data that can be used to check the consistency of the results.

What was said above about turbidity effects applies also to ground reflection which differs from Lambert's law (assumed above). So far, the theoretical results can be extended to Fresnel's law of specular reflection; if we know from the measurements the reflective properties of other types of reflection, the method can be extended to a more general type.
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DISCUSSION

Fritz: Dr. Sekera, you mentioned the method to determine the surface pressure of the lower boundary. Could you tell us to what accuracy you think you might get the pressure?

Sekera: It is very difficult to estimate the accuracy of this method right now. We will be able to get more information about the accuracy after we perform a test of the method on the skylight. Although the determination of Loschmidt's number (i.e., the number of air molecules in 1 ccm, from skylight measurement) was done a long time ago with an accuracy comparable to that of other methods, I do not believe that the method is accurate to such a degree that it could be used for the determination of earth surface pressure.

Kindle: Would it be possible to use a scanner and compare the brightness of the air light on the solar and antisolar side of the horizon in order to get the rough estimate of the aerosol content of the atmosphere? Or the measurement of sun's radiation during the sunset or sunrise?

Sekera: It should have been mentioned that the quantities referred to as "measured" are the Stokes parameter in relative units of the extraterrestrial solar radiation. Therefore, the necessary condition for the measurements is to measure the sun's radiation continuously with the Stokes parameter. A comparison with the theory of measurement of the emerging radiation near the horizon introduces the difficulty that the theoretical values are based on the assumption of a plane-parallel atmosphere, whereas near the horizon this assumption is not valid, as the curvature of the earth's atmosphere has to be taken into consideration.

Fritz: There is also a further difficulty. If you are going to scan closer to the horizon, you are overlooking tremendous geographical areas, which you have to assume are uniform.

Sekera: Because of the lack of time, I omitted completely the discussion of the experimental parts of the inversion method. In this method scanning is not a suitable experimental procedure; it should be replaced by simultaneous measurements from specified directions. The point Dr.
Fritz mentioned (i.e., the change of the viewed area of the earth's surface with the nadir angle of the observation) has to be carefully taken into consideration. A detailed discussion of the inversion method with proper mathematical analysis will be published in a paper, entitled "Determination of Atmospheric Parameters from Measurement of Polarization of Upward Radiation by Satellite or Space Probe" by Z. Sekera in *Icarus* 5, 1966.

REFERENCES


Fig. 1 Diagram illustrating the relationship between the distance of the neutral points from the antisun and the optical thickness for different zenith distances of the sun.
Fig. 2 The degree of polarization in the nadir as a function of the optical thickness for different zenith distances of the sun.
Fig. 3  Graph showing the monotonic relationship of the function \( M^{(1)}(\tau, \mu) \) and the optical thickness for three different values of \( \mu \).
All the serious suggestions for ozone studies from measurements of the solar radiation back-scattered by the earth's atmosphere have been confined to that part of the atmosphere above 35 km. It is recognized that this restricted study is of very limited interest and it is desirable to have reliable data on ozone parameters from the entire atmosphere for several studies. However, in spite of the fact that the idea is an old one (Singer and Wentworth, 1957), no significant contribution has been forthcoming, mainly because of the great difficulty encountered in the evaluation of the effects of multiple scattering and of the radiation reflected by the ground and the clouds.

Sekera and Dave (1961) first considered the effects of multiple scattering on the outgoing radiation in an approximate way. An understanding of both the scattering and ground reflection effects is now possible as a result of an important contribution to the field of radiative transfer in a nonhomogeneous atmosphere by Professor Sekera (1963). This work was supplemented when a physical interpretation was provided for the procedure of the successive iteration of the auxiliary equations (Dave, 1964). In that paper the expressions for taking into account the
effect of ground reflection on the radiation scattered by a nonhomogeneous, plane-parallel atmosphere were also derived explicitly.

In what follows, the discussion of multiple scattering effects will be limited to a plane-parallel, Rayleigh atmosphere in which an average ozone amount is distributed in an acceptable manner. No ground reflection effects are considered as the work has not been completed as yet. Neither the effect of large particle scattering, nor the effect of the sphericity of the atmosphere is considered. These last two effects, although negligible under certain conditions, should be appropriately evaluated and accounted for. Even though it is not impossible to compute the effects of large particles and the shape of the atmosphere on the outgoing radiation for a few cases, the computer time required would be enormous. Hence, any extensive numerical study should follow a careful, theoretical investigation.

Figure 1 shows the distribution of ozone which was used in this numerical study. A total ozone of 0.341 atm-cm was distributed in an acceptable fashion. This distribution is something like what one would encounter on an average in the middle latitude. The distribution has a maximum around 24 km and some ozone is assigned to layers up to 70 to 75 km in accordance with the photochemical theory. Extensive computations of the primary and multiply scattered radiations by the earth's atmosphere with this distribution of ozone have recently been completed (Dave and Furukawa, 1966).

Confining further discussion to the radiation leaving the top of the atmosphere along the nadir direction, i.e., \( \theta = 0^\circ \), Fig. 2 represents the contribution to the upward radiation from the various parts of the atmosphere for zenith angle of the sun, \( \theta_o = 60^\circ \). The contribution is normalized to unity at the level of maximum for each wavelength. These curves are for the case when only primary scattering is considered. For \( \lambda = 2550 \text{ Å} \), the maximum contribution comes from 53 km and a significant percentage is from the region of the atmosphere between 45 and 65 km. The lower wing of the curve extends right down to
42 km and the upper wing probably extends far above 70 km. The shape of
the curves is similar for $\lambda = 2550, 2875$ and 2975 Å except that the
curves shift downward with increasing $\lambda$. Since the contributions are
confined to relatively narrow regions and their positions vary with $\lambda$,
one can design an objective inversion scheme to obtain some information
on the structure.

For $\lambda = 2975$ Å, the maximum is around 38 km and the lower wing
extends down to 22 km, i.e., there is some contribution from the part
of the atmosphere below the ozone maximum. Because of this, higher
orders of scattering play a significant role and the contribution by
further higher orders is about 1% of that due to primary scattering,
for this particular case.

The curve for $\lambda = 3075$ Å exhibits a secondary maximum at about 10 km
and a significant fraction of the total contribution originates from
a fairly broad region of the atmosphere, extending from the ground to
40 km. Hence, for this particular case, this wavelength does not con-
tain much information on the vertical structure. Further increase of
$\lambda$ results in only one maximum in the troposphere and $\lambda > 3100$ Å may
be used for obtaining information about total atmospheric ozone.

The next two figures bring out the effect of multiple scattering
on the contribution-height curves. Figure 3, which is for $\theta = 60^\circ$,
shows the effect of multiple scattering at two wavelengths, $\lambda = 3025$
and 3075 Å. The broken and solid curves are for primary and multiple
scattering respectively. For $\lambda = 3025$ Å, where the lower wing of the
primary curve extends right down to the ground, the effect of multiple
scattering is shown by an increased contribution from the lower atmo-
sphere. The normalizing factors are $1.27 \times 10^{-4}$ and $1.29 \times 10^{-4}$ re-
spectively, and with a small increase in area under the curve, higher
orders contribute about 2 to 3% of primary scattering.

For $\lambda = 3075$ Å, the changes are more significant. The effect of
multiple scattering is to interchange the positions of the primary and
secondary maxima of the curve. As a result, proportionately more radiation originates from the lower atmosphere. Higher orders contribute about 45% of that due to primary scattering in this case.

Figure 4 shows the relative effects of higher orders for $\lambda = 3125$ Å but for two values of $\theta_o$, 0° and 60°. As before, for multiple scattering, the contribution is confined to the same part of the atmosphere for both cases. Consideration of primary scattering only raises the curves somewhat. Considering the normalizing factors and areas under the curves, the higher orders contribute 82% and 84% for $\theta_o = 0°$ and 60° respectively.

This effect of multiple scattering, namely, the lowering of the effective height of scattering, can be used to advantage to obtain information about the total ozone content in the vertical column (see Fig. 5). For this purpose, we shall consider the following numerical experiment. In the absence of any satellite observations, $I(\lambda; \theta, \theta_o)$ represents the computed intensities from the forthcoming tables (Dave and Furukawa, 1966). Subscript m represents that multiple scattering is taken into account. The model then assumes total ozone lying on the top of a molecular atmosphere whose reflecting property is given by $I_p$ (Case I) or $I_m$ (Case II). The subscripts p and m respectively represent the cases of atmospheres having only primary scattering and all orders of scattering. This model assumes a double path of the radiation through the ozone layer and hence should be valid approximately as long as the scattered radiation originated from very near the ground. However, the scattered radiation does not all originate from these levels and hence the ozone amount obtained from such treatment of data is labeled apparent total ozone $X_A$.

Figure 6 shows the values of $X_A$ as recovered from this kind of analysis for different zenith angles of the sun $\theta_o$ and wavelengths $\lambda$. For Case I, the primary-scattering model, $X_A$ decreases with increase of $\lambda$. For $\lambda = 3075$ Å only about half the real ozone $X$ is recovered while for $\lambda = 3175$ Å, $X_A$ has negative values for values of $\theta_o$ between
In the second case, where the atmospheric scattering properties are accounted for more realistically by taking into account all orders of scattering, the results are much better. The trends are reversed in the sense that $X_A$ now increases with $\lambda$. For wavelengths 3125 and 3175 Å and $\theta_0$ between 0° and 60°, one obtains values for $X_A$ between 0.315 and 0.335 atm-cm within 10% of X. For higher values of $\theta_0$, $X_A$ decreases rapidly with increase of $\theta_0$ and the model breaks down as the effective height of scattering rises into the ozone layer. However, for these large values of $\theta_0$, one will have to take into consideration the sphericity of the atmosphere.

The results presented here suggest that meaningful information on total atmospheric ozone can be obtained from measurements of the solar radiation back-scattered by the earth's atmosphere. For this, one must take into account the effects of multiple scattering. Further extensive theoretical-numerical study is necessary in the several directions outlined earlier if the satellite is to be kept active over the greatest fraction of its orbit for obtaining reliable total ozone information.

Acknowledgment: The authors wish to acknowledge the assistance of Paul M. Furukawa in the work presented here.

REFERENCES


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Total Ozone = 0.341 atm-cm

Fig. 1  Ozone distribution assumed for the computations.
Fig. 2 The variation of the contribution to the upward radiation with height for different wavelengths. Only primary scattering included. The curves are normalized to unity at the height of maximum contribution. $\theta_o = 60^\circ$, $\theta = 0^\circ$. The surface underlying the atmosphere is assumed to be perfectly black, i.e., $R = 0.0$. 

Total Ozone = 0.341 atm-cm

Contribution to the upward radiation

Height (km)

0.0 0.4 0.8

Contribution to the upward radiation

0 20 40 60 80

2550 Å

2875 Å

2975 Å

3075 Å

3126 Å

3400 Å
Fig. 3  The variation of the contribution to the upward radiation with height for different wavelengths. The curves are normalized to unity at the height of maximum contribution. $\theta_o = 60^\circ$, $\theta = 0^\circ$, $R = 0.0$. 
Fig. 4  The variation of the contribution to the upward radiation with height for two different zenith angles of the sun. The curves are normalized to unity at the height of maximum contribution. $\lambda = 3125 \text{ Å}, \theta = 0^\circ, R = 0.0$. 

Total Ozone = 0.341 atm-cm 

--- Primary scattering only 
--- All orders of scattering 

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Plane-parallel, Rayleigh atmosphere; No ground reflection

\[ I(\lambda; \theta; \theta_0) \]

\[ I_{p,m}(\lambda; \theta; \theta_0) \]

\[ \tau = \tau^{(s)} + \tau^{(a)} \]

Actual measurements from satellite or in their absence, computed specific intensities.

Computed specific intensities. \( I_p \) only primary scattering. \( I_m \) all orders of scattering.

Case I: \( I(\lambda; \theta; \theta_0) = e^{-a_\lambda x_{\lambda}(\sec \theta + \sec \theta_0)} I_p(\lambda; \theta; \theta_0) \)

Case II: \( I(\lambda; \theta; \theta_0) = e^{-a_\lambda x_{\lambda}(\sec \theta + \sec \theta_0)} I_m(\lambda; \theta; \theta_0) \)

Fig. 5 An approximate method of obtaining values of total ozone in the atmospheric column from the satellite measurements of the ultra-violet radiation back-scattered by the atmosphere.
Fig. 6 The variation with $\theta_0$ of the apparent total ozone amount ($X_A$) as recovered using the approximate model described in Fig. 5.
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SATELLITE PROGRAMS
NOTE: Since these papers were presented, parts of the proposed programs have been implemented.
I see by the program that my talk is titled "The Weather Bureau and the Satellite Program." I will refer briefly to that, but I would like to touch on some things which may not have received enough attention, based on questions which have been raised during the course of the meeting.

Prior to Sputnik I, there was a growing interest in the Weather Bureau in the possibility of using earth orbiting satellites to observe the atmosphere. Harry Wexler, in particular, and Sig Fritz were proponents in the early 1950s. The Meteorological Satellite Section, as it was originally called, was established in the summer of 1958 by bringing together a small group from various parts of the Weather Bureau to study the question of the use of satellites for meteorology. Shortly thereafter, NASA was established and the Meteorological Satellite Section became sort of the meteorological arm of NASA. Tiros I was launched on 1 April 1960 and was immediately a howling success, perhaps too much so in that it was obvious that immediate operational use could be made of the satellite data. Thus, what had been strictly a research and development project immediately started to suffer schizophrenia, trying still to be a research and development project but at the same time trying to satisfy operational demands. We were faced with the very obvious operational application of satellite data. As a result the Government decided in 1961 that a National Operational Meteorological Satellite System (NOMSS) should be established. The responsibility for this program
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was assigned to the Weather Bureau with NASA supplying assistance on the
space technology aspects.

In order to meet its new responsibilities, the Weather Bureau estab-
lished the National Weather Satellite Center (NWSC) in 1962. The Mete-
orological Satellite Section, by then known as the Meteorological Satel-
lite Laboratory, became the research arm of NWSC. In addition, the NWSC
developed two other major areas. One is the Operations Division which
has been growing in its scope. With the advent of the Tiros Operational
Satellite (TOS) system early in 1966, this will become a major enter-
prise engaged in receiving and processing the raw satellite data into
forms that can be used for a variety of operations, and also processing
the data for deposit in NWSC. A Systems Engineering Division is primar-
ily concerned with working with NASA on the engineering aspects involved
in establishing the TOS system. They are also working on certain future
developments, such as ocean buoys and constant-level balloons for use
with satellites.

The NWSC is guided by three broad program objectives. The first
objective is to obtain meteorological satellite observations over the
entire globe on a regular daily basis as soon as possible. The TOS
system is being established for this purpose. Admittedly, this is being
done for operational purposes, but I think you will recognize the value
of having a continuity of observations built up for research use. Super-
imposed upon the TOS program will be the NASA R & D program of special-
ized launches.

The second major objective is to observe the earth's atmosphere
continuously. We have heard many times this week in discussion of meso-
scale phenomena that there is a problem with data from polar orbiting
satellites in that observations are available only once every 12 or 24
hr; the need for more frequent observations has been stressed. This
need, we believe, can best be satisfied by making use of synchronous
satellites. Plans are well along in the NASA R & D program for utilizing
synchronous satellites for meteorological experiments. (We heard earlier this week about the camera system which Dr. Suomi is developing with Hughes Aircraft Corporation which will be flown on one of the Applications Technology Satellites (ATS) of NASA.)

The third major objective is sounding the atmosphere on a global basis. This was the subject of yesterday's discussion. I believe this represents the most difficult objective to reach from the standpoint of space technology. But it is obviously the most important objective to reach. We must push forward with the development of any techniques which can provide the quantitative observations of the atmosphere on a global basis required for numerical prediction. You have heard here about one satellite infrared spectrometer experiment and many groups are working on various experiments directed toward the sounding of the atmosphere.

In summary, the three objectives as we see them in the Weather Bureau are first, to get an operational system as soon as possible; second, to add the capability of continuous observations; and third, to add the capability of sounding the atmosphere on a global basis.

I would like to describe briefly the satellites which are in operation today. NASA has in orbit Tiros VII, VIII, and IX, and they have launched Tiros X for the Weather Bureau. These four satellites are still providing useful data. Tiros VII, VIII, and X are spin stabilized, as all Tiros satellites except IX have been, with the optic axes of the cameras parallel to the spin axis. Tiros VII has two wide-angle cameras still operating; each of these is equipped with tape recorders. It also carries the five-channel Medium Resolution Infrared Radiometer (MRIR) and the Suomi hemispherical radiometer. Radiation data were obtained during the first two years of the operating life of Tiros VII.

Tiros VIII has one wide-angle camera still operating. The other camera on this satellite was an Automatic Picture Transmission (APT) camera which is not operating at this time. Tiros X has two wide-angle
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Cameras with tape recorders; one is not operating at the present time. Tiros IX is in the so-called "cartwheel" configuration and in a sun-synchronous, quasi-polar orbit. In essence, this configuration permits the taking of pictures while viewing straight down rather than the wide variety of angles with respect to the earth's surface typical of all other Tiros satellites launched to date. Tiros IX is equipped with two wide-angle cameras with tape recorders; neither of the tape recorders is working at the moment.

Now I would like to describe the TOS system which will be initiated early in 1966. The first launch will use a spacecraft which is identical to Tiros IX. The second launch, about a month later, will use a spacecraft in the cartwheel configuration at 750 nautical miles altitude, but equipped with two APT camera systems for the direct transmission of pictures to any local users equipped with relatively inexpensive ground stations. With the TOS-APT system, a local station will get coverage of an area in excess of 10,000,000 square miles on each of the daily series of passes over the station. The next TOS spacecraft will use two Advanced Vidicon Camera System (AVCS) units to provide global data for central analysis. The AVCS is, in principle, the same kind of camera and tape recorder system used on Tiros, but with higher resolution and better dynamic range. Both the APT and AVCS systems were developed in NASA's Nimbus program. The TOS-AVCS spacecraft also will carry a set of Suomi flat-plate radiometers to start building up a global climatology of the atmospheric heat budget. Subsequent TOS launches will be scheduled so as to maintain one of each type, APT and AVCS, in operation at all times.

The TOS spacecraft will be placed, hopefully, in a circular orbit at an altitude of 750 nautical miles, considerably higher than previous meteorological satellites. A sun-synchronous orbit will be used such that the satellite will cross the equator at the same local time on each orbit. This is accomplished by selecting an orbit inclination slightly off the polar axis of the earth, such that the earth's oblateness will torque the orbit plane at the same rate that the earth
is moving around the sun. Thus, the angle between the orbit plane and the earth-sun line remains constant throughout the life of the satellite. This was the same orbit used for Numbus and for Tiros IX and I.

It is important to note that initially it is necessary to have two different spacecraft in operation simultaneously: the APT spacecraft for direct readout to local users and the AVCS spacecraft for the collection and storage of global data for central analysis. Each satellite has two cameras and supporting subsystems, not intended for simultaneous operation, but for redundancy to increase satellite life. Two major Command and Data Acquisition (CDA) stations, located at Fairbanks, Alaska and Wallops Station, Virginia will be used to control all of the satellites and to receive the stored data from the AVCS-equipped satellites. Wide-band (48 kc) microwave communications facilities from the two stations are used to transmit the data from the CDA stations to the NWSC in Washington, where the data are processed and interpreted prior to the output being disseminated to the National Meteorological Center and to domestic and overseas users. The observations are also processed in archival format and catalogued for storage in the NWSC.

NASA is working on a number of developments which we hope will improve the TOS system in the coming years. Of particular interest is a single camera system which will perform both of the functions of the presently separate APT and AVCS cameras. This will permit the use of a single spacecraft rather than two separate types of spacecraft required initially in the TOS system. NASA also is studying the possibility of using the High Resolution Infrared Radiometer (HRIR) on the TOS spacecraft.

I would like to refer briefly to the availability of satellite data for research. The Weather Bureau issues catalogues of meteorological satellite picture data. These catalogues include reproductions...
of all of the operational nephanalyses and listings of when and where the pictures were taken, etc., and the appropriate film roll number to order from NWSC for each picture sequence. Copies are available in the form of 100-ft rolls of 35-mm film, either positive or negative transparencies.

Catalogues on the Nimbus HRIR and picture data have been issued by NASA. They also have produced catalogues for the MRIR data obtained from the various Tiros satellites. These catalogues also list the various formats in which the radiation data can be obtained and a number of computer programs which have been developed for further processing of the radiation data. The Weather Bureau cloud catalogues are available from the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. The NASA catalogues are available directly from NASA. There are a number of computer programs available from Goddard, the NWSC, and some universities for handling satellite data. Much of the Tiros radiation data are being processed in the form of daily maps of albedo and total outgoing radiation. I would strongly recommend that a person beginning to use satellite data visit NASA, NWSC, and the universities having large programs using meteorological satellite data to find out what has been done, what data are available in various forms, what computer codes are available, etc. This may save a tremendous amount of work in initiating a new program. A recent article by Robert L. Pyle (1965) summarizes the meteorological satellite data and formats generally available.

I have heard some questions about the availability of training material for use in course work. We continue to be involved in a number of training programs and workshops. Therefore, we are developing considerable material, some of which may be suitable for use in university courses. We would be happy to do what we can to make some of this material available. Vincent Oliver has the primary responsibility in NWSC for this work and can provide examples of the material being
developed. This is only a beginning; we would like to have your suggestions regarding the types of material you think would be most suitable for university instruction.

DISCUSSION

Kellogg: Thank you, Dave. I think we ought to leave this part of the discussion open for questions now a while but remember when Morrie Tepper talks about the NASA program, he will get into the future of the vehicles themselves. I think that perhaps we should also let the NASA people talk about the future of the instrumentation. If there are questions on what Dave has said, then the floor is open for that.

Watson: Dave, is there any research going on to send more nephs over the facsimile lines using a reduced scale (a reduced map scale) say 1 to 30 or 40 million?

D. Johnson: I am not aware of any move to use a reduced map scale. It is not possible to produce operationally the nephs in the large number of scales and projections which would be required to satisfy all the possible desires of the many users. Therefore, we have standardized on the Mercator projection for the tropics and polar stereographic for higher latitudes with one or two map scales in each case that seem to be most satisfactory for a majority of the users.

Oliver: On that point, I have been discussing with the NMC people the idea of putting the complete neph of the whole northern hemisphere on the 1000-mb chart that comes out once a day. They seem to be favorably inclined. This would be quite late and not quite synoptic, but at least you would see the whole picture once or twice a day.

Renard: Do any plans exist presently to digitize the picture data?

D. Johnson: Not the present Tiros information. We have done this experimentally and are preparing to do this for TOS. We hope to use the computer to rectify the picture data and produce mosaics on standard map projections. Eventually, this may replace the present nephanalysis, although we are not sure how soon it will be done operationally.
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Renard: Are there any plans to obtain and store all the APT data that is going to be collected ultimately?

D. Johnson: Yes. We will encourage the APT receiving stations to provide pictures for archiving. Of course, this will be on a voluntary basis except for the stations operated by ESSA.

Renard: Where in all of these satellites does NASA control end and ESSA control begin?

D. Johnson: ESSA has the responsibility for the operation of the TOS system. We specify to NASA what spacecraft we want and transfer money to them for the spacecraft and the launchings. They then have the spacecraft built and launch the satellites into the proper orbit. At this point, NWSC takes over the operation of the complete system. Subsequently, NASA launches replacement satellites as needed to provide daily coverage. NASA is responsible for all aspects of the development and operation of the Nimbus and Tiros research and development satellites.

REFERENCE

METEOROLOGICAL FLIGHT PROGRAM PLANS

Tiros/TOS

The success of Tiros as a research satellite and in the application of the data operationally has encouraged developments toward a Tiros Operational System (TOS). This system is funded and managed by the Environmental Science Services Administration (ESSA). The NASA responsibilities are to develop the spacecraft and supporting ground equipment, launch the spacecraft into orbit, check out the system operationally, and then turn it over to ESSA for regular operations. There will be two spacecraft in orbit at all times -- one, the Advanced Vidicon Camera System (AVCS) spacecraft, will provide global recorded cloud cover data; the second, the Automatic Picture Transmission (APT) spacecraft, will provide the local data directly to local users. The AVCS spacecraft will deliver its data to the CDA stations at Fairbanks, Alaska and Wallops Island. Local APT user equipment will receive the continuous transmission of the APT spacecraft.

There are two obvious deficiencies in this first operational system to be in operation in 1966 and 1967. First, two Tiros spacecraft are required in orbit at all times to accommodate the AVCS system and the APT system. The second deficiency is that the systems discussed thus far provide only daylight data. The second group of TOS spacecraft for 1967 and 1968 will combine the AVCS-APT capability into one

* Summary of paper presented at meeting.
spacecraft by using the recorder we are developing for the APT camera. During the period 1968 to 1970 a separate spacecraft will make it possible to get global recorded and direct local readout of nighttime cloud cover data, using the High Resolution Infrared Radiometer (HRIR) modified for spin-stabilized spacecraft. Two satellites will be required because the HRIR resolution is less than that required by the users, and vidicon systems will probably continue to obtain daylight data. Finally, in the early 1970s, a development of a higher resolution radiometer system will allow a single spacecraft to fully satisfy the requirements for day and night global and local readout.

Tiros cloud cover data can describe the location of storm areas and tracking of these makes it possible, by extrapolation, to predict the likely future location of the storm. However, it is not possible, using Tiros data, (1) to locate the areas where new storms are expected to develop, (2) to anticipate changes in intensity of a storm, or (3) to anticipate changes in the motion of the storm.

It is the R & D meteorological satellites such as Nimbus that will be used to conduct the research and instrument development required to solve these and other problems.

**Nimbus**

Nimbus C is being readied for launch in 1966 as the next research flight in the NASA meteorological satellite program. Four primary sensors will be carried -- AVCS, APT, HRIR, and MRIR. Successful flight test of the AVCS and APT on Nimbus I allowed us to use them on TOS. The HRIR also flown successfully on Nimbus I, will be modified for Nimbus C to permit readout by specially equipped local APT stations. The five-channel Medium Resolution Infrared Radiometer (MRIR) will be an uprated version of the one flown on Tiros.

Nimbus B will follow Nimbus C by about one and one-half years. A 50-watt SNAP-19 RTG will assess the operational suitability of radioisotope power for meteorological satellites and augment the
satellite's solar power supply. Other experiments are described below.

A carbon dioxide spectrometer is being developed by ESSA. The bands of the spectrum in the vicinity of the 15\textmu m channel provide the radiation primarily from different levels of the atmosphere. Since radiation is a function of temperature, it is possible to reconstruct the temperature of the radiating layer and thence a vertical temperature profile. With this IR spectrometer, the temperature profile can be reconstructed only above the cloud level.

In order to measure the wind along with the other basic meteorological parameters, we are developing the Interrogation, Recording, and Location System (IRLS), which, aboard the satellite, will be a black box. Programmed by a Command and Data Acquisition (CDA) station, it will sequentially interrogate instrumented platforms on the earth and in the atmosphere and record their observations. The satellite will retransmit the data to the central receiving station. Successive orbital positions of constant-level balloons will provide mean wind velocities. A test of this system on Nimbus B will involve a few transponders to check the location and resolution capability with stationary and moving platforms. We are also considering a complementary approach to IRLS using the combination of the Navy Omega navigation system and a communication-type satellite.

The IR interferometer spectrometer will measure the radiation of the Earth and atmosphere from 5 to 20\textmu m. This instrument, which is smaller but more complex than the ESSA spectrometer, will provide not only information on the vertical temperature structure but also on water vapor and ozone distribution.

The Image Dissector will provide continuous daylight cloud cover pictures for recorded and direct local readout. The HRIR will be further developed so APT ground equipment will require minimum modification. Thus Nimbus B will provide day and night global and local cloud cover data. The MRR will be flown again to obtain further heat
balance data. Finally, a solar ultraviolet experiment will monitor the sun's output in five UV bands to detect time variations of relative intensity.

Nimbus D will be launched about 18 months after Nimbus B. Experiment candidates for Nimbus D include: (1) Image Orthicon Camera for day-night television, (2) Dielectric Tape Camera for improved resolution and fidelity, (3) microwave radiometer (18-20 Gc) for effective surface temperature measurements under clouds, (4) sferics (600 Mc with 200- to 300-mile resolution) for locating thunderstorm regions, (5) WEFAX (weather facsimile) for experimental transfer of weather data and maps via APT, (6) improved IRLS (8 to 40μ), (7) improved IRLS to handle more instrumented platforms, (8) improved ESSA spectrometer, (9) filter wedge spectrometer for measurement of the vertical water vapor distribution by radiation in the 6.3μ band, (10) backscatter UV experiment for vertical distribution of ozone, (11) 10 to 11μ HRIR experiment for obtaining both day and night cloud cover information, (12) image dissector for geographical and visual cloud cover reference, and (13) the ESSA cloud top spectrometer.

Applications Technology Satellites

Four Applications Technology Satellites (ATS) will be launched to synchronous altitude, where developmental experiments will be conducted of more continuous viewing of the cloud cover, for continuous and variable time scale observation of atmospheric behavior and structure. The first experiment, a spin-scan camera, will provide readout every 20 min of the sunlit disc of the earth effectively from 50°N to 50°S lat. Radiometers are being considered for later ATS flights as well as day-night TV cameras so nighttime data can be provided as well.

Manned Meteorology

There have been cloud photography experiments flown aboard Gemini, with more to come. The pictures and our discussions with the astronauts
indicate that they can contribute scientifically to meteorology. Three studies are under way to determine the potential of man in space for meteorology. One contract is to consider how the solution of meteorological programs can be furthered by man in space. The second contract focuses on means of utilizing man in space in accelerating meteorological instrumentation development. The third contract considers the manned spacecraft/experiment interface.

Post-1970 Programs

We have three projected post-1970 flight programs. The first is the Polar Meteorological Satellites (PMS) to continue the Nimbus kind of research and instrument developments. The second is the Advanced Meteorological Satellite (AMS) to combine developments in Tiros, Nimbus and other applicable programs toward the development of a post-TOS operational satellite system. The third is the Synchronous Meteorological Satellite (SMS) development which will evolve from our experience aboard ATS. These three areas are under study and our plans are not firm.

PROCEDURES FOR PARTICIPATION IN SPACE FLIGHT INVESTIGATIONS

NASA encourages the participation of scientists in its programs, and invites proposals from scientists to conduct (1) investigations on planned flight missions, (2) investigations which would require missions not specifically planned, and (3) design studies for future scientific investigation.

The procedure for submitting proposals was described. For complete details, those interested are referred to the document "Opportunities for Participation in Space Flight Investigations." *

* This publication is updated semiannually. Communications concerning its content and distribution should be addressed to:

Physics and Astronomy Programs, Code SG
Office of Space Science and Applications
National Aeronautics and Space Administration
Washington, D. C. 20546
The document gives proposal preparation instructions, submission procedures, the basis for selection of flight experiments, and the responsibilities of the participating principal investigator. Further discussion included description of the general NASA internal procedures for processing proposals. (The following is for domestic sources in universities, industries, and government installations other than NASA field centers. Procedures for foreign and NASA field center proposals differ only in slight details.) A scientific experiment proposal is received in the Research Grants and Contracts Office where it is registered and distributed to subject-interested offices for scientific merit, competence and experience of the investigator. Following evaluation by these groups, a meteorological proposal is submitted by the Meteorological Programs Office to the Planetary Atmospheres Subcommittee of the Space Science Steering Committee (SSSC) and simultaneously to the Advisory Group on Supporting Technology for Operational Meteorological Satellites (AGSTOMS). These subcommittees categorize the experiment in one of four ways: Category I, a prime experiment for inclusion aboard a flight; Category II, a lower priority experiment suitable for inclusion aboard a flight; Category III, the investigation requires further development of the associated experimental apparatus; Category IV, the proposed investigation is rejected for the mission under consideration. The recommendations of these groups are returned to the Meteorological Programs Office which integrates these scientific and technological recommendations with programmatic considerations and presents its recommendations for flight to the SSSC.

The recommendation of the SSSC is forwarded to the Associate Administrator for Space Science and Applications for his approval. It is then returned to the appropriate Program Office and/or Center for implementation. These groups then work with the investigator.
THE REQUIREMENT FOR METEOROLOGICAL RESEARCH ON MARS

We have discussed so far at length the problem of the meteorology of Earth. I would now like to pose some thoughts on the study of the weather on Mars.

Recently obtained Mariner IV data have stimulated further consideration of plans for the exploration of Mars. Manned landings presumably are to follow. Initial plans involve unmanned flybys, orbiters, and landers, which will provide, among other data, information on the basic composition and structure of the atmosphere of Mars. However, prior to serious consideration of manned landings on Mars, more detailed information is required on the variability of the Martian atmosphere; i.e., systematic investigation of the weather on Mars. The possible consequences of relying only on a relatively few data samples as a basis for coping with "Weather on Mars" can be illustrated by the "science-fiction" technique of considering some likely results of such an attempt at exploration of the Earth by Martians.

The hypothetical reversed roles could be described as follows. Initially, the Martians would have made as complete an astronomical description of Earth as possible from their planet. A bluish haze which surrounds Earth would make observation difficult, but they could conclude correctly the general significance of the several forms of H₂O, their phase relation to temperature, and the relatively small temperature range on Earth compared to Mars. However, the surface pressure of about 100 Martian atmospheres and the abundance of corrosive oxygen in the atmosphere would be felt to rule out life on Earth, at least as known on Mars.

Their first orbiter would send back a limited number of pictures of Earth from about 2000 km, showing extensive white and dark areas. The white areas would be clear and detailed. Large hemispheric patterns, globally organized, as well as small-scale patterns would...
lead to the correct conclusion that atmospheric motions on earth with their characteristic vortices are of many scales. Meaningful details of the dark areas would be largely lacking.

Prior to a "manned" landing, two probe landers would be sent down -- one into a white area (White area Probe or WAP) and the other into a dark area (Dark Area Probe or DAP). It is quite possible that the WAP could land in a cold frontal region in middle latitudes, and the DAP in a Siberian anticyclone in winter.

If the Martians would design their "manned" spacecraft in accordance with these criteria they might launch two Manned Earth Landers, one into a white area and one into a dark area. The one destined for a white area could enter a U.S. East Coast hurricane and then perish after a short transmission. The Dark Area Manned Earth Lander could similarly land in a desert region, such as either Death Valley or the Sahara. Thus probe and lander environments could be significantly different in supposedly similar areas. The moral for which the tale is contrived is that systematic investigations (rather than point observations by single or even several probes) must be the bases for defining the range of conditions to be expected, and the rate of change of the conditions. The systematic investigation would form the basis for predicting weather on Mars in a manner similar to that used on Earth.

The dynamical numerical approach is most likely to provide the tool for predicting this variability but the input data required would be difficult to attain and in fact are not yet available even for the Earth. We are working toward achieving the Earth capability in the 1970s. Thus, in devising an observation program for Mars we would consider a similar system of polar orbiting satellites and atmosphere-immersed sensor platforms, though some problems (such as data transmission) will be more complicated, others simpler (such as balloon hazards to aviation). Mars might also be considered as a place to conduct weather-control experiments.
APPENDIX

WORKSHOP PARTICIPANTS
PARTICIPANTS IN THE
WORKSHOP ON THE USE OF SATELLITE DATA
IN METEOROLOGICAL RESEARCH

Tomio Asai, National Center for Atmospheric Research,
Boulder, Colorado 80302

Carl I. Aspliden, Meteorology Department,
Florida State University, Tallahassee, Florida 32306

William R. Bandeen, National Aeronautics and Space Administration,
Goddard Space Flight Center, Greenbelt, Maryland 20771

Celso S. Barrientos, Geophysical Sciences Laboratory,
New York University, 2455 Sedgwick Avenue, Bronx, New York 10468

F. C. Bates, Institute of Technology, St. Louis University,
College Station, P. O. Box 8020, St. Louis, Missouri 63156

A. W. Brewer, Physics Department, University of Toronto,
Toronto, Ontario, Canada

John Brown, National Center for Atmospheric Research,
Boulder, Colorado 80302

H. Butler, National Aeronautics and Space Administration,
Goddard Space Flight Center, Greenbelt, Maryland 20771

Irving B. Chernetz, National Task Group for Mesometeorology,
Fort Monmouth, New Jersey 07703

Kuldip Chopra, University of Miami,
P. O. Box 8132, Coral Cables, Florida 33124

Mr. Chow, Department of Geophysics and Geophysical Engineering,
St. Louis University, St. Louis, Missouri 63156

P. E. Church, Department of Atmospheric Sciences,
University of Washington, Seattle, Washington 98105
John H. Conover, Air Force Cambridge Research Laboratories,
L. G. Hanscom Field, Bedford, Massachusetts 01730

Jitendra Dave, National Center for Atmospheric Research,
Boulder, Colorado 80302

Arnett S. Dennis, Institute of Atmospheric Sciences, South Dakota
School of Mines and Technology, Rapid City, South Dakota 57701

Lt. Col. Norman L. Durocher, Joint Meteorological Satellite

Amos Eddy, University of Texas,
631 Engineering Sciences Building, Austin, Texas 78712

R. Fleagle, University of Washington, Seattle, Washington 98105

Sigmund Fritz, U. S. Weather Bureau, ESSA, Washington, D. C. 20235

Tetsuya Fujita, University of Chicago, Chicago, Illinois 60637

Paul Furukawa, National Center for Atmospheric Research,
Boulder, Colorado 80302

Arnold H. Glaser, ARACON Geophysics Company,
Virginia Road, Concord, Massachusetts 01742

Guy G. Goyer, National Center for Atmospheric Research,
Boulder, Colorado 80302

Rudolf Hanel, Laboratory for Atmospheric and Biological Sciences,
Goddard Space Flight Center, Greenbelt, Maryland 20771

Bernhard Haurwitz, National Center for Atmospheric Research,
Boulder, Colorado 80302

*Eero Holopainen, Department of Meteorology and Oceanography,
University of Michigan, Ann Arbor, Michigan 48104

David Houghton, National Center for Atmospheric Research,
Boulder, Colorado 80302

*Now with the University of Helsinki, Department of Meteorology,
Porthania-Helsinki, Finland.
Willard S. Houston, Jr., USN, Joint Meteorological Satellite Program
Office, Headquarters, U. S. Air Force (CCG-QMS),
Washington, D. C. 20330

Lester F. Hubert, National Weather Satellite Center, ESSA,
Washington, D. C. 20235

David Johnson, Meteorological Satellite Laboratory, ESSA,
Washington, D. C. 20235

Paul Julian, National Center for Atmospheric Research,
Boulder, Colorado 80302

Shih-Kung Kao, Meteorology Department,
University of Utah, Salt Lake City, Utah 84112

Akira Kasahara, National Center for Atmospheric Research,
Boulder, Colorado 80302

Yale H. Katz, The RAND Corporation,
1700 Main Street, Santa Monica, California 90406

W. W. Kellogg, National Center for Atmospheric Research,
Boulder, Colorado 80302

Earl C. Kindle, Research Triangle Institute,
P. O. Box 490, Durham, North Carolina 27702

Jean I. F. King, Geophysics Corporation of America,
Burlington Road, Bedford, Massachusetts 01730

H. Klieforth, Desert Research Institute, University of Nevada,
Reno, Nevada 89507

P. Koteswaram, National Hurricane Research Laboratory,
Miami, Florida 33124

T. N. Krishnamurti, Department of Meteorology,
University of California, Los Angeles, California 90024

Douglas Lilly, National Center for Atmospheric Research,
Boulder, Colorado 80302

*Now with the Environmental Science Division, Room 3E1037, The Pentagon,
Washington, D. C. 20301.
*Julius London, National Center for Atmospheric Research, Boulder, Colorado 80302

E. Lorenz, Massachusetts Institute of Technology, Cambridge, Massachusetts 02139

E. A. Martell, National Center for Atmospheric Research, Boulder, Colorado 80302

Earl McLaughlin, Republic Aviation Corporation, Building 62, Farmingdale, Long Island, New York 11735

Paul Meschler, Defense Research Corporation, P. O. Box 3587, Santa Barbara, California 93105

Yale Mintz, Department of Meteorology, University of California, 405 Hilgard Avenue, Los Angeles, California 90024

Clifford J. Murino, St. Louis University, College Station, P. O. Box 8020, St. Louis, Missouri 63156

Morris Neiburger, Department of Meteorology, University of California, Los Angeles, California 90024

Chester Newton, National Center for Atmospheric Research, Boulder, Colorado 80302

George W. Nicholas, National Aeronautics and Space Administration, Goddard Space Flight Center, Greenbelt, Maryland 20771

Rolf M. Nilsestuen, Bureau of Naval Weapons Project FAMOS, U. S. Fleet Weather Central, Navy Department, Washington, D. C. 20390

William Nordberg, National Aeronautics and Space Administration, Goddard Space Flight Center, Greenbelt, Maryland 20771

G. Ohring, Geophysics Corporation of America, Bedford, Massachusetts 01730


*Now with the University of Colorado, Department of Astro-Geophysics, Boulder, Colorado 80302.
Charles A. Palmer, National Center for Atmospheric Research, Boulder, Colorado 80302

Hans A. Panofsky, Meteorology Department, Pennsylvania State University, University Park, Pennsylvania 16802

Richard J. Reed, Department of Atmospheric Sciences, University of Washington, Seattle, Washington 98105

Elmar Reiter, Colorado State University, Fort Collins, Colorado 80521

Robert J. Renard, Department of Meteorology and Oceanography, U. S. Naval Postgraduate School, Monterey, California 93940

D. F. Rex, National Center for Atmospheric Research, Boulder, Colorado 80302

Herbert Riehl, Department of Atmospheric Sciences, Colorado State University, Fort Collins, Colorado 80521

Robert Riggs, Bendix Systems Division, Ann Arbor, Michigan 48107

Walter Orr Roberts, National Center for Atmospheric Research, Boulder, Colorado 80302

Charles W. C. Rogers, ARACON Geophysics Company, Virginia Road, Concord, Massachusetts 01742

Stig Rossby, Department of Meteorology, University of Wisconsin, Madison, Wisconsin 53706

Frederick Sanders, Massachusetts Institute of Technology, Room 54-1612, Cambridge, Massachusetts 02139

Leslie D. Sanders, National Severe Storms Laboratory, ESSA, 1616 Halley Avenue, Norman, Oklahoma 73069

Yoshikazu Sasaki, Civil Engineering and Environmental Sciences, University of Oklahoma, Norman, Oklahoma 73069

Walter J. Saucier, Department of Meteorology, University of Oklahoma, Norman, Oklahoma 73069

Werner Schwerdtfeger, Department of Meteorology, Science Hall, University of Wisconsin, Madison, Wisconsin 53706
Zdenek Sekera, Department of Meteorology, University of California, Los Angeles, California 90024

Robert Sheets, National Hurricane Research Laboratory, U. S. Weather Bureau, Miami, Florida 33124

William C. C. Shen, Meteorology Research Division, Control Data Corporation, 8100 34th Avenue South, Minneapolis, Minnesota 55420

Paul E. Sherr, ARACON Geophysics Company
Virginia Road, Concord, Massachusetts 01742

S. Fred Singer, University of Miami, P. O. Box 8132, Coral Gables, Florida 33124

*Bengt Söderberg, National Center for Atmospheric Research Boulder, Colorado 80302

N. Spencer, National Aeronautics and Space Administration, Goddard Space Flight Center, Greenbelt, Maryland 20771

**Patrick Squires, National Center for Atmospheric Research, Boulder, Colorado 80302

V. E. Suomi, Department of Meteorology, University of Wisconsin, Madison, Wisconsin 53706

M. T. Tepper, National Aeronautics and Space Administration, 1512 H Street, Washington, D. C. 20546

Sidney Teweles, U. S. Weather Bureau, ESSA, Washington, D. C. 20235

Aylmer H. Thompson, Department of Oceanography and Meteorology, Texas A. & M University, College Station, Texas 77843

P. D. Thompson, National Center for Atmospheric Research, Boulder, Colorado 80302

Henry van de Boogaard, National Center for Atmospheric Research, Boulder, Colorado 80302

*Now with the Royal Swedish Air Force, Barkaby, Sweden.

*Now with the Desert Research Institute, University of Nevada, Reno, Nevada, 89507.
Harry van Loon, National Center for Atmospheric Research, Boulder, Colorado 80302

Thomas H. von der Haar, 35 North Park Street, Madison, Wisconsin 53715

Lauri Vuorela, Department of Meteorology, University of Helsinki, Porthania-Helsinki, Finland

Norman K. Wagner, Department of Geosciences, University of Hawaii, Honolulu, Hawaii 96822

Warren Washington, National Center for Atmospheric Research, Boulder, Colorado 80302

Eberhard W. Wahl, Department of Meteorology, University of Wisconsin, Madison, Wisconsin 53706

David Q. Wark, National Weather Satellite Center, ESSA, Suitland, Maryland 20235

*Gunter Warnecke, Institut für Meteorologie und Geophysik Der Freien Universität Berlin, 1000 Berlin 33 (Dahlem) Podbielaskiallee 62, Germany

Bruce F. Watson, Navy Weather Research Facility, Naval Air Station, Norfolk, Virginia 23511

William K. Widger, Jr., ARACON Geophysics Company Virginia Road, Concord, Massachusetts 01742

Jay S. Winston, Meteorological Satellite Laboratory, ESSA, Washington, D. C. 20235

Wilford Zdunkowski, Department of Meteorology University of Utah, Salt Lake City, Utah 84112

**Edward J. Zipser, Department of Meteorology Florida State University, Tallahassee, Florida 32306

* Now with the National Aeronautics and Space Administration, Goddard Space Flight Center, Greenbelt, Maryland 20771.

** Now with the National Center for Atmospheric Research, Boulder, Colorado 80302.