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1988 CIRA

SATELLITE RESEARCH WORKSHOP

September 21-23, 1988

Pingree Park, Colorado

CIRA Cooperative Institute for Research in the Atmosphere

Colorado State University

FOREWORD

This document reports on a Satellite Research Workshop sponsored by the Cooperative Institute for Research in the Atmosphere (CIRA) that was held at the Colorado State University's Pingree Park campus from September 21-23, 1988. The workshop was designed to investigate research and applications opportunities using data from the next generation GOES and TIROS satellites. The workshop consisted of a limited number of general presentations by experts in the following areas: 1) climate and large scale circulations; 2) mesoscale modeling; 3) nowcasting; 4) polar lows; 5) rainfall estimation; 6) satellite data use (imagery, microwave, sounding); 7) severe local storms; and, 8) tropical cyclones. Three working groups met and discussed application of satellite data for various scales of motion: 1) large time and space scale studies; 2) synoptic scale phenomena; and, 3) nowcasting.

Several important goals of the workshop were met. Those goals were: 1) familiarize participants with potential measurements and information available from the new generation of satellites; 2) stimulate discussion and exchange of ideas concerning potential meteorological uses of the data; 3) explore new avenues to approach the use of meteorological satellite data for specific research problem areas; 4) formulate suggestions for the operation of these new satellites to support specific research objectives; and, 5) address the need of validation and exploratory field measurements that address the observations expected from these satellites.

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1988 CIRA SATELLITE RESEARCH WORKSHOP
PINGREE PARK
SEPTEMBER 21-23, 1988

AGENDA

Wednesday, September 21, 1988

10:30 AM	Depart
12:00 - 1:00 PM	Lunch in Dining Hall
1:30	Check in at Main Conference Center - Hotchkiss Lodge Lobby
2:00	Large Group Session - Hotchkiss Lodge Seminar Room
	Introduction - Dr. James Purdom
	Presentations and Discussion
	Mr. William Shenk - Satellite Imagery **Dr. William Smith - Satellite Sounding**
	Break
	Dr. Stanley Kidder - Satellite Microwave **Dr. Roger Pielke - Mesoscale Modeling**
5:15	Adjourn
6:00	Dinner in dining Hall
7:15	Large Group Session - Hotchkiss Lodge Presentations and Discussion
	Dr. William Gray - Tropical Cyclones **Dr. Greg Forbes - Nowcasting** **Dr. Thomas Vonder Haar - Large Scale**
9:00	Adjourn

Thursday, September 22, 1988

7:00 AM	Continental Breakfast - Hotchkiss Lodge
8:00	Large Group Session - Hotchkiss Lodge Seminar Room
	Presentations and Discussion

Dr. Eric Rasmussen	-	**Polar Lows**
Dr. Geoff Austin	-	**Rainfall**
Dr. James Purdom	-	**Severe Storms**

9:45	Break
10:00	Small Groups - Individual Lodges
12:00 PM	Lunch
1:00	Small Groups - Individual Lodges
3:00	Open/Recreation
6:00	Barbecue Dinner - Steak/Trout
7:30	Fireside Chats - Hotchkiss Lodge Brief Overviews of Small Group Activities

Friday, September 23, 1988

8:00 AM	Breakfast
8:45	Large Group Session - Small Group
9:30	Break
10:00	Presentations and Discussions - Hotchkiss Lodge Seminar Room
	Open/Recreation
12:00 PM	Lunch
1:00	Large Group Session: Wrap up and Summary - Hotchkiss Lodge
2:30	Depart

SATELLITE WORKSHOP - AUGUST 1988
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SATELLITE IMAGERY

Talk given by William E. Shenk
at the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

What I'm going to try to do is point out some general principles of the power of the measurements we can make with geosynchronous orbiting satellites, then try to spark some ideas that may lead to a variety of new analysis and forecast techniques. I'll try to give an overview of some of the ideas for how we plan to use the new GOES series, the GOES I-M, and the more mature series, the NOAA K, L, and M. However, except for AMSU, there is not a lot of change planned for the NOAA series, as compared to GOES.

There is some material in your handouts for the new GOES imager. Let me refresh your memory. The current GOES VAS can use all of the channels like images. The GOES I-M imager is a separate instrument that will be used strictly for imaging, and there is another instrument for sounding. These are the spectral bands for the imager (Fig. 1), and these are the signal to noise specifications compared to the performance that is being estimated by the contractor (ITT).

One of the main uses of the new GOES instrument will be not only to look at the full earth disk like we have in the past, but to sectorize as well. We can look at the full earth approximately every 30 minutes, or look at a large sector from 60° N to 60° S and 120 degrees E-W which is almost the full earth. Also, you will be able to look at a 3000 by 3000 km area in about three minutes, because you can restrict the E-W as well as the N-S limit of the scan. This satellite is on a three axis stabilized platform, which is a complete change from the current spinning satellites. We're no longer going to spin, we are going to stare, and move mirrors back and forth over small angles to acquire the images. This allows us to be much more radiometrically efficient than was possible in the past. Therein lies one of the main advantages of the new system. We can achieve a much higher signal to noise ratio and scan small areas very fast.

Next I would like to make a few suggestions as to what our future imaging strategy ought to be. From geosynchronous orbit you can devise a variety of measurement scenarios. You can choose your area, as well as how frequently you look at that area. So, let's make sure we design our measurement scenarios to fit the scale and temporal history of each phenomenon. For example, a thunderstorm grows in a certain amount of time. We ought to measure at a rate that is more frequent than that, so we can accurately measure the vertical rise rate. Or if we want to measure the cloud motion, we need to look at that cloud often enough to perceive if, and how, it is changing with time. We've gotten into a groove over the years, believing that we have to take images at certain fixed intervals. We need to fit our temporal resolution and coverage requirements more closely to each phenomenon we survey or to the factors associated with the parameters we measure, rather than go with a set image interval and coverage like we have in the past.

I often emphasize the importance of stereo because stereo cloud top height measurement is the best and most accurate method we have. You can do it with very high spatial resolution visible data during the daytime hours. We should make much greater use of this tool. At night lower spatial resolution stereo is still possible but multispectral techniques like the CO_2 slicing method for cloud top height should also be used.

With the GOES I-M we will be able to more precisely locate our data. We are also going to be merging these data with a lot of other data sources. Combining GOES I-M imagery with NEXRAD is a good example for severe weather warning. In the GOES I-M program we are planning to achieve a daytime navigation accuracy of 4 km (3 sigma + 8 hours from local noon) and 6km (3 sigma, at night + 4 hours from local midnight). In the future, I'd like to see us even do better than that (e.g. 2 km, all day).

We have had our satellites at 75° W and 135° W from the very beginning of the GOES program. However, I believe that we ought to move GOES West further east. We need a better look at the United States from that satellite. I would suggest 120° W. That would also improve our stereo coverage dramatically, particularly in the western portion of the Atlantic Ocean where hurricanes threaten the southeastern coast and severe extratropical storms develop which affect our entire eastern coastline.

Audience: Bill, when you say it is within 3 sigma, what does that mean the accuracy is most of the time?

Answer: It means that 99.7% of the time it will be within the estimated accuracy.

Audience: And what percent of the time will it be within 2 km, for example?

Answer: We are assuming that the errors will be Gaussian, so during the daytime 2 km is between 1 and 2 sigma.

Audience: It still seems awfully bad. Is that the best that it can do?

Answer: Please remember that is the prediction accuracy. I should have said that in the beginning. These estimates of GOES earth location accuracy are based on making a 24 hour prediction, so we will not have to use landmarks or other navigational aids. However, we can still use landmarks, and we're hoping to use a star reference system as well, such that for a given image or a given series of images. You should be able to register images and earth locate to a better accuracy than those figures.

Audience: Are the problems with location due to warping of the satellite platform?

Answer: There is a warping effect. Since you aren't spinning, there is a slowly varying thermal effect during the 24 hour orbital period and the thermal problem is rather significant.

Audience: Is that the main source of error?

Answer: It is the main long term source of error. Long term is errors that occur over a frequency of hours. We are more concerned with the short term jitter. The longer term errors we think we can reduce or remove through modelling. But about half of the error source is in the East-West scan system of the imager, which is a short-term (e.g. seconds) source of error. Fortunately, we know these errors are there, and the contractors are working diligently to reduce them. Hopefully, we aren't going to put the satellite in orbit and find we've forgotten some error sources. We are aware of a number of error sources and the contractors are trying hard to correct them.

Audience: I think that you need to emphasize two great advantages of GOES I M; the improved radiometric sensitivity, together with the ability to rescan multiple areas, that we're unable to do very well today, especially when there's a conflict with operations. On the downside may be some of the difficulties that we just mentioned.

Audience: You say, or imply, that the grid will be 24 hour predicted.

Answer: That is the idea. To eliminate as much ground processing as possible.

Audience: If you subtract one picture from another that is 4km off....

Answer: Please remember, that case would only occur 0.3% of the time during the daytime. The 4 km error is not one sigma, it is three sigma. So two thirds of the time (for one sigma) you're error should be 1 1/3 km. If you want more precision, you can still go through the same type of procedures that you use now with landmarks. You can reduce the errors even further, down to less than 1 km.

Now, here is a list of products that NOAA/NESDIS expects to produce from GOES I-M (Fig. 2) early in the program. There are several pages of these. These products are called "Day 1" products and, shortly after GOES I goes up, these will be operationally produced.

Shown next are the "Day 2+", research products (Fig. 3). When the satellite goes up there will be research groups that are interested in looking into these. If a product from this list proves successful, it will be incorporated into the operational mainstream. I've got three pages of these. I'm just showing cloud heights as an example. For cloud heights, the possibilities range from multispectral infrared techniques to visible and infrared stereo. There are numerous other quantities like tropospheric moisture, surface temperature, winds from cloud motions, etc. For those of you that are interested, I can have copies made of the complete list, and you can look at them later.

Audience: You might mention that the Day 2 products are just ideas for products that might be developed after the initial Day 1 products. It is not by any means all inclusive, or prioritized. It is just a series of ideas that people had. When we're thinking about research products, perhaps some of those will eventually build into operational products.

Answer: Exactly. We don't want to create the impression that this is the final list.

Audience: When there is a defined product list for Day 2+, what will happen to those? Will they be archived at NESDIS; will they be transmitted out so that users can pick them up and archive them?

Answer: That is a question that we are addressing in a group called the GOES I-M Science Evaluation Working Group. We are working on defining who is going to be looking at these, and what our distribution system will have to be.

Audience: Does that include archiving?

Answer: To me it would include archiving.

Now what I want to show is a matrix for these Day 2+ research products we intend to create (Fig. 4) from the imager and some factors which should lead to improving these products.

On the left hand side is a list of imaging parameters. Across the top is listed items that should lead to better results for those parameters such as high frequency imagery (and by high frequency I mean imagery at < 5 minute intervals, perhaps as frequent as every 30 seconds). Some other items are the use of stereo or multispectral analysis. Therefore, for each parameter I'm trying to indicate where the different degrees of improvement or sophistication in the analysis might result in the improvement of that product.

Take, for example, convective intensity. Certainly high frequency imagery has a positive impact on our ability to quantitatively measure the physical size of thunderstorms and their growth rates. Stereo will give us more accurate cloud height information. For cloud motion derived winds, the same thing occurs. High frequency imagery will track the clouds better, and stereo will give us a better fix on the altitude. Multispectral techniques are also important (e.g. for identifying cloud types). Therefore, by using high frequency imagery, stereo and more than one channel, we can create a number of possibilities to come up with ways to improve a rather large list of future techniques.

Next I'd like to show my estimate of how the GOES I-M imager channels might be used for deriving various parameters (Fig. 5). You will recognize many of the same parameters (e.g. surface temperature, convection intensity) you saw on the other chart. I've listed the five channels of the GOES I-M imager across the top, and have tried to indicate where I feel each of these channels could be grouped together to estimate an important parameter. In some cases the groupings may seem a little surprising. Examples of this include the suggested channels for estimating surface temperature, and lower tropospheric moisture. For instance a visible channel is involved in an estimate of surface temperature and lower tropospheric water vapor since a clear column is needed, and the visible channel tests for clouds. There are many other similar examples in Fig. 5.

This new imager has some major features that are improvements over the VAS (Fig. 6). Fig. 6 shows which of those improvements are expected to have the greatest positive impact on the derivation of which parameters. On GOES I-M, we have higher IR spatial resolutions, there is a much better signal to noise in all of the channels, and we have oversampling in the visible channel. The current VAS visible channel samples once per instantaneous field of view in the East-West direction, but on GOES I-M, we're going to sample 1.75 times per field of view. We are going to make improvements in calibration. The major one will be seeing through the entire optical system to make the calibration. There is higher digitization; 10 bits in the visible channel versus 6 in the VAS, and 10 bits in the IR versus 8. And we think we are going to have better channel to channel registration. All of these will benefit the quantitative analysis of the data.

This is the area (Fig. 7), with the current locations of GOES EAST and GOES WEST, where we can produce cloud stereo. You can see it covers the US and just off the coasts. Again, if we move GOES WEST over to 120° W, and keep GOES EAST where it is now (satellite subpoint separation of 45° of longitude shown in Fig. 7), the eastern stereo area will go pretty far out in the Atlantic and cover hurricanes and extratropical storms off the east coast. But even where they are now, the area is not small. I feel it is very worthwhile doing stereo in conjunction with thunderstorm analysis over the U.S.

Audience: Meteosat scans from the south to the north and therefore sometimes we have problems making stereo. But with the new GOES won't we be able to scan south to north if we wanted to do stereo observations and combine them with Meteosat data?

Answer: That was the plan. The GOES I-M imager can do it. It can scan south to north. However, in the design of the software the program to do that was inadvertently left out, and due to the money crunch we went through not long ago that was one of the items that was not reinserted into the program.

Audience: I sure would like to address that in our various working groups, because one of the things we need for model initialization, I think, is cloud drift winds at the correct altitudes. Certainly with stereo that gives us a lot of the Atlantic where we can do a very good job of saying what are the heights of these cloud drift winds.

Answer: Absolutely. In fact, we are talking about a $100,000 software item, not a huge amount of money.

Audience: With 1.3 km one sigma error in registration how does that translate in stereo imaging height error?

Answer: We know from independent measurements that the height error is 500 meters, that is, we can do subpixel accuracy with the current GOES. With the next series of GOES, with the oversampling in the visible channel we are hopeful of 300 meter accuracy. That may mean that you have to manually register the data to do it. You may have to do landmark registration between images and bypass the planned 24 hour prediction system.

One of the questions often asked by thunderstorm research meteorologists has been how frequently do we need to take images of thunderstorms. This next chart (Fig. 8) shows 30 second interval pictures of the top of a thunderstorm, taken by a side looking camera in an airplane. You can see the overshooting dome. It is about 5 km across, and you can see how rapidly it changes. This series of photos shows that in order to capture thunderstorm morphology properly, 30 second imagery is a must. So that is why we built that capability into the GOES I-M. Initially we'll try it in a research mode and see how useful is the very frequent imagery.

This next figure (Fig. 9) came out of an excellent paper by Dennis Chesters that was produced a few years ago during the VAS Demonstration program. It shows a developing thunderstorm complex in Missouri which is moving eastward. At the bottom is a time history of the 6.7 μm data. What happens with these thunderstorms as they vertically developed is that they act as a partial obstruction to the flow. Look what happens on the upwind side of the Missouri thunderstorm as the air upstream from the cell complex hits this partially immovable object. You get a drying out of the atmosphere which is indicated by the warmer 6.7 μm temperatures on the west side of the thunderstorm complex. Now what this does to the local stability fields surrounding these cells, by producing more dry air aloft, I think, is to help destabilization in the immediate environment of the thunderstorm. To me, this is a fascinating case of scale interaction and the geosynchronous satellite data can provide new insights on this process.

One last point I would like to make about the future geosynchronous data, and that's the planning of data acquisition scenarios. Paul Menzel, Fred Mosher and I have put together a couple of papers which show how you can use GOES EAST and WEST to image the different areas outlined in Figure 10, primarily for convection monitoring and determining winds from cloud motions in association with severe local storm events. The areas are covered by 3 minute interval images from GOES EAST and from GOES WEST. The common area is within the triangles. That is where stereo can be done. We have also developed tropical cyclone scenarios.

Now I'm going to touch briefly on the products from NOAA K, L, and M which is a more mature system, so the changes in it, except for the microwave, are smaller. This is the NOAA/NESDIS list (Fig. 11). There is a primary list and a secondary list. The primary list is the same as the Day 1 list that I showed for GOES. The secondary list is the research type of products. Being a more mature system the primary list is longer. The primary things that NESDIS will be using imagery for include sea surface temperature, and cloud height. There are other areas like snow and ice mapping and in Fig. 11 you will see the resolutions, and the different sensors involved. Interestingly, the AVHRR is involved in more than just imaging products. It is used to support the sounding process as well. It will also provide the information on sea surface temperature which is the bottom level of the sounding. The AVHRR 3 will still have 5 channels at any one time. The most significant change will be that during the daytime there will be a 1.6 μm reflectance channel for distinguishing between ice and water clouds, and between snow and clouds. Then at night it switches over to the 3.8 μm channel that we've used before.

The AMSU will be the major new instrument on NOAA K, L, and M and since it will contain channels which can look at rainfall, I want to discuss its potential for rainfall measurements for a couple of minutes. I hope this doesn't steal too much of Stan Kidder's thunder, but I would like to show some airplane measurements in the millimeter frequencies and also show some data from the new DMSP SSM/I.

The next figure (Fig. 12) is from the 92 and 183 GHz aircraft radiometer that was flown on NASA ER-2. On the GOES image in Fig. 12 you can see a cloud line off the coast. The airplane covered the region outlined in green. At the eastern end of the line (top color panel) was some more developed convection where the radiometer measured backscattered radiation that gave a relatively cold equivalent blackbody temperature (140-150 K) as compared to the ocean background of temperature of 240-250 K. However, for clouds which did not reach the ice phase, the equivalent blackbody temperature depends on emission from water droplets and therefore the clouds appear warmer than the background. Thus, you get two types of signal, depending on whether the clouds reach the ice phase or not. Please note that these clouds are relatively small features; on the order of 5-10 km. AMSU will not be able to resolve such small features, but nonetheless the signal from the backscattered radiation will be sufficiently great to identify the areas of strong convection. The SSM/I data gives us further evidence that that's going to be the case.

There are two more general thoughts I would like to leave with you. One is how we can combine data from the low orbit and geosynchronous instruments (Fig. 13). These ideas are rather speculative, but I wanted to provide a few thoughts based on what I have just shown. One of the key areas for combining data is the measurement of precipitation. GOES adds the time history of the precipitation; something you don't get from low orbit. Also, the stereo capability provides accurate cloud heights as a function of time. The AVHRR, on the other hand, provides higher spatial resolution in the IR than GOES.. Also, high frequency microwave gives you some unique data including the possibility of several channels in the 183 GHz region to give you some idea of the vertical distribution of precipitation. Those channels will be part of the AMSU-B instrument.

The combination of low orbiting and geosynchronous measurements should also improve the estimates of cloud type; including multiple cloud layers. As you know, with visible and infrared data you are restricted to seeing mostly the top cloud layer. When you add the microwave, you should be able to determine whether or not water clouds exist beneath a cirrus deck. I would like to see that extra degree of freedom be brought into the analysis where we combine microwave from the low orbiting satellites with the time history from GOES visible and IR data along with the higher spatial resolution from the AVHRR.

The last thing I would like present is some ideas for the future; what we need to do in the imaging world to make further improvements (Fig. 14). We still need higher spatial resolution. We've made great strides in this area. In the visible I can make a good case for getting down to the 100 and 200 meter level (e.g., for winds from cloud motions, accurate cloud amounts). In the IR, we should improve to at least 500 to 1000 meters. In the microwave very high spatial resolution should be developed (1 to 2 km) to try to resolve individual convective cells. Perhaps if we had a microwave imager in geostationary orbit with < 5 km resolution we could track imbedded small cells much the way we track clouds now to obtain the winds.

Audience: Why do you accept a worse spatial resolution with microwave and infrared, than the visible?

Answer: With microwave 1 to 2 km should be sufficient to resolve individual convection cells. In the IR, I would actually like to see resolutions down to 100 to 200 meters. However I know that in geosynchronous orbit that's going to take about a 5 or 10 meter telescope which would be a huge investment.

Audience: Is this what you think is practical or what you think is necessary to do a meteorological job?

Answer: I think that these are the resolutions that will satisfy almost all of our meteorological requirements and will still be reasonably practical to achieve.

We also need more radiometric accuracy, better signal to noise ratios, and noise equivalent temperature difference improvements at least down to the 0.1 K level. Right now a quarter of a degree to a half degree is more common. We need to pay more attention to the diffraction smearing, particularly in geosynchronous orbit. How much of the energy is really coming from where we think we're looking? Channel to channel registrations need to get down to the 5-10% level. They're at the 20-40% level now. Then there are the effects of sensor response. Is the energy really coming from the place we think we are looking, or is it coming from several instantaneous fields of view back? We need to have low image jitter, especially for jitter that is less than or equal to seconds in frequency. Low frequency jitter we can generally deal with; we can model it out. High frequency jitter cannot be easily modelled. We need high earth location accuracy. I would like to see us achieve less than or equal to 1 km. I'd like to expand our sensing in geosynchronous orbit to include both land and ocean measurements as well as the atmosphere. There are degrees of freedom for the earth scientists and the ocean scientists that have never been explored like changing sun angles and thermal inertia. For oceans and land we can substantially reduce cloud and atmospheric interference by looking at a scene frequently. It also turns out when we stare at small areas the data rates go down by at least an order of magnitude below what you need with low orbiting satellite to achieve similar resolutions because the low orbiting satellite is moving at approximately 7 km/sec relative to the earth's surface.

I hope that you can see that there is a vast new world that is coming to our doorstep with GOES I-M and the improvements to NOAA K, L, and M. There are improvements in radiometric sensitivity, time and spatial resolution, and other features of future sensor performance (e.g. sampling) that we need to take advantage of over the next decade. I appreciate Jim inviting me to give you a few ideas.

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FIG. 1. GOES I-M IMAGER SENSING PERFORMANCE

SPECTRAL CHANNELS	1	2	3	4	5
PURPOSE	CLOUD COVER	NIGHTTIME CLOUDS SURFACE TEMP.	WATER VAPOR	SURFACE TEMP.	SEA SURFACE TEMP. AND WATER VAPOR
WAVELENGTH (μM)	0.55 TO 0.75	3.80 TO 4.00	6.50 TO 7.00	10.20 TO 11.20	11.50 T0 12.50
S/N OR NEΔT-SPEC	150:1	1.4K AT 300K	1.0K AT 230K	0.35K AT 300K	0.35K AT 300K
S/N OR NEΔT PREDICTED	320:1	0.09K	0.34K	0.12K	0.15K

FIG. 2

NESDIS GOES I-M PRODUCTS DAY ONE
(PART 1)

PRODUCTS	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSOR
Cloud Parameters				
o Cloud Heights (Cloud Top Temp)	±50 mb	±10 km	N/A	Imager 11.2 um
Enhanced Data Sets (Imagery)				
o GOES Projection (GOESFAX) (WEFAX)	---[1]	VIS 1 km IR 4-8 km	N/A[2]	Imager 3 channels 1 composite[3] 3 channels
o Lambert Conformal (AWIPS-90)	---	VIS 1 km IR 4-8 km	N/A	Imager 5 chnls. plus 2 derived imagery[4]
o Polar Projection (AWIPS-90)		VIS 1 km IR 4-8 km	N/A	Imager 5 chnls. plus 2 derived imagery[4]
Atmospheric Parameters				
o Vertical Temperature Profiles (°K)	---[5]	30 km	40 levels 1000-0.1 mb	Sounder
o Layer Mean Virtual Temperatures (°K)	---[5]	30 km	14 layers 1000-10 mb	Sounder
o Vertical Moisture Profiles (Specific Humidity)	±30%	30 km	15 levels 1000-300 mb	Sounder
o Layer Precipitable Water (mm)	---[6]	30 km	3 levels 1000-300 mb	Sounder
o Total Precipitable Water (mm)	±10%	8 km	N/A	Imager
o Channel Brightness Temps (°K)	---[7]	30 km	3 km	Sounder

FIG. 2 - Cont'd

NESDIS GOES I-M PRODUCTS DAY ONE
(PART I CONT.)

AREAL COVERAGE	FREQUENCY	DATA GRID SIZE	USERS
CONUS E/W FD E/W	1/hr	50 km	NMC, NSSFC NHC
FD E/W FD E/W	2/hr 8/day	1/4/8 km 8/km	NWS Field Direct Broadcast, NCDC
CONUS E/W Hawaii Puerto Rico	2,4,12/hr 2,4,4/hr 2,4,4/hr	1/4/8 km	NWS Field NCDC
Alaska Northern Hemisphere	2,4,4/hr 2,2,0/hr	1/4/8 km	NWS Field NCDC
CONUS E/W; Adj. oceans	1/hr 8/day	80 km	NHS NSSFC, NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC, NSSFC NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC NSSFC, NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC, NSSFC, NCDC
CONUS E/W FD E/W	1/hr 4/day	8 km	NMC, NHC, NSSFC, NWS Field, NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC NSSFC, NCDC

FIG. 2 - Cont'd

NESDIS GOES I-M PRODUCTS DAY ONE
(PART II)

PRODUCT	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSOR
o Lifted Index	± 2°K	8 km	N/A	Imager
o Geopotential Heights (m)	8	30 km	14 layers sfc-10 mb	Sounder
o Thermal Wind Profiles (Gradient Winds)(m/s)	± 7 m/sec	30 km	14 layers sfc-10 mb	Sounder
o Moisture Analysis (Interactive)	---	8 km	6 levels	Imager
o Precipitation Estimates (Scofield, Interactive)	± 30%	4 km	N/A	Imager
<u>Data Bases</u>				
o Imager	N/A	1/4/8	N/A	Imager
o Sounder	N/A	10	N/A	Sounder
o Calibration	N/A	1/4/8/10	N/A	Imager/Sounder
<u>Winds</u>				
o Cloud Drift	Low level 2-7 mps High level 5-10 mps	---	± 50 km	Imager
o Moisture Drift	---	---	---	Imager
o Deep Layer Mean	5 m/s	60 km	1 layer	Imager/Sounder

FIG. 2 - Cont'd

NESDIS GOES I-M PRODUCTS DAY ONE
(PART II CONT.)

AREAL COVERAGE	FREQUENCY	DATA GRID SIZE	USERS
CONUS E/W FD E/W	1/hr 4/day	8 km	NMC, NHC, NSSFC, NWS Field, NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC, NSSFC, NCDC
CONUS E/W Adj. oceans	1/hr 8/day	80 km	NMC, NHC, NCDC, NSSFC
PD E/W (over water)	4/day	2.5° lat	NMC
CONUS E/W Hawaii Puerto Rico	2/hr	---	NMC, NWS Field, NWS RFC, NCDC
FD	2/hr	1/4/8 km	NCDC, NHC, Office of Hydrology
CONUS E/W Adj. oceans	1/hr	10	NCDC
---	---	---	NCDC
50N-50S E/W	4/day	---	NMC GTS, NCDC
50N-50S E/W	4/day	---	NMC GTS, NCDC
---	4/day		NHC, NMC, NCDC

FIG. 2 - Cont'd

[1] Dash indicates value will be added to list as information becomes known.

[2] N/A indicates value not applicable to this product.

[3] VIS/11.5 um IR composite (GOESFAX).

[4] Total precipitable water (AWIPS-90).
Lifted Index (AWIPS-90).

[5] Equivalent to NMC forecast (used as first guess).

[6] 20% improvement over NMC forecast (used as first guess).

[7] Equivalent to NEΔT values corresponding to each channel.

[8] Equivalent in meters to corresponding values for mean virtual temperatures.

AWIPS-90	Advanced Weather Information Processing System - 1990's
FD	Full Disk
GTS	Global Telecommunications System
NCDC	National Climatic Data Center
NHC	National Hurricane Center
NMC	National Meteorological Center
NSSFC	National Severe Storms Forecast Center
NWS	National Weather Service
RFC	River Forecast Center

FIG. 3

GOES IM Proposed Day 2 Products
(December 20, 1988)

Items on this list are proposed products based on NWS and other requirements. These are Day Two Products (dates of implementation not yet defined).

Products

A. Cloud Parameters

Cloud Heights (6.7, 7.3mu)

Cloud Heights (CO2)

Cloud Heights
Stereo (VIS/IR)

Cloud type and coverage
(ASOS PRODUCT)

Fog/Stratus Identification

Cirrus cloud heights (6.7/11.2um)

Cirrus/water vapor
differentiation

Convective cloud tracking

Cloud emissivity

Enhanced Data sets (Cloud Imagery)

GOES Projection

11.2/6.7 mu

FIG. 3 - Cont'd

C. Atmospheric Parameters

Precip Estimates
IFFA (Adler)
(Automated)

Lower troposphere moisture
(Split window, I or S)

Buoyancy parameter

Convective Cloud
Tops and intensity (Expansion;
trop penetration)

D. Ocean Surface Parameters

SST thermal composite

E. Data Bases

F. Radiation Budget (Daily)

Solar Insolation

G. Radiation Budget (Monthly)

H. Winds

Cloud Drift Mesoscale
Land/Water (VIS,11.2/3.8um

I. Ozone

Total Ozone

J. Land Surface Parameters

Fires

Surface heating rate

Surface Temp

FIG. 3 - Cont'd

Soil Moisture

Skin Climatology
  Two week

K. Aerosols

L. Storm Parameters

Dvorak Intensity

Hurricane Eye soundings

FIG. 4

POTENTIAL GOES I-M PRODUCTS BEYOND THE INITIAL PHASE

"DAY 2 + RESEARCH"

IMAGING	HIGH FREQUENCY	STEREO	MULTISPECTRAL TECHNIQUES
CONVECTION INTENSITY	X	X	
IMPROVED CLOUD MOTION WIND DETERMINATION ACCURACY	X	X	X
MESOSCALE WINDS FROM CLOUD MOTIONS	X	X	X
LOWER TROPOSPHERIC MOISTURE	X		X
CLOUD PARAMETERS (AMOUNT, EMISSIVITY, HEIGHT, ETC.)	X	X	X
IMPROVED SURFACE TEMPERATURE	X		X
SNOW COVER AND ICE MAPS	X		X
SOIL MOISTURE	X		X

Fig. 5

GOES I-M

IMAGING CHANNELS (μm)

TECHNIQUE/PARAMETER	0.55-0.75	3.8-4	6.5-7.0	10.2-11.2	11.5-12.5
1. Surface temperature and Lower Tropospheric Moisture					
(1) Day	S		S	P	P
(2) Night		P	S	P	P
2. Convection Intensity	P*		S	P	
3. Winds from Cloud Motions$^{\Delta}$					
a. Mesoscale					
(1) Day	P*		S	S	
(2) Night			S	P	
b. Global	P*		S	P	
4. Cloud Parameters					
a. Type	P	P	P	P	S
b. Height	P*		P	P	S
c. Amount	P			P	
5. Mid Tropospheric Water Vapor			P		
6. Circulation Features (e.g., Jet Streams)	S		P	P	
7. Snow Maps	P				
8. Soil Moisture				P	
9. Radiation Balance	P	S	S	P	S
10. Forest Fires	S	P		P	

P - Primary S - Secondary

$^{\Delta}$ Best cloud motion results will also use cloud parameter products

* Stereo

FIG. 6

GOES I-M IMAGING PRODUCT IMPROVEMENTS

	HIGHER IR SPATIAL RES.	BETTER NEΔT OR S/N	E-W VISIBLE CHANNEL OVERSAMPLING	CALIBRATION IMPROVEMENTS	HIGHER DIGITIZATION	BETTER CHANNE TO CHANNEL REGISTRATION
WINDS FROM CLOUD MOTIONS	X		X			
SURFACE TEMPERATURE	X	X		X	X	X
CLOUD PARAMETERS	X	X	X	X	X	X
CONVECTION INTENSITY	X	X	X	X	X	
LOWER TROPOSPHERIC MOISTURE	X	X		X	X	X
PRECIPITATION	X	X	X	X	X	
DUST AND AEROSOLS	X	X	X	X	X	
SNOW AND ICE		X	X			

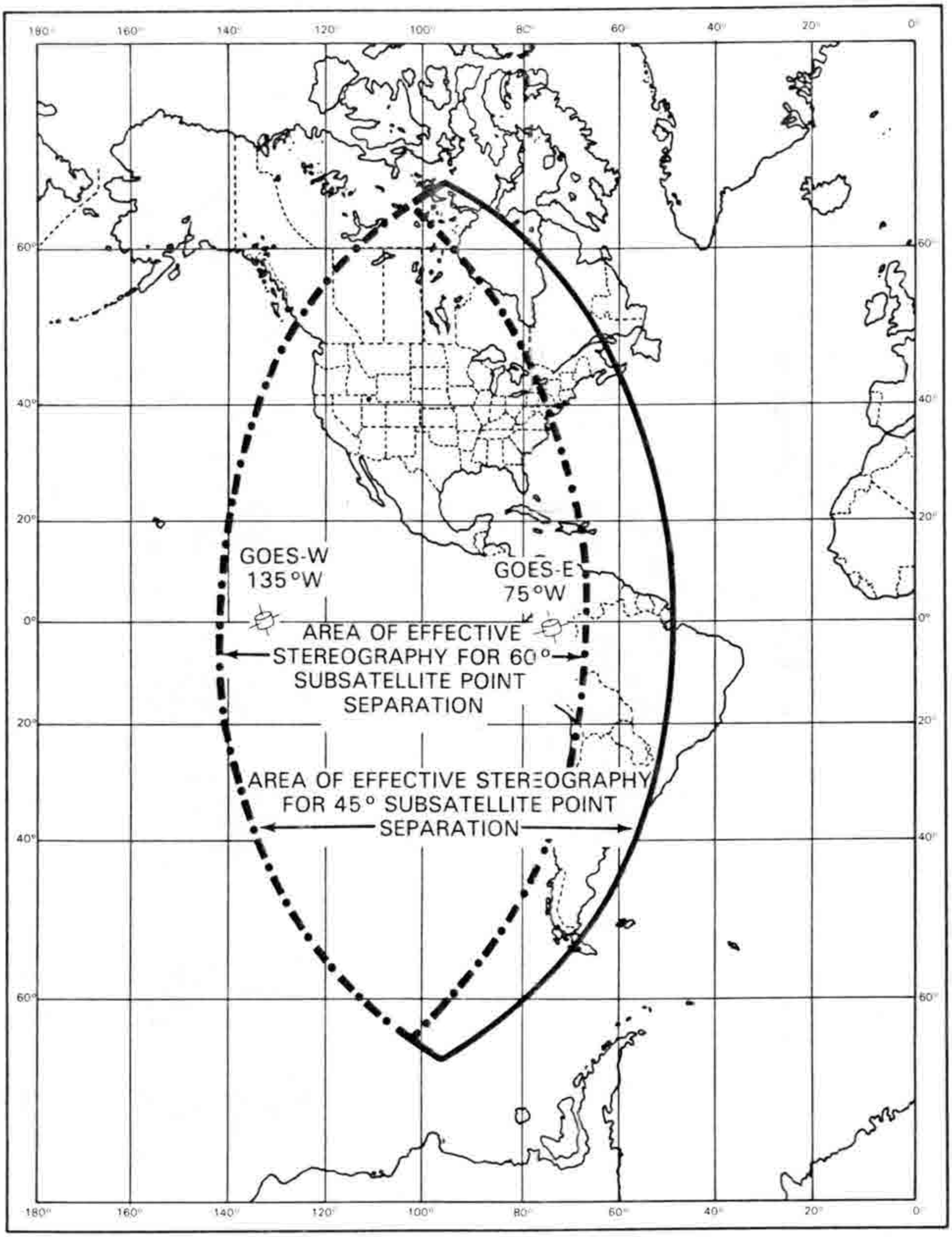
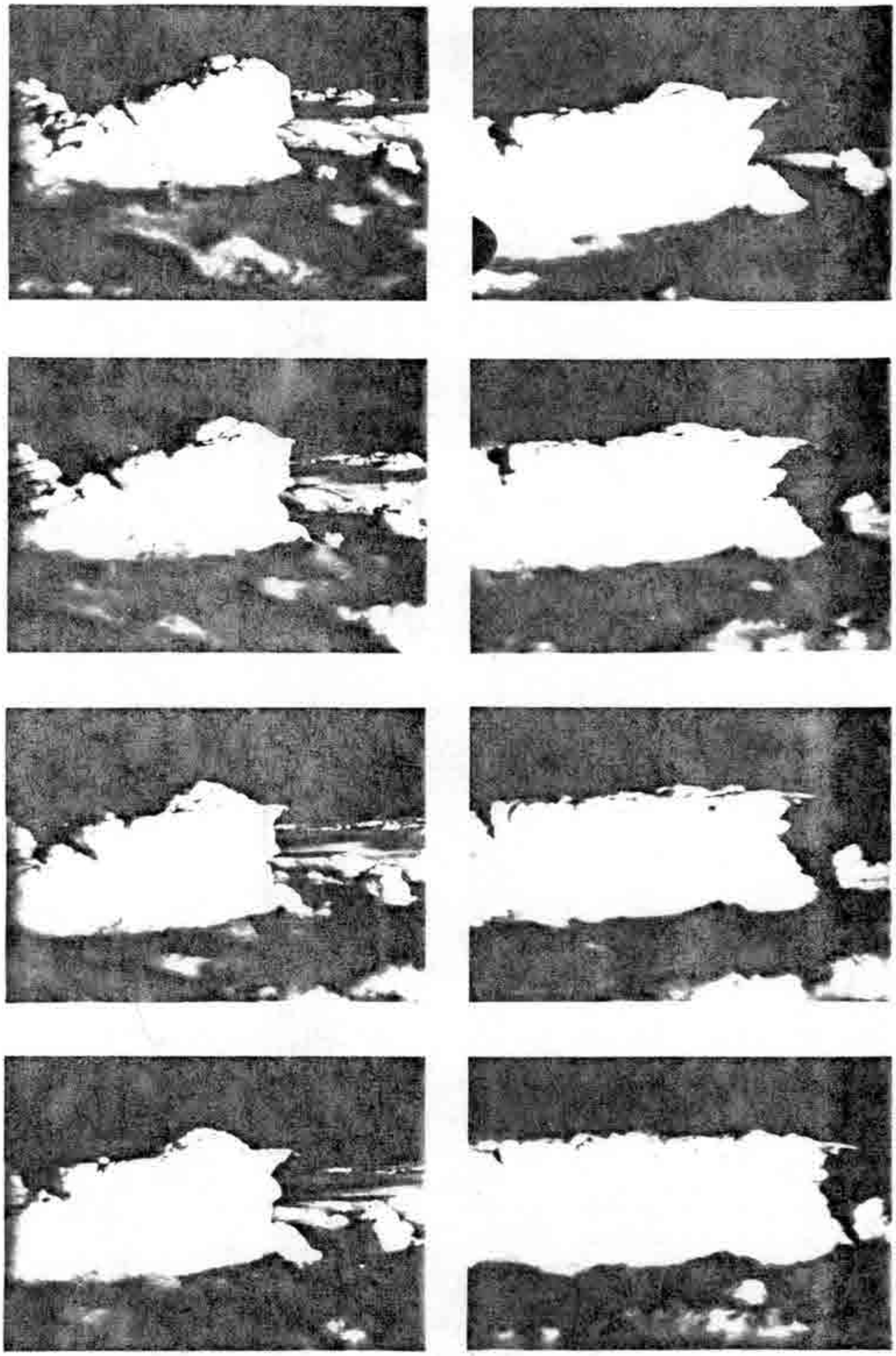


Fig. 7 MERCATOR PROJECTION MAP OF THE WESTERN HEMISPHERE, SHOWING THE EFFECTIVE STEREO COVERAGE OF GEOSYNCHRONOUS SATELLITES SEPARATED BY 45° AND 60° OF LONGITUDE. IF GOES-W WERE MOVED TO 120°W, STEREO COVERAGE WOULD BE SUBSTANTIALLY IMPROVED (SATELLITE SEPARATION OF 45°)

SEQUENCE SHOWING THE COLLAPSE OF AN OVERSHOOTING THUNDERSTORM TOP OVER JUNCTION, TEXAS ON MAY 6, 1973



1849 CST

1849½ CST

1850 CST

1850½ CST

1851 CST

1851½ C

1852 CST

1852½ CS

Fig. 8

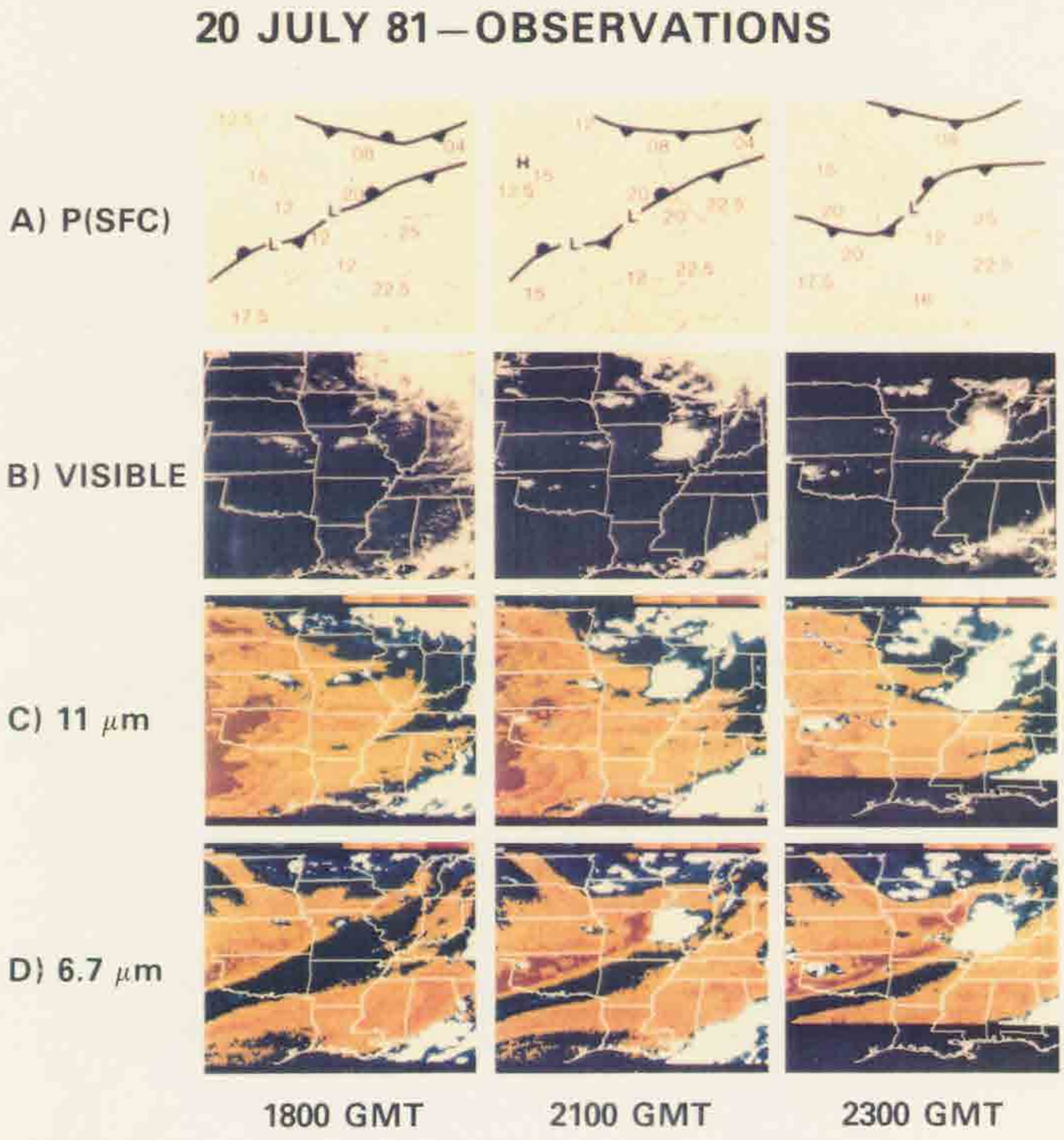

20 JULY 81—OBSERVATIONS
A) P(SFC)
B) VISIBLE
C) 11 μm
D) 6.7 μm
1800 GMT
2100 GMT
2300 GMT

Fig. 9

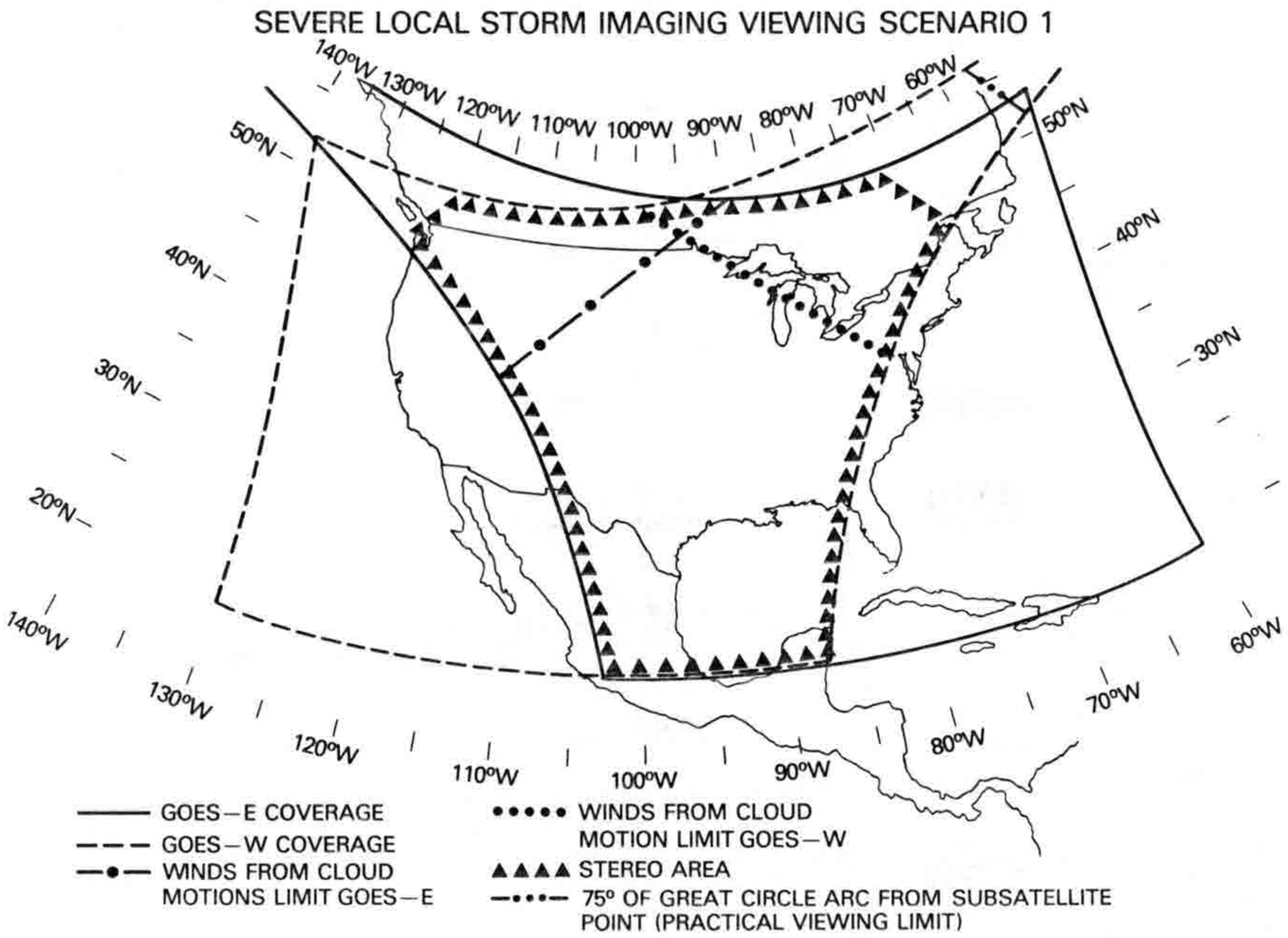

SEVERE LOCAL STORM IMAGING VIEWING SCENARIO 1

Fig. 10

FIG. 11

TABLE V-2. NOAA-K, L, M PRODUCTS LIST

I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
A. CLOUD PARAMETERS						
I.1A Cloud Liquid Water (non-precipitating clouds)	±0.1 milimeter	50 km at nadir increasing with scan angle	N/A	AMSU	GLOBAL	Every Orbit
B. IMAGERY						
I.1B Polar Stereographic Mapped Mosaics Visible, Day IR, Night IR	±5 km	1024x1024 Mesh/Hemisphere	N/A	AVHRR/3	GLOBAL	Daily
I.2B Mercator Mapped Mosaics -Visible, Day IR, Night IR	±5 km	Equatorial Strip 360 deg Longitude 80 deg latitude	N/A N/A	AVHRR/3	GLOBAL	Daily (Shared Processing)
I.3B Stretched Gridded Single Orbit Visible, Day IR, Night IR	±5 km	4 km	N/A	AVHRR/3	GLOBAL	Every Orbit (Shared Processing)
I.4B Polar Stereographic AMSU Mapped Mosaics, Channels 16 and 17		1024 X 1024 (See Appendix D)	N/A	AMSU	GLOBAL	Daily
I.5B Polar Stereographic Full Resolution Mapped Images	1.4 km 5.8 km	8192 X 8192 4096 X 4096	N/A	AVHRR/3	LOCAL AREA	Daily
C. ATMOSPHERIC PARAMETERS						
I.1C Vertical Temperature Profiles (Point Temperatures)	Surface -700mb,±2.5 700mb-Trop.,±2.0 Trop.-2mb,±2.5 2mb-0.1mb,±3.0 OVER WATER Surface-Trop.,±2.0 Trop.-2mb±2.5 2mb-0.1mb±	50 km at nadir increasing with scan angle	Minimum of 40 levels; sfc-0.1 mb	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)
I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
C. ATMOSPHERIC PARAMETERS (CONTINUED)						
I.2C Vertical Water Vapor Profiles (g/Kg)	Surface -700 mb, ± 2.5 700mb-Trop., ± 2.0 Trop.-2mb, ± 2.5 2mb-0.1mb, ±3.0 OVER WATER Surface-Trop., ± 2.0 Trop. -2mb ± 2.5 2mb-0.1 mb ± 3.0	50 km at nadir increasing with scan angle	Minimum of 15 levels; sfc-0.1 mb	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
I.2C.a Total Precipitable Water (cm)	±0.6 cm over land ±0.3 cm over water	50 km at nadir increasing with scan angle	N/A	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
I.2C.b Layer Precipitable Water (mm)	±20% - Land ±10% - Water	50 km at nadir increasing with scan angle	3 layers sfc-300 mb	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
I.3C Quantitative Precipitation (None, Light, Medium, Heavy)	4 Categories (TBD)	15-30 km	N/A	AMSU/AVHRR/3	GLOBAL	6-12 Hours (Shared Processing
I.4C Clear Equivalent Blackbody Temperatures for 20 HIRS, 20 AMSU and 3 AVHRR Channels (deg K)	± 1 Deg C	50 km at nadir increasing with angle	N/A	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
I.4C.a Thickness Values (M) Byproduct of I.4C	Equivalent to accuracy goal for Layer Mean Vertical Temperature	50 km at nadir increasing with angle	Minimum of 20 layers sfc - 0.4 mb	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)

I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
C. ATMOSPHERIC PARAMETERS (CONTINUED)						
I.5C Layer Mean Vertical Temperatures (Deg. K)	0.25 Deg C more accurate than Vertical Temperature profile layers given under 1C	50 km at nadir increasing with angle	Minimum of 20 layers sfc - 0.4 mb	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
D. OCEAN SURFACE PARAMETERS						
I.1D Global SST Observations	0.5 Degree C.	8 km spaced 8-25 km Apart	N/A	AVHRR/3	GLOBAL	Every 6 Hours (Shared Processing)
I.2D Local SST Observations	0.5 Degree C.	2 km spaced 2-11 km Apart	N/A	AVHRR/3	U.S. coastal waters and selected areas	Every 6 Hours
*1.3D Monthly Mean SST	0.5 Degree C.	1.0 Degree Lat/Long	N/A	AVHRR/3	GLOBAL 75 S - 80 N	Monthly
*1.4D Semi-Monthly Mean SST	0.5 Degree C.	1.0 Degree Lat/Long	N/A	AVHRR/3	GLOBAL 75 S - 80 N	Twice/Month
*1.5D Global SST	0.5 Degree C.	0.5 Degree Lat/Long	N/A	AVHRR/3	GLOBAL 75 S - 80 N	Twice/Week
*1.6D Regional SST	0.5 Degree C.	1/16 - 1/32 Degree Lat/Long	N/A	AVHRR/3	U.S. coastal waters and selected areas	Twice/Week
*1.7D Local SST	0.5 Degree C.	1 - 4 km	N/A	AVHRR/3	U.S. coastal waters	Twice/Week
*1.8D Ocean-Feature	1 km location	1 - 4 km	N/A	AVHRR/3	U.S. coastal	Twice/Week

* Produced by Ocean Products Center

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)
I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
E. ICE AND SNOW COVER						
*I.1E Edge	1 km	Line Position	N/A	AVHRR/3 AMSU	GLOBAL	Weekly (Shared Processing)
*I.2E Cover	+5%	100 km2	N/A	AVHRR/3 AMSU	GLOBAL	Weekly (Shared Processing)
*I.3E Type: New, First Year, Multiyear, Melting	(TBD)	1000 km2	N/A	AVHRR/3 AMSU	GLOBAL	Daily (Shared Processing)
I.4E Snow Cover Area	± 10%	25 km	N/A	AVHRR/3 AMSU	GLOBAL	Weekly
F. DAILY RADIATION BUDGET						
I.1F Albedo	TBD	2.5 Degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
I.2F Daytime Outgoing Longwave Radiation	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
I.3F Nighttime Outgoing Longwave Radiation	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
I.4F Daily Average Outgoing Longwave Radiation	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
I.5F Available Solar Energy	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily

* Produced by Joint Ice Center

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)

I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
F. DAILY RADIATION BUDGET (CONTINUED)						
I.6F Absorbed Solar Radiation	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
I.7F Net Radiation	TBD	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Daily
G. MONTHLY MEAN RADIATION BUDGET						
I.1G Daytime Outgoing Longwave Radiation	5.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly
I.2G Nighttime Outgoing Longwave Radiation	5.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly
I.3G Absorbed Solar Energy	5.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly
I.4G Available Solar Energy	5.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo	N/A	AVHRR/3	GLOBAL	Monthly
I.5G Albedo	0.005%	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly
I.6G Daily Averaged Outgoing Longwave Radiation	5.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly
I.7G Net Radiation	7.0 w/m^2	2.5 degree Mercator & 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL	Monthly

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)
I. PRIMARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
I. OZONE						
I.1I Total Ozone (Dobson Units)	±15 % Tropical ±30 % Polar	50 km at nadir increasing with scan angle	N/A	HIRS/3	GLOBAL	Every Orbit
I.4I Total Ozone (Dobson Units)	± 1.0%	200 km sub-satellite	N/A	SBUV/2	GLOBAL	Daily
I.5I Level Ozone (Mixing Ratio)	± 5%	200 km sub-satellite	16 levels	SBUV/2	GLOBAL	Daily
I.6I Layer Ozone (Dobson Units)	± 5%	200 km sub-satellite	11 layers	SBUV/2	GLOBAL	Daily
J. VEGETATION INDEX	TBD	15 km	N/A	AVHRR/3	GLOBAL	Daily

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST
II. SECONDARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
A. CLOUD PARAMETERS						
II.s.2A Cloud Top Temperature (Deg K)	1.0-5.0 Deg K increasing with decreasing cloud amount	50 km at nadir increasing with scan angle	N/A	AVHRR/3 HIRS/3 AMSU	GLOBAL	Every Orbit
II.s.3A Cloud Top Pressure (mb)	15-60 mb increasing with decreasing cloud amount	50 km at nadir increasing with scan angle	N/A	HIRS/3 AVHRR/3 AMSU	GLOBAL	Every Orbit
II.s.4A Cloud Amount (%)	±10% increasing with decreasing cloud amount	50 km at nadir increasing with scan angle	N/A	AVHRR/3 AMSU HIRS/3	GLOBAL	Every Orbit
II.s.5A Cloud Composition (Type)	TBD	50 km at nadir increasing with scan angle	N/A	AVHRR/3 AMSU	GLOBAL	Every Orbit
C. ATMOSPHERIC PARAMETERS						
II.s.7C Tropopause Temperature (Deg K)	±1.5 Deg K - 2.5 Deg K	50 km at nadir increasing with scan angle	N/A	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit
II.s.8C Tropopause Pressure (mb)	±40 mb	50 km at nadir increasing with scan	N/A	AMSU/HIRS/3 AVHRR/3	GLOBAL	Every Orbit

FIG. 11 - Cont'd

TABLE V-2. NOAA-K, L, M PRODUCTS LIST (CONTINUED)
II. SECONDARY

OBSERVATION	ACCURACY	HORIZONTAL RESOLUTION	VERTICAL RESOLUTION	SENSORS	COVERAGE	FREQUENCY
K. AEROSOL						
II.s.1K Optical Depth at 0.5 Microns	±0.02%	50 km	N/A	AVHRR/3	GLOBAL (Ocean Only)	7 Days
II.s.2K Size Distribution	TBD	50 km 125x125 Polar Stereo.	N/A	AVHRR/3	GLOBAL (Ocean Only)	7 Days

TROPICAL CONVECTION OVER WATER
17 SEPTEMBER 1979

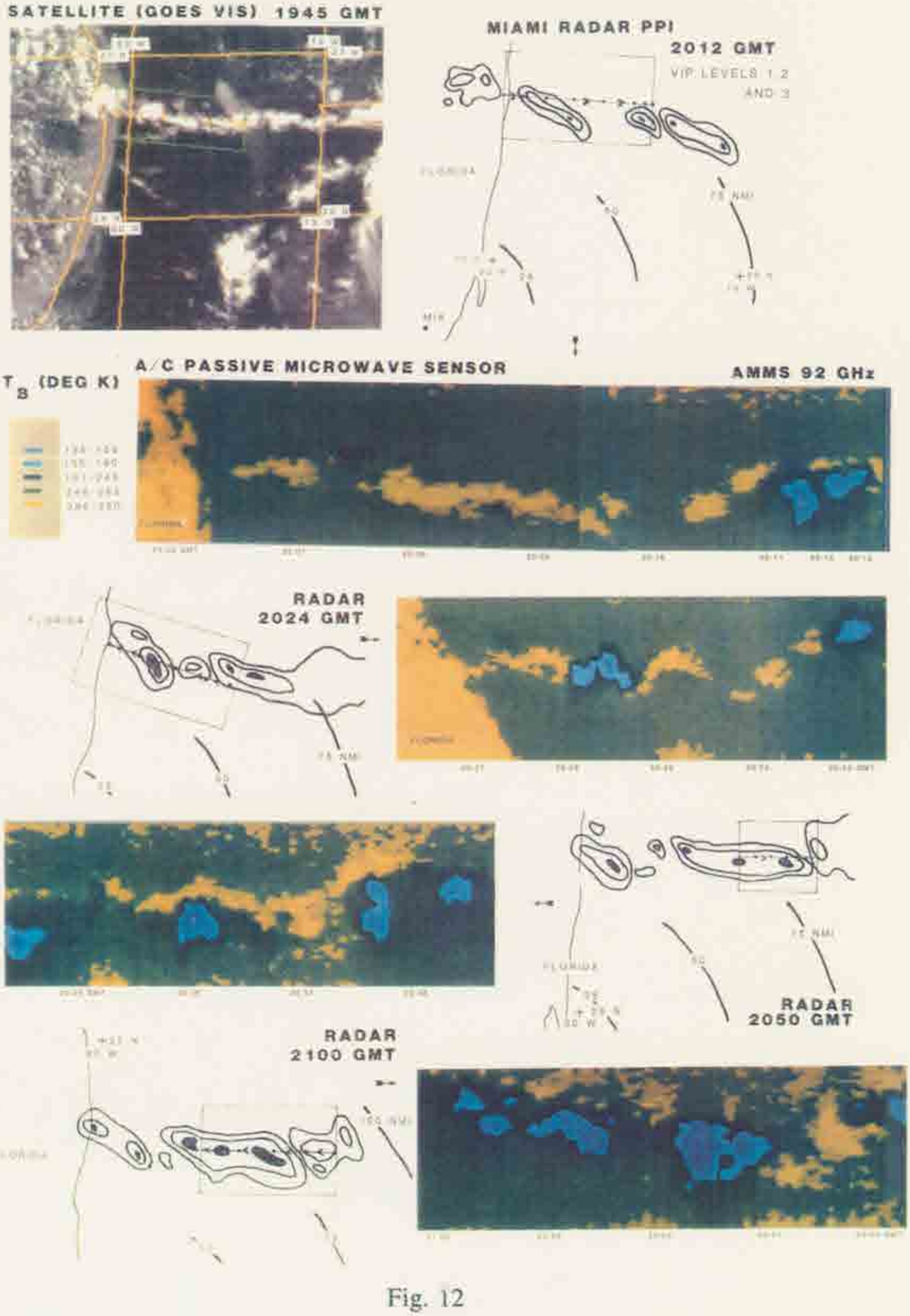


Fig. 12

FIG. 13

OPPORTUNITIES FOR COMBINING DATA FROM GOES I-M AND NOAA K-M

	PARAMETER	COMBINATION
1.	PRECIPITATION	GOES IMAGER, AVHRR, AMSU (90, 183 GHZ)
2.	CLOUD TYPE (INCLUDING LAYERS, IMBEDDED CONVECTION)	GOES IMAGER, AMSU, AVHRR
3.	SEA SURFACE TEMPERATURE	AVHRR, AND AMSU (183 GHZ), GOES IMAGER, GOES SOUNDER
4.	TEMPERATURE AND WATER VAPOR PROFILES	GOES IMAGER AND SOUNDER, AVHRR, TOVS
5.	CLOUD TOP HT.	1. GOES STEREO WITH ANY RADIATION DERIVED METHOD 2. STEREO BETWEEN AVHRR AND GOES IMAGER

FIG. 14

FUTURE IMAGING RECOMMENDATIONS

- HIGHER SPATIAL RESOLUTION FOR THE ATMOSPHERE--ULTIMATELY TO:
  - 100-200 M - VISIBLE
  - 500-1000 M - IR
  - 1-2 KM -- MICROWAVE
- MORE RADIOMETRY
  - S/N, NE T IMPROVEMENTS
  - DIFFRACTION, MTF, ENCIRCLED ENERGY
  - CALIBRATION
  - CHANNEL-TO-CHANNEL REGISTRATION
  - SENSOR RESPONSE
- LOW IMAGE JITTER - ESPECIALLY < SECONDS
- HIGH EARTH LOCATION ACCURACY < 1 KM (3)
- EXPAND GEO SENSING TO INCLUDE CHANNELS FOR OCEAN AND LAND MEASUREMENTS
  - REDUCED CLOUD AND ATMOSPHERE INTERFERENCE
  - CHANGING SUN ANGLE AND THERMAL INERTIA AS INFORMATION
  - LOWER DATA RATES THAN LOW ORBIT

Satellite Sounding

Talk given by William L. Smith
At the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

I want to raise a few points. One of them is related to Bob McClatchey's question (because it is very important one) that is: Are we making the best use of the satellite data that we have available now. The answer is both yes and no; no in some very critical situations, which I'll show you in a moment. Yes, in that even the best use of today's data isn't going to produce satisfactory results. The later point is that we need to encourage our administrators to implement the new sounding technology that is available now. GOES-Next could have been a lot more than it is going to be on the basis of technology available during its design. It is not as big a jump forward as it should have been based on available technology. So the two points I'd like to discuss are; (1) Better use of current soundings in critical weather situations, and (2) the need to implement the much better available satellite sounding technology as soon as practically possible in order to achieve the forecast results needed.

I think I will start with a recent example. I want to reemphasize some of the synergisms that Bill Shenk addressed; that is, we have satellite systems and we want to make use of them to solve environmental problems. Most of our emphasis would be on the meteorological problems although some of it could be placed on land resources and other environmental phenomena. We had a very important tropical storm last week, named Gilbert, the most intense Atlantic tropical storm, at least as far back as we have kept records. I hate to pour salt into open wounds but the forecasts were terrible. They were not good for Gilbert at all. In fact the official forecast kept dragging this storm north into the gulf. There was a preparedness alert put out all along the U.S. Gulf coast, which I suppose was the conservative thing to do except for the fact that it cost the U.S. taxpayers many millions of dollars to prepare for a hurricane disaster. The official forecast kept driving Gilbert up into the Galveston area where there was lots of bucks spent to defend against its predicted destruction. Maybe the worst thing that can happen the next time a similar situation arises is the 36-48 hour forecast put out for a tropical storm land fall won't be taken seriously because the Gilbert forecast was so bad. I cannot tell you exactly why the Gilbert forecast was bad but I know that one contributing factor was that the quantitative satellite data that was available was not used as extensively as it should have been.

What I'm talking about now is the wind data that are produced operationally by the VDUC system at the World Weather Building; a system we have spent a lot of manpower and dollars on developing for operational use. I'm going to use a couple of illustrations which show Gilbert at a very intense stage before it went ashore on the Yucatan peninsula. As shown in the first figure (Fig. 1), which is the official 72 hour forecast, Gilbert is being driven northward; I think based largely on the MFM numerical forecast. The green curve shown is the actual track out to 72 hours. The blue curve is the barotropic forecast, (which isn't too far astray, from the actual track) and it was obtained using a wind analysis based on a high density of wind data that was provided by the GOES satellite. These winds are estimated from cloud motions and water vapor motions. They are produced routinely by the VDUC system. The "operational" winds (not produced by the VDUC system) are by contrast very sparse. They are so sparse that the analysis system doesn't pay that much attention to them.

The next illustration (Fig. 2), shows the density of the winds. The density is much higher than what's produced operationally. These were produced routinely by VDUC (although I don't think VDUC is considered an operational system yet).

The official hurricane trajectory forecast comes out of Miami. It's based on a lot of models, including climatology, but I think what was largely responsible for the official forecast "bust" in the case of Gilbert was the numerical problem. The numerical model did not have the benefit of the dense wind information that was available over the Gulf of Mexico during Gilbert's traversal. You would be hard pressed to find a half a dozen winds in the Gulf that were produced by the "operational" system. From the few low, middle, and upper level winds that were produced operationally, you can see that the flow in the environment of that storm is easterly.

The next illustration (Fig. 3) shows the deep layer mean flow which is generally indicative of storm motion at least during the next 24 hours or so. So the indicated forecast for storm motion at sea is strongly westerly, or maybe slight west-northwesterly. The models produce what you would expect at 72 hours later. Model forecasts are critically dependent on their initial data and my point here is that there were a lot more initial data available to go into the models than were used for forecasting the motion of Gilbert.

The next figure (Fig. 4) is presented to show that this failure in the operational forecast due to lack of wind data was a consistent occurrence even when the storm was right over the Yucatan peninsula. The experimental forecast based on all the satellite wind data put the Gilbert landfall on the Mexican coast, near Southern Texas. The official forecast at 48 hours had the storm taking a very sharp jag to the north and into Galveston. The slide shows the actual trajectory of the hurricane and again the barotropic forecast, based the high density wind analysis, was much more accurate than the "official" forecast. NMC needs to study the impact of high density winds in a situation like this on the NGM forecast.

Bill Shenk: I would like to make one more comment about Gilbert. To draw conclusions from small samples is always dangerous, but in Cammille the same forecast problem occurred. They were trying to recurve that thing well before it did. And we speculated as we were watching this storm, the same thing might be happening with Gilbert, namely, great storms like this tend to reinforce the ridge on the outflow side. That appeared to be what was happening in Cammille and what we thought was happening here too. Those winds should have helped to determine that.

Smith: I don't want to presume that the "official" forecast would have been different, necessarily, but the fact is that it did not use the dense coverage of winds that were produced by a system that was put in there for that purpose. The density of the data makes a difference in how well the analysis, and consequently the numerical forecast, pays attention to them.

I could show many examples of the variety of things that can be done with satellite data. What's important is that the data coming out of the satellite gets used. We have good useful data now but it's not all being used effectively. That's particularly true of soundings. Let's look at a few systems just to see what's currently available. Bill Shenk addressed this subject pretty well. We have a polar orbiting infrared/microwave sounder system which has a low spectral resolution which translates into a low vertical resolution. We have the DMSP system which is very capable in the microwave. I will show you an example where its spatial resolution is a limiting factor. We have the AMSU that is coming along in the future. The Air Force is putting a similar capability on a future DMSP satellite around 1992, called SSM/T2. In the geostationary arena we currently have the VAS. We also just heard from Bill Shenk about GOES-Next capabilities.

The next overlay (Fig. 5) gives some examples of weighting functions which describe the vertical resolution of the sounding systems. Shown are the TOVS IR, TOVS microwave, VAS IR and the SSM/T channels. The biggest difference in the capability of these systems is not the vertical resolution of these systems, which is relatively poor in all cases if you look at the width of these weighting functions which are 10-12 km in half width. Instead, consider the very distinct difference in the spatial resolutions seen here. The DMSP microwave sounder is about 200 km resolution versus 100 km resolution for the MSU. The infrared resolution improves from the polar orbiter to the geostationary satellite by a factor of 9 (i.e., 8 km versus 24 in linear resolution).

What impact does sounding radiance spatial resolution have with respect to hurricane Gilbert of last week? Shown in the next illustration (Fig. 6) are just two infrared channels of Gilbert. This is the position of the storm in the MSU channel 2 image, mainly by the strong attenuation due to the rain. You can see the eye in this 4 μm infrared window image fairly well. You can see that 4 μm radiation penetrates most of the cirrus. This is the IR channel at 11 μm, in which you can more clearly distinguish the rain bands. Of course, you can also see rain bands in the microwave data, this green area here, as opposed to this area which is high brightness temperature which is also seen to be clear in the infrared. The spatial resolution limits the ability to differentiate the intensity of these rain bands as one would like to. AMSU will help solve that problem. MSU channel 3, which looks in the upper troposphere, looks more or less at the thermal pattern of the storm rather than the precipitation pattern of the storm. This particular channel is not affected by precipitation very much and you can see the intense warm core of the storm. This image was not taken when the storm was at its most intense stage. Actually, it was about 30 knots or so weaker than the most intense status which was before it landfalled.

The next illustration (Fig. 7) is a pattern of the 250 mb temperature distribution as derived from the MSU data. The horizontal gradient is from about -30°C at the center to an environmental temperature of about -40°C. So it's about 10° across here as seen by the MSU.

For comparison, the next illustration (Fig. 8) shows the 250 mb isotherms for the same storm as seen a day earlier by the SSM/T. The storm was more intense at this stage, but the horizontal gradient as derived from SSM/T profiles is about half as great. There is a -35°C at the center versus -40°C in the environment. The resolution of SSM/T is one fourth the spatial resolution of MSU, leading to a degradation in the ability to detect the horizontal gradient of atmospheric temperature actually associated with that particular phenomena. We cannot relax our requirements on the spatial resolution. The SSM/T has more channels than MSU. It should do a better job on temperature profiling than the MSU, but you see that the horizontal gradient is half as great and that is due to the one-fourth horizontal resolution of that sensor. The AMSU is going to help tremendously. First of all our AMSU sounding capability is going to improve. We've got more channels. The vertical resolution, however, is not significantly better than the SSM/T and MSU. The horizontal resolution is considerably better. Another factor of four over MSU in area resolution should help greatly. We probably want to go even higher than that, if technology permits it. The water vapor channel sounding capability will be much more improved.

Let me talk a little bit about my earlier statment that better technology could be used, or should have been used especially for GOES-Next. I don't want to sound too negative, but I want to stimulate some ideas, some concerns, and some pressures. I'm talking about the vertical resolution limitation of the current sensors. Looking at the infrared spectrum (Fig. 9) which is covered by our current sounders going from about 18 μm down to 3.5 μm, we have channels which cover the spectral regions shown by these bars, but the resolution is not very good if you look at the spectrum in detail. This is brightness temperature as a function of wave number. All these wiggles in the spectrum are due to the individual absorption lines due to various constituents like carbon dioxide, ozone, and water vapor (which is very important). Current systems smear over these spectral features. That translates into a direct smearing of the vertical profiling resolution.

Look at the variation in brightness temperature at fairly high resolution (Fig. 10). You can see over a very small wavenumber region, say about two wavenumbers or so, that the brightness temperature can vary as much as 60°C, measuring emissions ranging from the tropopause to very near the surface. This is the width of our current filter radiometers. The filter smears over the vertical structure due to this spectral smearing effect. The technology is available to do the necessary resolution, say by using a Michelson interferometer such as the High Resolution Interferometer Sounder (HIS) we now fly on the NASA ER-2 aircraft.. The HIS gives us higher resolution at individual wavelengths, due to a spectral resolution 30 times greater and also provides several thousand radiance measurements rather than the dozen or so available from current systems. It also turns out that the spectrum is not in any way redundant when viewed at high resolution. All the weighting functions that you get by looking at the spectrum don't overlap completely. Some finite degree of independent information exists throughout the spectrum, so there is more to be gained than what you can get from a dozen or so or broad band channels. You have to take advantage of this independence. Weighting functions at 0.2 wavenumber resolution are a lot sharper than those of current systems. The improved spectral resolution together with the much greater number of spectral channels translates into about a factor of two to three improvement in vertical resolution. This factor is quite critical with regard to the utility of temperature and moisture sounding data in meteorological forecasting.

Jim Purdom: In the foreseeable future, until we get up an instrument like the HIS instrument, we are really confined to using the sounding data through some type of model or a technique to derive perhaps a detailed sounding. But the basic data are just going to give us broad mean layer information. So we should think in terms of a broad layer mean as we work toward the HIS instrument getting at narrower layers. Can we take the broad mean layer temperature and through coupling it with a model add resolution?

Smith: We might think of doing things of that nature in our research. One of the big problems with todays sounding data is that they are vertically averaged temperatures and the problem is that the atmosphere is coupled horizontally and vertically. If you can't resolve a phenomena in terms of its vertical scale, it's not going to do you much good to resolve it in terms of its horizontal scale. That's certainly true of water vapor and temperature. So one ultimately needs the high vertical resolution to have the full impact of these data. For large scale general circulation models the sounding data are probably adequate and are doing a very effective job, as has been shown by impact experiments. Here we are dealing with models that are resolving scales of 500 km or greater, but we are now moving towards global models that resolve scales that are on the order of 100 km or even less; with these models we are going to run into very severe limitation of the current satellite data. The problem is the time scale is not consistent with the vertical resolution scale. This resolution inconsistency is what is now limiting our use of the VAS temperature sounding data, for example, in mesoscale models. Water vapor is somewhat of an exception to this limitation. In terms of temperature accuracy limited by vertical resolution, we need to move from our current capability of 1.5 to 3°C down to 1°C. With regards to water vapor our current capability is in the 20% or more arena as you move upward in the atmosphere. This again will be improved by moving toward higher spectral resolution and a greater number of channels.

Let me show a few examples of the type of high resolution vertical coverage we are talking about. The next illustration (Fig. 11) shows results from aircraft data that were achieved with the HIS instrument. Shown also is a radiosonde during this COMEX case. The COMEX mean, a mean of all the radiosondes for the COMEX period, is used as initial data for the HIS solution of the actual temperature profile and is also shown. The green is the retrieval obtained with the high spectral resolution data over the radiosonde station at the time of the radiosonde release. You can see that it's not perfect. There are still differences of a degree or two. They are consistent with the expected errors in the radiosonde and HIS profiles. Comparing Figures 11 and 12, you can see that the high resolution system can retrieve vertical structures that can not be resolved by the current operational system. The dash curves are dew point temperature and the solid curves are air temperature.

The final idea I want to leave you with is unproven. The vertical resolution from HIS is particularly good for water vapor because water vapor is an exponentially decaying quantity with altitude. Even for relatively low spectral resolution systems, the weighting functions for water vapor channels are relatively sharp compared to carbon dioxide channels which are used for temperature sounding. That is because the carbon dioxide is uniformly mixed constituent. A high resolution system, which measures many water vapor channels will have a distinct advantage in getting fine scale water vapor features. If you have the capability from a geostationary spacecraft to look at the three dimensional distribution of water vapor in time, you will be able to track water vapor features in relatively narrow layers. I show this viewgraph just to show once again with an aircraft instrument that we now have this capability; one can resolve very small scale features of the water vapor distribution. This is a vertical crossection; the water vapor structure between the surface and the aircraft level of 60 mb is observed as the aircraft traverses along the horizontal track. The horizontal resolution of these data are about 2 km, consistent with the vertical detail that's resolved by this kind of instrument. If we have this capability on a geostationary satellite, we might have a fairly reasonable solution to the wind profile problem. It appears that we could achieve at least 2 km vertical resolution, which is a lot better than we have now over most of the world.

A few additional considerations. We've got to think about amalgamating active systems with passive systems if we want to make great improvements in sounding capability. Certainly the lidar systems, particularly the DIAL (DIfferential Absorption Lidar) systems, have been demonstrated from the ground and from aircraft for the measurement of constituent profiles such as water vapor profiles and ozone. If we think about coupling that capability with a passive interferometer or high resolution spectrometer, one can conceive of getting temperatures with very high vertical resolution using water vapor emission and water vapor soundings. You can use those high vertical resolution water vapor weighting functions to solve for high vertical resolution temperature features if the water vapor distribution is known. You're going to get much higher vertical resolution than you can achieve now using CO_2 and H_2O_2 emission. The combination of active and passive radiometry should provide extremely high resolution water vapor and temperature soundings. I think we can demonstrate this by putting together some data that has already been achieved from aircraft. The ultimate system should be some combination of these two types of sounding techniques.

To conclude, let me reiterate the two points I made at the beginning of my talk. We've got to make better use of the data that is available now. It is not being used as effectively as it should be or could be. The second point is: we've got to use the technology that is available now. We're flying on GOES-Next, a sounding radiometer that is essentially equivalent to that which we flew on NIMBUS 6 in 1975. That is a 15 year old technology. We need to get the much improved, currently available technology in orbit at the earliest possible date.

DISCUSSION

Bob McClatchey: I have two or three comments. First of all, Bill deserves a lot of credit for pushing his idea for high resolution measurements, even sometimes when people in the community have not been very supportive of it. I think that there is more information there. I think there really is more at stake. There was another comment that Jim made during the course of Bill's presentation. It raised a question, why don't we try to get more information out of the existing profiles by ordering the data in between. I think that is a dangerous thing to do. In fact, we ought to be working harder toward obtaining truly physical retrievals, not dependent on other statistical information or even first guesses for that matter. But a physical retrieval that attempts to get out of the data what's in the data and avoids the potential problem of introducing something that really isn't there, and then trying to make meteorological sense out of it later. That concerns me a lot with that approach to the problem.

Regarding the high resolution technique, people need to understand that although the weighting functions are made somewhat narrower, the real advantage is having closely-spaced, but still somewhat broad, weighting functions. What you're trying to do is get the information out of the differences between closely-spaced channels and their respective weighting functions. The question is how accurately do the radiometric measurements need to be made in those adjacent channels in order to get out the useful information.

The last comment is relative to your last chart. You ought to add radar, so when we start talking about active as well as passive systems, we really ought to start looking more seriously at radar from satellites, because we need to find ways to look through the clouds and within the clouds with accurate systems to get the high resolution in the presence of clouds, not limiting ourselves to lidar which can't see through clouds.

Smith: I'll respond to a couple of your comments. They are very good ones. One regarding buggering the profiles with ancillary information, there is a long standing debate on that particular problem. The problem with the models is they are so good that we can't see much independent information coming from today's radiances when added to the models. There are systems to put the radiances in the model, actually imbedding the retrieval into the analysis system. The natural process for incorporating those radiances is to take a forecast, calculate the radiances, compare it with the observations and then change the model if there is significant difference. The problem is that most of the time there's not a significant difference, and the reason for that is because the measurement vertical resolution is poor. We've got to increase the vertical resolution; the ECMWF model operates over more than a dozen levels at a 125 km horizontal resolution. With regards to GOES-Next, I don't think there will be a significant difference from VAS in sounding capability, except from the point of view of being able to sound more often and in the area of your choosing. There won't be higher absolute accuracy. I don't think there's going to be significantly higher precision of measurement. What I've seen in the latest specs is disappointing. More accuracy would help. But we haven't changed the physics of the measurement. The jump won't come until we take that quantum jump in spectral resolution. Even then it's going to be marginal. We're talking 1°C accuracy and 1-3 km vertical resolutions.

Jim McGuirk: Let me make two comments. You said that in the comparison of model generated and observed radiances you get very large differences, implying that there's lots of good information left in the sounding data that is not getting in the models. Finally, I get very discouraged when I hear you talk about the magnificent systems, that will come along, when I see magnificent systems we've built in the past. For example, VAS has been up 7 or 8 years and none of that information is getting into NMC models.

Smith: With regards to making pictures out of soundings, we're quite successful at trying to get the sounding data to be used for severe storm forecast products, where stability and water vapor images are created from the sounding data, so the forecasters can actually look at a picture of it rather than looking at numbers.

Bill Shenk: My first comment is on the signal-to-noise and the planned GOES-Next sounder. The latest information is we are going to be able to meet the specified signal-to-noise. I have the details in a chart much too busy to show, but that's what the latest information is and that's about two days old. Remember, I had a chart which had a number of parameters on one side and then several different ways that we might use the information, or program the satellite, or combine with other things. I had that for imaging and it turns out I had a similar chart for sounding. If the improvement with the HIS is 100% improvement over what we have now. If we try a few things like sounding frequently using soundings with multispectral imaging, using a longer dwell time than the one we had planned, like a 0.4 second or 0.1 second, combining the sounding with imaging data, or using the simultaneous sounding coverage from two satellites, we might take up 10 or 20% of that 100% that Bill might be talking about for a much better sounding. Beyond that point, it's relatively small compared to what Bill will be able to do with that better sounding. But for the profiling arena these sorts of operations should be able to help our accuracy, consistency, and coverage of the soundings to some degree beyond what we're doing now with the VAS. Also, we can use the sounding channels as imaging channels for a number of other purposes. These type of operations with the satellite will to some degree improve those sort of problems.

Smith: GOES Next will be an improvement over VAS, but we will still need much greater information content.

Jim Purdom: I guess the thing you can say about satellite sounding is it doesn't look like a rawinsonde, doesn't smell like a rawinsonde, it doesn't taste like one either. I guess it's not a rawinsonde. Maybe we've got to try to use the data from what it is. I think that's an area where we could become a little more actively engaged in research instead of trying to turn an elephant into a mouse; go ahead and use a herd of mice.

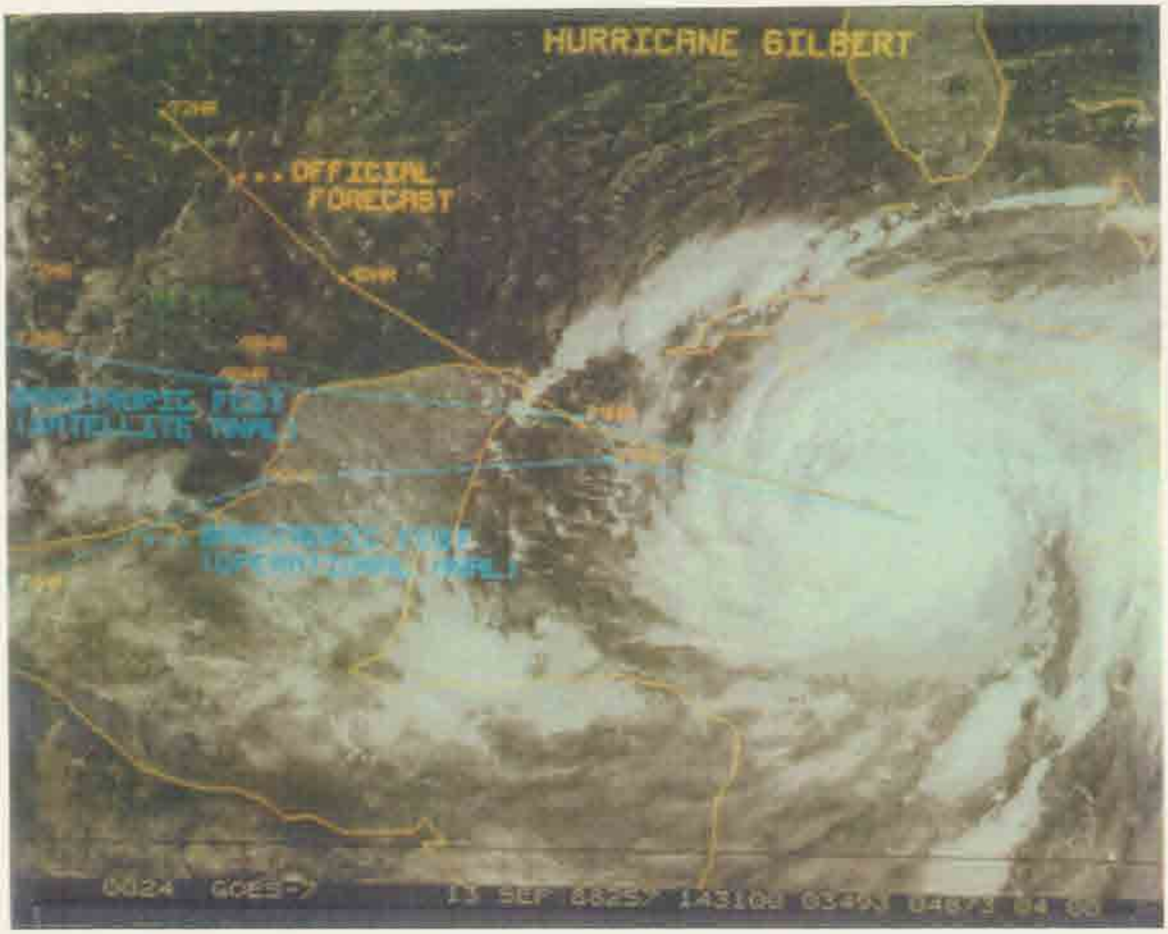


Fig. 1. 72-hour "official" forecast for Hurricane Gilbert (solid line), actual track (green curve), and a barotropic forecast (blue curve) based on GOES-VAS wind data. The tracks are overlaying an infrared image of Hurricane Gilbert.

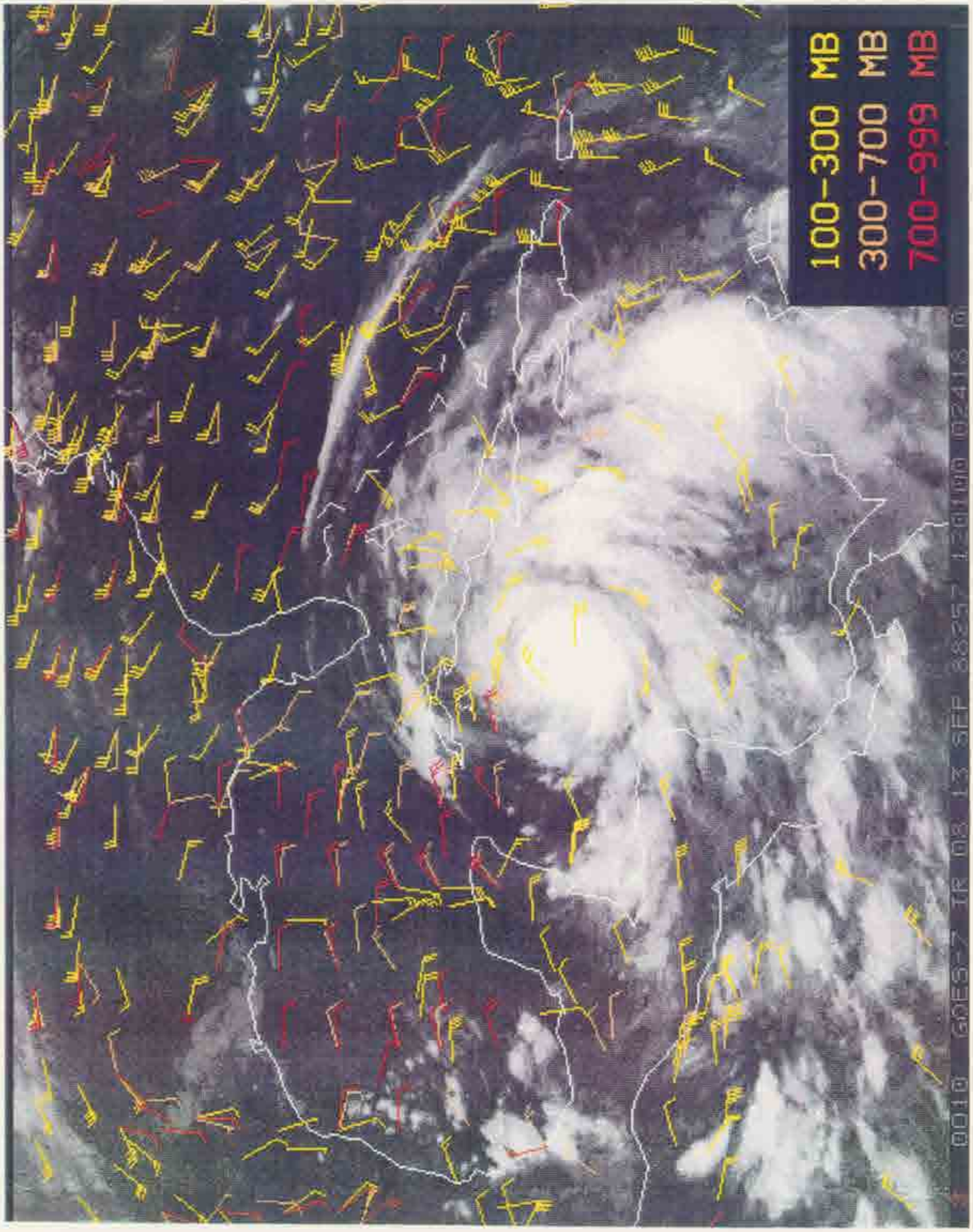


Fig. 2. Density of VAS cloud and water vapor "wind" data overlaying a water vapor radiance image.

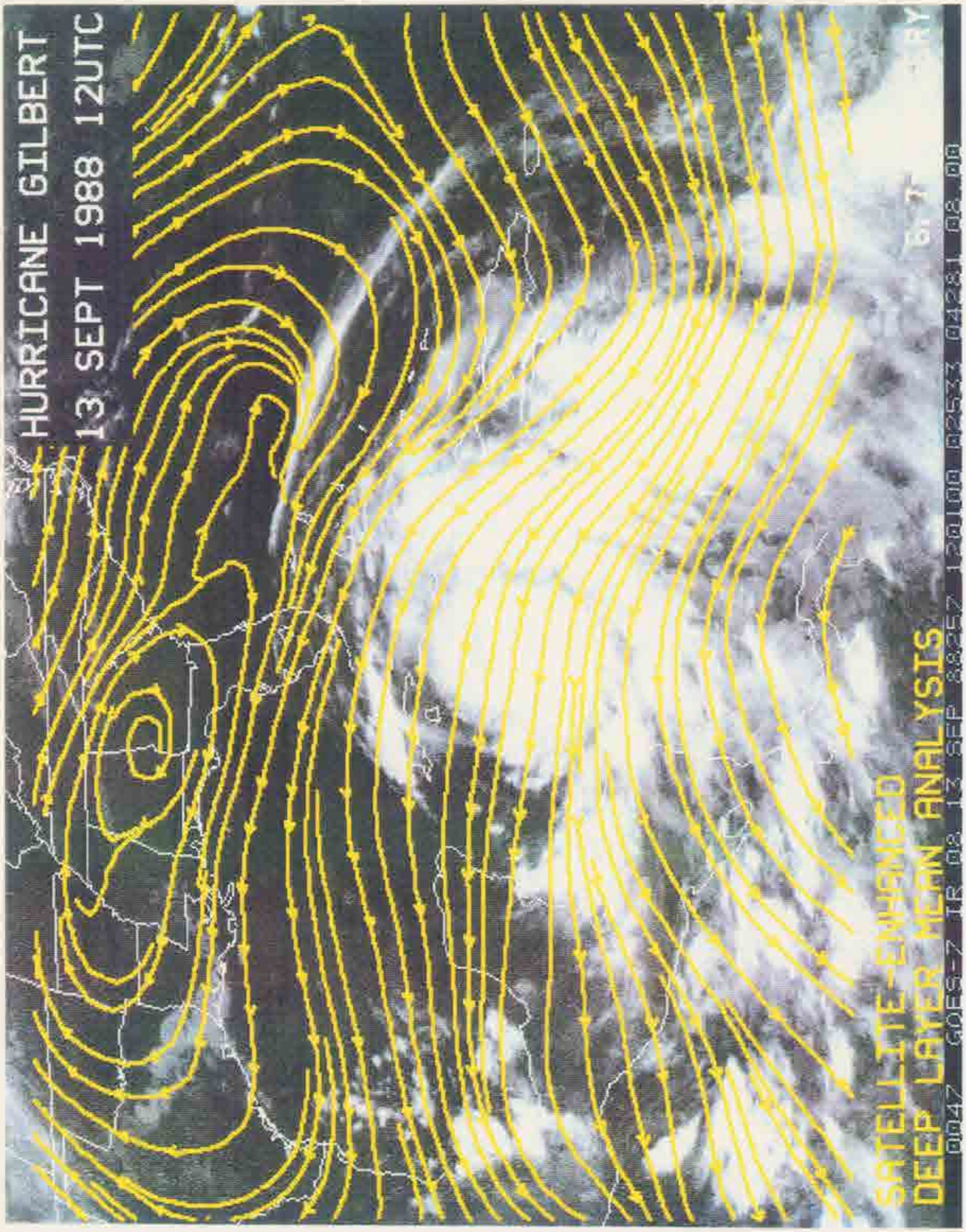


Fig. 3. Deep layer main wind field derived from VAS "wind" data overlaying a visible channel image of the storm.

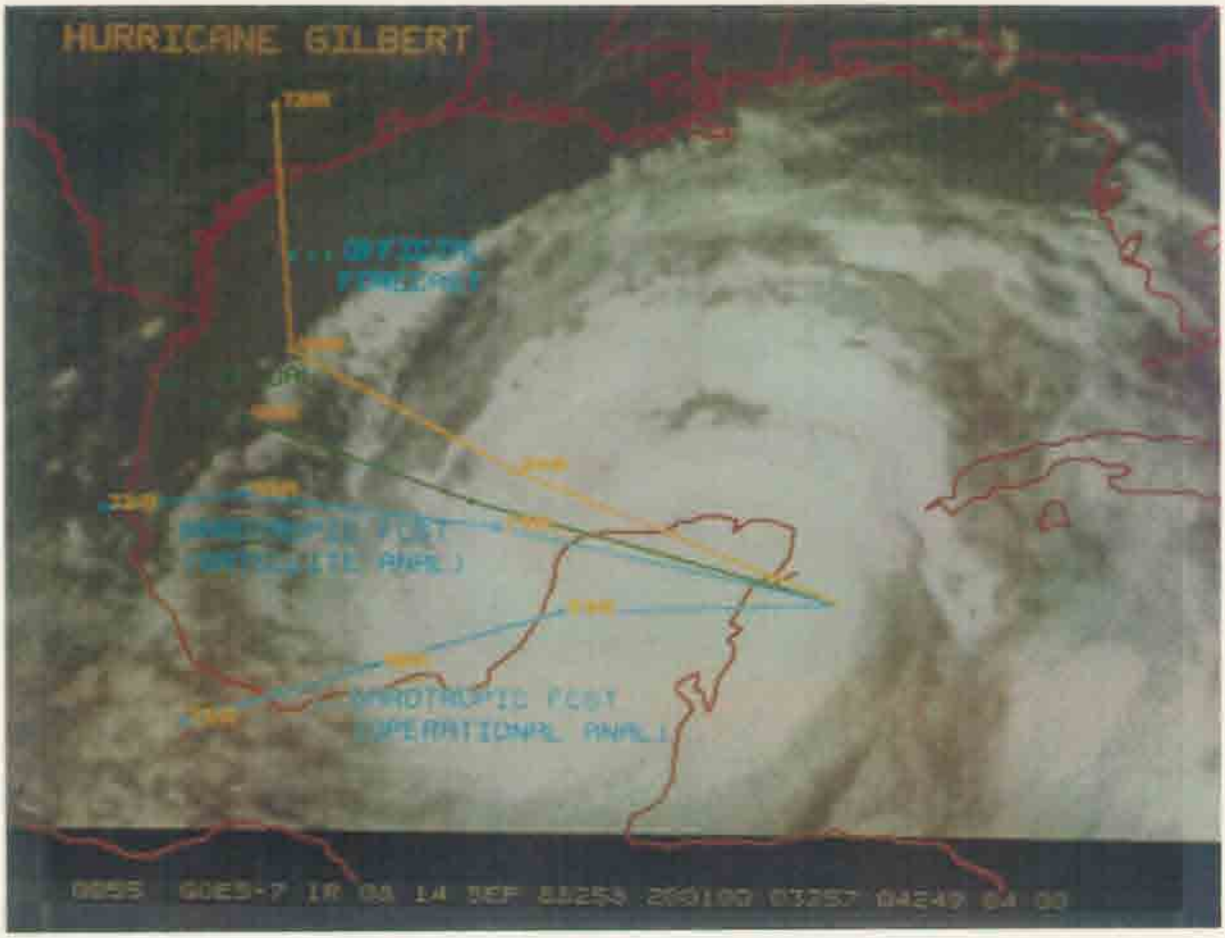


Fig. 4. 48-hour "official" forecast compared to a barotropic forecast based on high density VAS "wind" data.

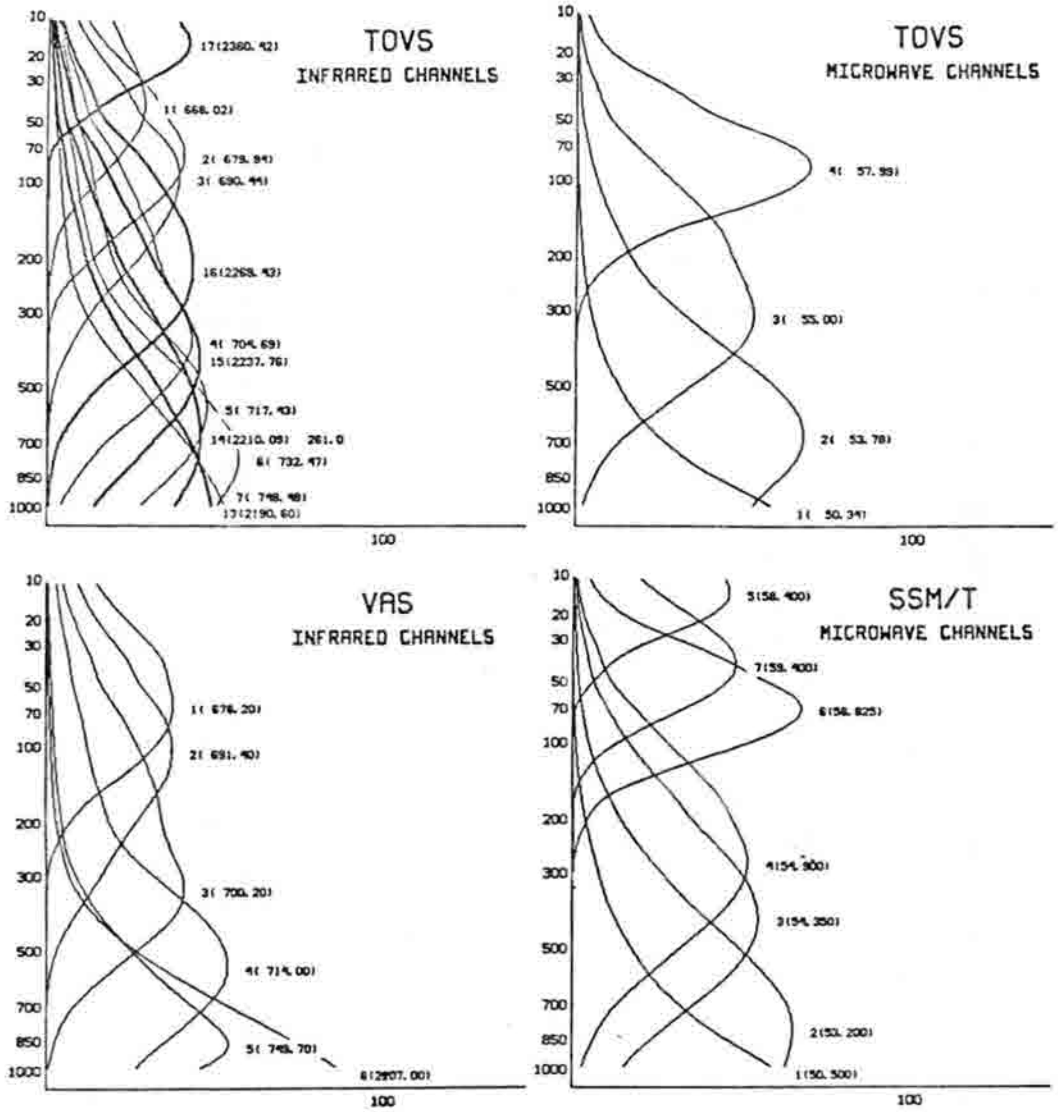


Fig. 5. Vertical profile "weighting" functions for TOVS, VAS, and SSM/T infrared and microwave sounding instruments.

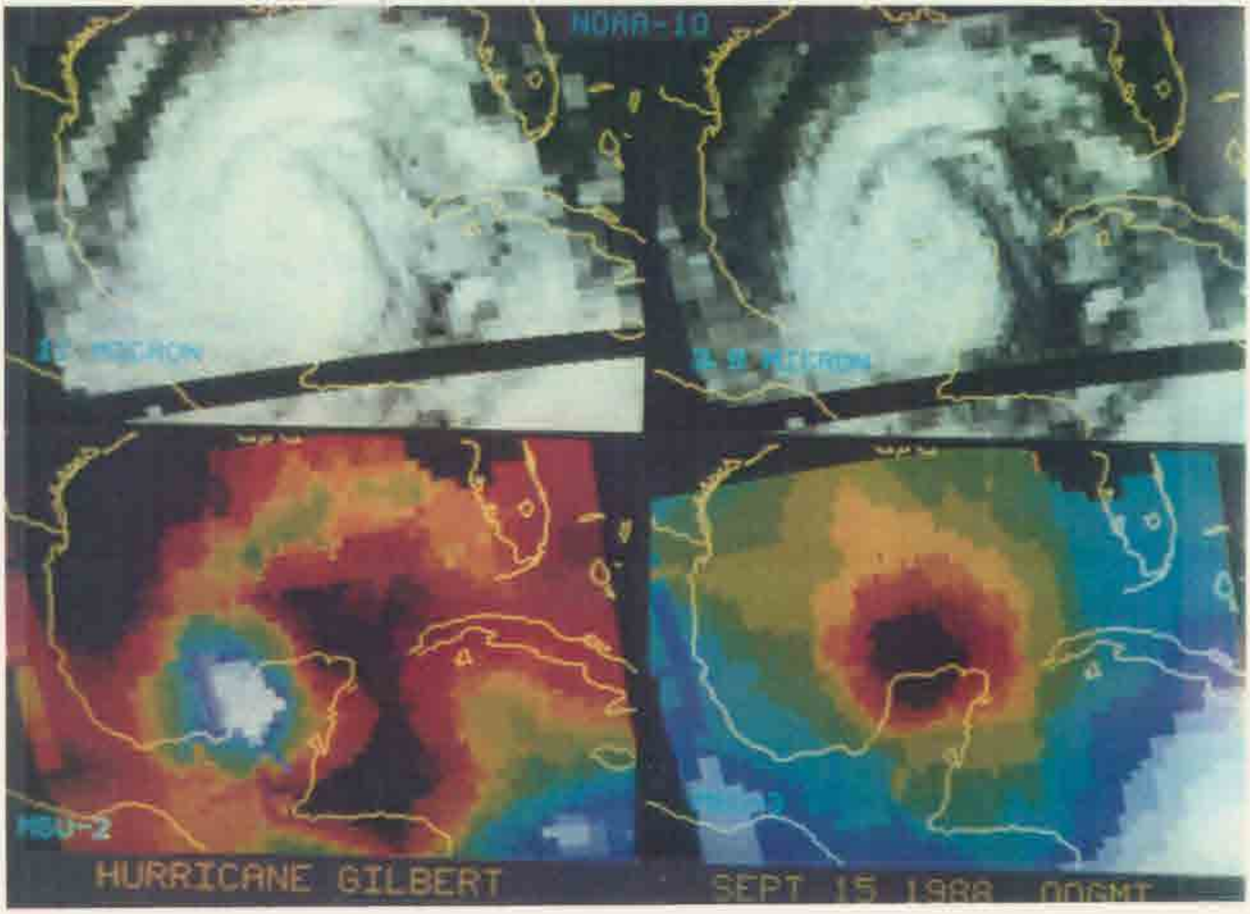


Fig. 6. (a) TOVS 11 μm and 4 μm infrared images of Hurricane Gilbert. (b) MSU channel 1 and channel 2 images of Hurricane Gilbert.

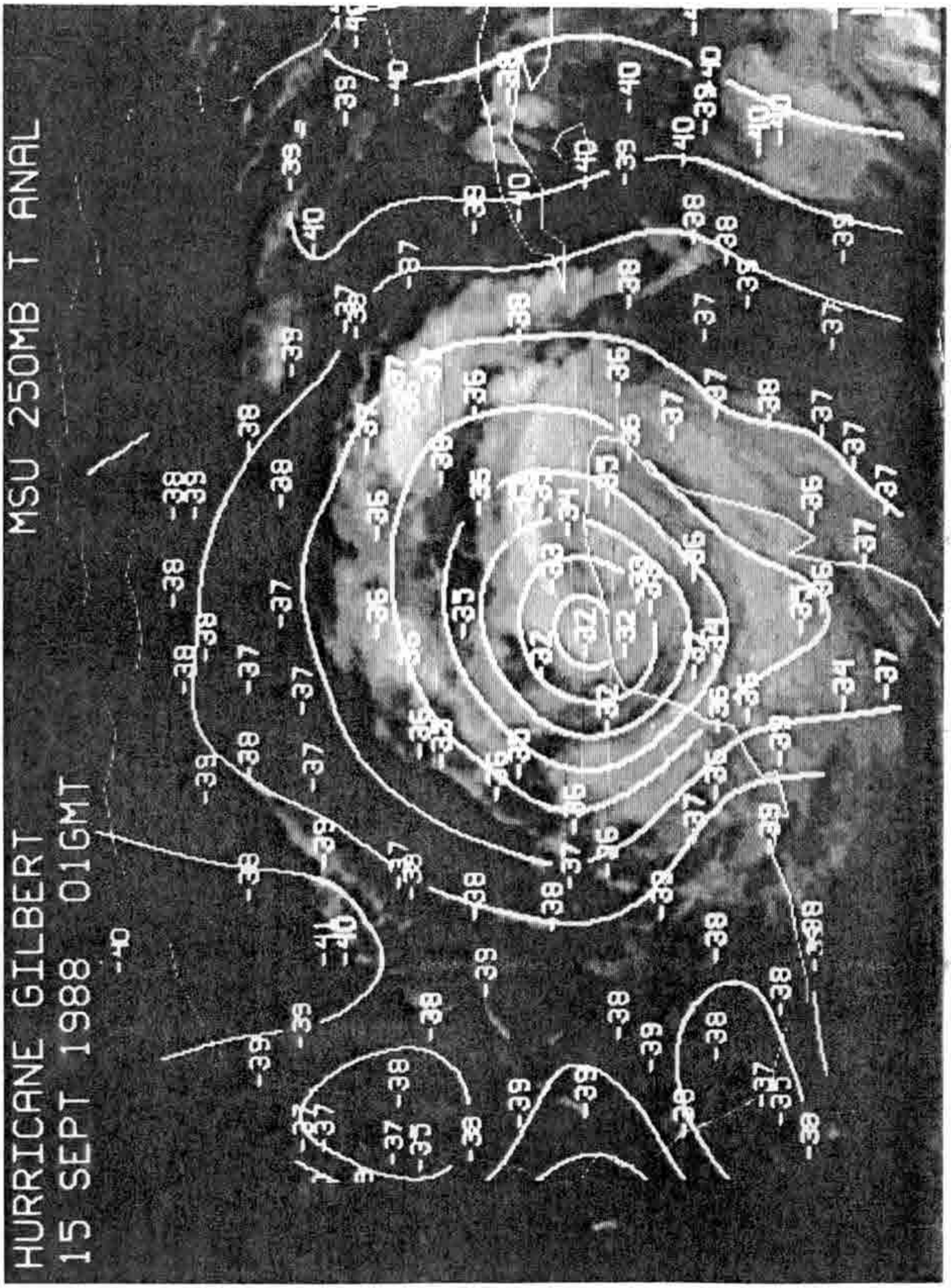


Fig. 7. Isotherms at 250 mb derived from MSU temperature profiles.

Fig. 8. Isotherms at 250 mb derived from SSM/T temperature profiles.

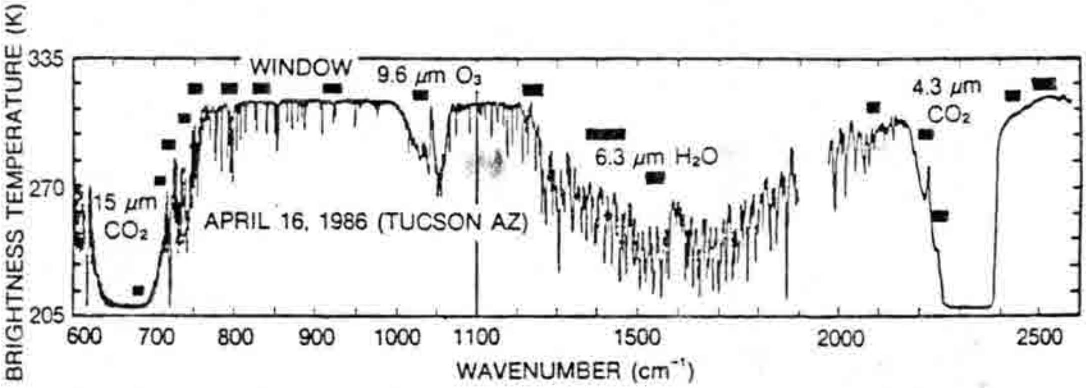


Fig. 9. HIS spectrum of upwelling radiance.

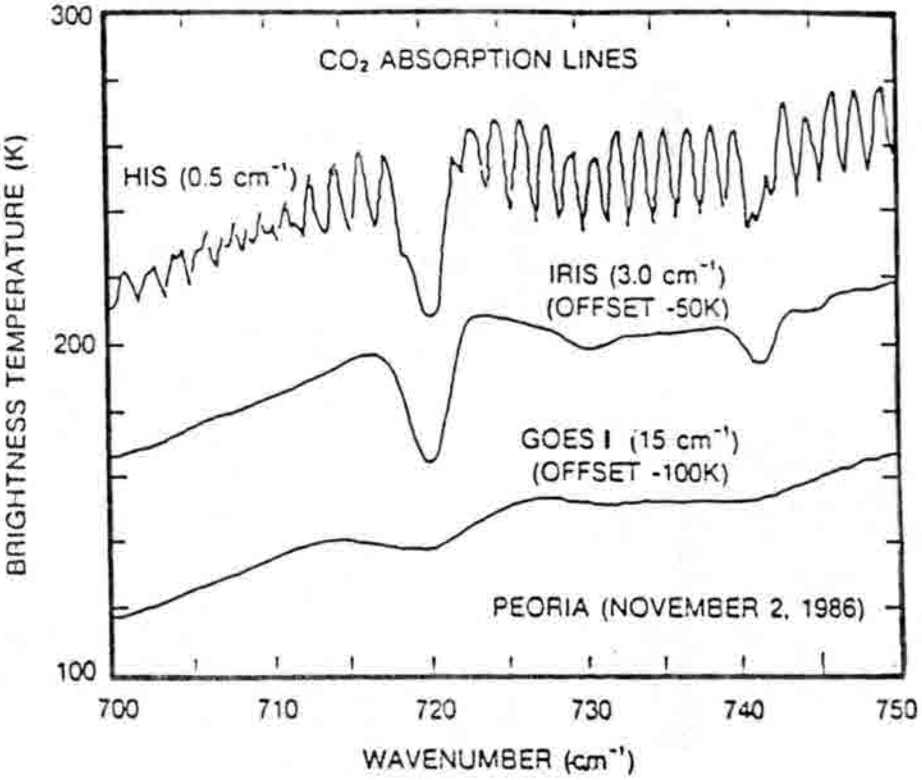


Fig. 10. Small portion of the 15 μm thermal emission spectrum observed at several spectral resolutions.

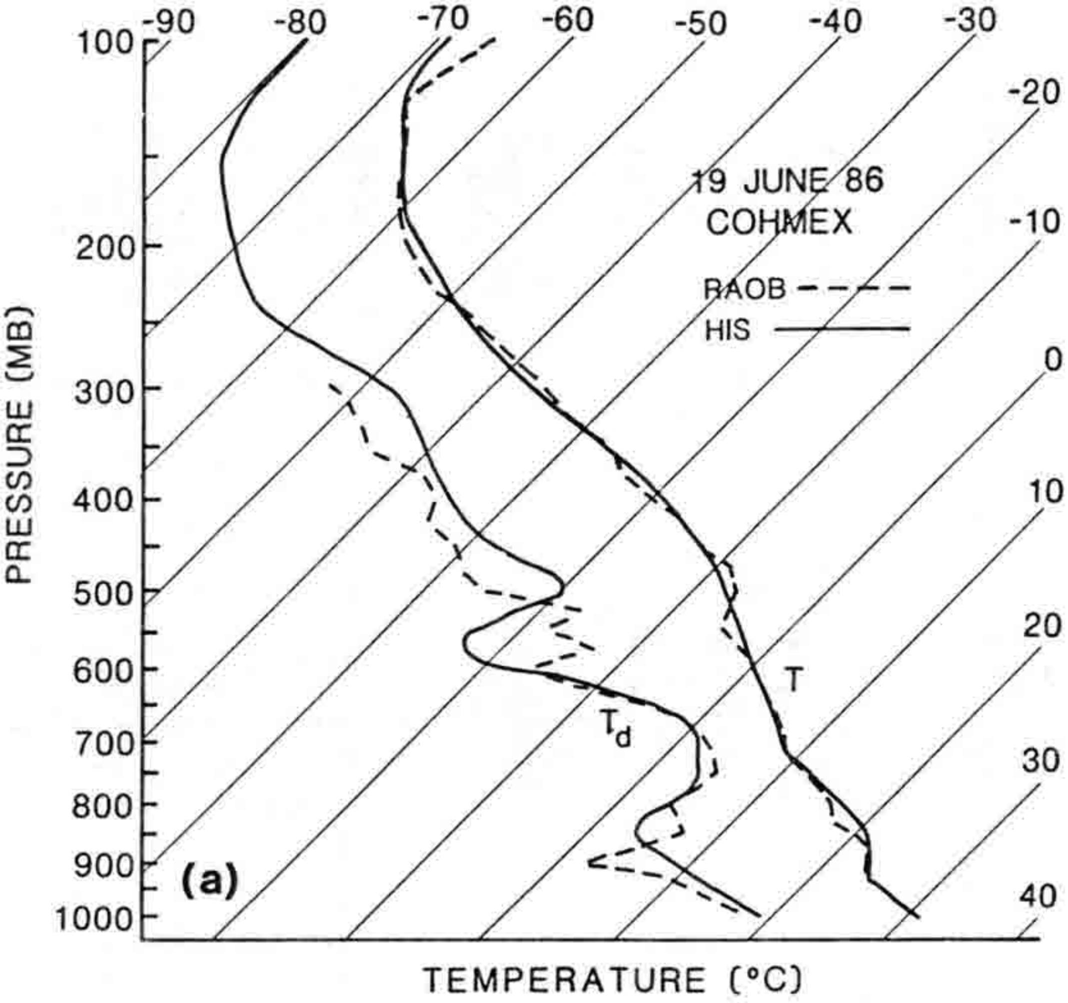


Fig. 11. HIS sounding retrieval compared to a radiosonde observation.

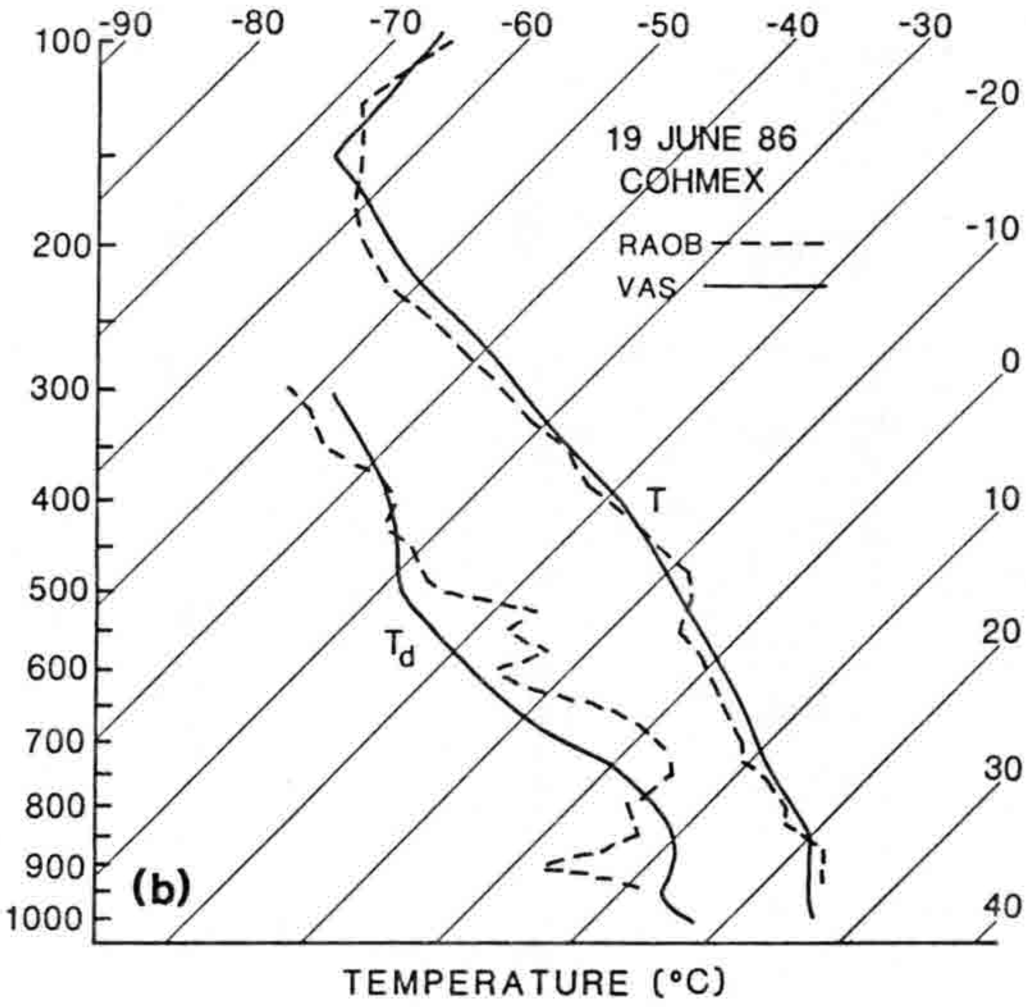
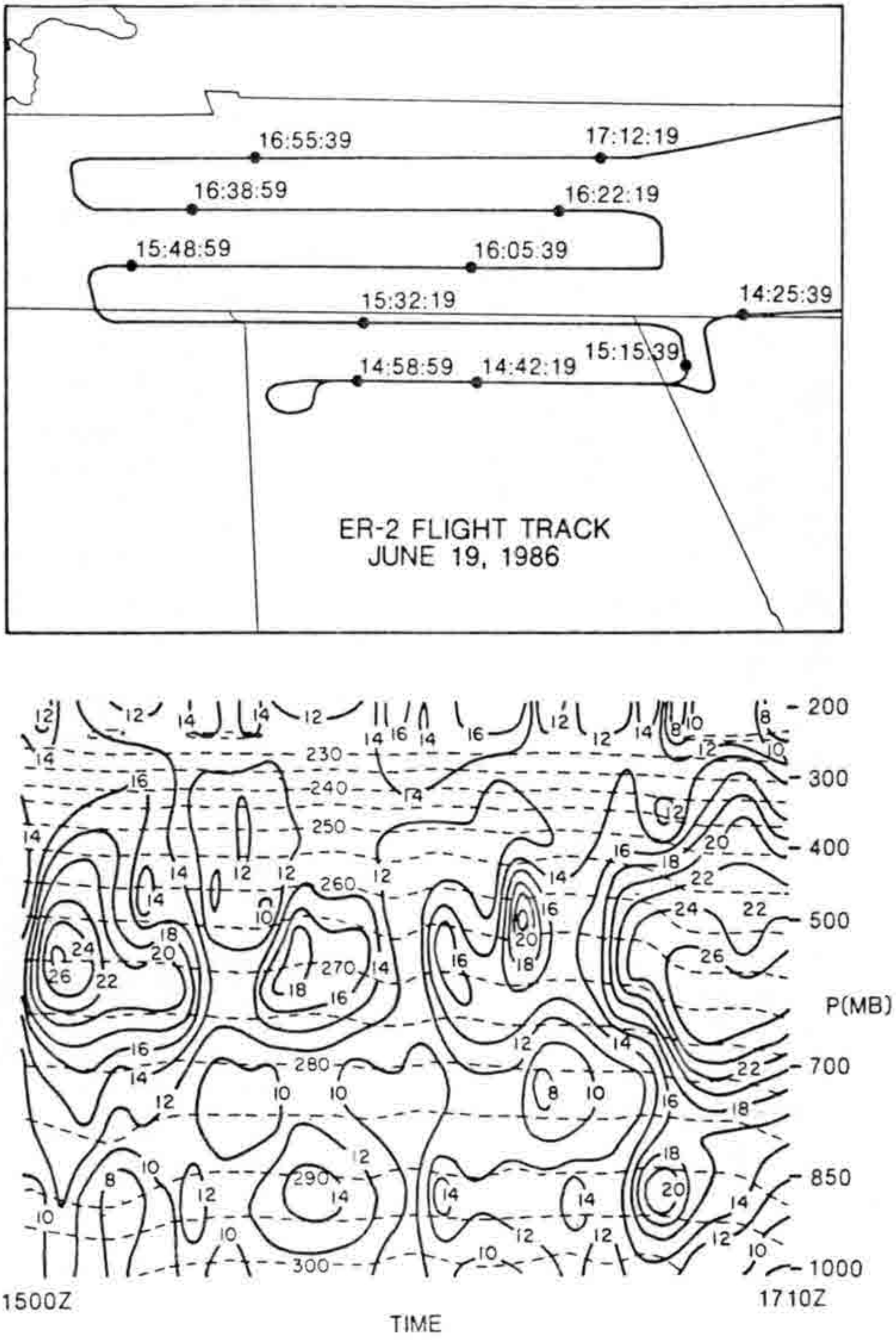


Fig. 12. VAS sounding retrieval compared to a radiosonde observation.



HIS TEMPERATURE AND DEWPOINT DEPRESSION (19 JUNE 1986)

Fig. 13. Vertical time (space) cross-section of temperature and water vapor observed by the HIS from the NASA ER2 on June 19, 1985.

Microwave Instruments on Next-Generation Environmental Satellites: New Opportunities

Talk given by Stanley Q. Kidder
At the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

Instrument Capabilities

Well, I have good news and bad news about microwave instruments on next-generation satellites. The bad news is that barring some sort of miracle there are going to be no microwave instruments on GOES-Next[1]. There were proposals by a couple of gentlemen over here (Tom Vonder Haar and Bill Shenk) to put a microwave instrument on GOES-Next, but the mass margin evaporated; so apparently it is not to be. This means that we are not going to get the improvement in time resolution that we would like in next-generation microwave measurements.

The good news is that the new microwave instrumentation which is currently flying on the DMSP[2] satellite (SSM/I[3]) and which will fly on NOAA-K,L,M (AMSU[4]) and on future DMSP satellites (SSM/T-2) is much improved over older instruments (SSM/T[5] on DMSP and MSU[6] on NOAA satellites). AMSU and SSM/T-2 are both scheduled for launch in 1992. The first SSM/I was launched in 1987, and six more SSM/I instruments have been ordered. Table 1 compares current and near-future microwave instruments.

Let's first look at the number of channels. On the current MSUs, there are four channels in a very narrow range from 50.3 to 57.95 GHz. MSU's purpose is to make soundings through clouds to augment the current primary sounder which is the HIRS/2[7] instrument. That's its sole purpose, and very little else has been done with MSU data.

The SSM/T is similar to the MSU. It has more channels (seven) but only half the ground resolution. The SSM/I is an imaging instrument. It has seven channels ranging from 19.35 to 85 GHz. Its purpose is to retrieve geophysical parameters other than temperature. The SSM/T-2 will retrieve water vapor profiles using five channels. AMSU-A and AMSU-B together will have twenty channels--three times the number of previous instruments--and will retrieve temperature, moisture and geophysical parameters. Furthermore, AMSU-A will be *the primary sounding instrument* on NOAA-K,L,M.

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[1] GOES-Next is the next generation of Geostationary Operational Environmental Satellites.

[2] Defense Meteorological Satellite Program, operated by the U.S. Air Force.

[3] Special Sensor Microwave/Imager.

[4] Advanced Microwave Sounding Unit. It consists of two instruments, AMSU-A and AMSU-B.

[5] Special Sensor Microwave/Temperature.

[6] Microwave Sounding Unit.

[7] High Resolution Infrared Radiation Sounder/2.

Table 1
Microwave Instrument Comparison

Parameter	SSM/T	MSU	SSM/I	AMSU-A	AMSU-B	SSM/T-2
Satellites	Current DMSP	Current NOAA	Current DMSP	NOAA-K,L,M	NOAA-K,L,M	Future DMSP
Channels	7	4	7	15	5	5
Freq. Range (GHz)	50.5-59.4	50.3-57.95	19.35-85.5	23.8-89.0	89.0-183.3	90-183.3
NEDT (K)	0.4-0.6	0.3	0.4-1.7	0.25-1.20	0.8	0.5
Beam Width	14°	7.5°	0.3°-1.2°	3.3°	1.1°	3@183°
Best Ground (km)	204	110	12.5-50	48	16	50-100 Res.
Scan Steps	7	11	64-128	30	90	14-28
Swath Width (km)	2053	2347	1394	2179	2179	2053

Let's quickly go through the precision of the instruments. The Noise Equivalent DT (NEDT) will not improve much on the next generation of instruments. It will decrease slightly to 0.25 K on the best AMSU-A channels; however, it will be 0.8 K on AMSU-B and will range up to 1.2 K on AMSU-A. On SSM/I, NEDT ranges from 0.4 to 1.7 K.

Another important improvement with the next generation is in the beam width and the resultant ground resolution. The beam widths are going to be significantly improved on the next generation satellites. The half-power beam width of 7.5° on MSU will decrease to 3.3° on AMSU-A and 1.1° on AMSU-B. It ranges from 0.3° to 1.2° for SSM/I depending on channel. The best ground resolution of 110 km for MSU is going to decrease to 48 km for AMSU-A, a very good improvement. For AMSU-B, it is going to be about 16 km. SSM/I resolution ranges from 12.5 to 50 km, again depending on the frequency.

The number of scan steps are going to be increased in order to take advantage of the new resolution. There are only 11 scan steps now on MSU. Later I will show you a slide that dramatically demonstrates that 11 scan steps is not very good. There will be 30 scan steps for AMSU-A and 90 for AMSU-B. There are between 64 and 128 scan steps for SSM/I. It is interesting that AMSU-A is the Advanced Microwave Sounding Unit, yet it will have a similar number of scan steps or ground resolution to the SSM/I which is called an imager. So I think sounding and imaging are coming together, which is a good thing. Of course it doesn't rival the infrared instruments, but it's a dramatic improvement.

Swath widths for these instruments are all similar, but slightly less for SSM/I. From an altitude of 833 km, where the NOAA satellites fly, this is not broad enough to give complete global coverage. You need about 2833 km in order to have continuous coverage at the equator. So all these instruments have gaps. It's going to be slightly worse for AMSU-A and -B than it was for MSU, which is bad news for some applications.

Let me give you a quick glimpse at the resolution capabilities. Figure 1 shows the scan pattern for the current MSU instrument in comparison with the HIRS instrument. At nadir HIRS scan spots are about 17 km across and their centers are separated by about 26 km. MSU scan spots are 110 km across and are not contiguous. Their centers are separated by about 138 km--a 20% underlap. Figure 2 shows the MSU scan pattern superimposed on a map of the U.S. This is coarse resolution. Of course, it didn't have to be anything more than coarse because MSU is secondary to HIRS; its primary job is to make tropospheric soundings in overcast situations. There are a lot of good reasons for improving the resolution, however. Figure 3 shows the scan patten for AMSU-A. There are 30 scan spots per line as opposed to 11 for MSU. The scan spots are smaller (about twice the resolution), and they are contiguous. Now we are getting to the point that we will be able to study the details in meteorological systems.

Before I go on to what the frequencies are going to be on the new instruments, let me review the microwave spectrum. Figure 4 gives atmospheric transmittance versus frequency expressed in gigahertz (GHz) which is the frequency unit of choice for microwave folks. There are four bands of primary importance. One is a weak water vapor rotation line at 22.235 GHz. An oxygen (O_2) band appears around 60 GHz; this band is currently used on the Microwave Sounding Unit. There is another oxygen line near 118 GHz, which is what probably will be used for soundings from geosynchronous altitude (if a microwave instrument ever gets up there) because of antenna size considerations. Finally at 183 GHz there is another water vapor rotation line. Windows are between absorption bands: near 150, 90, 37, and less than 22 GHz.

Table 2 shows the frequencies used on current and future microwave instruments. MSU has only four frequencies right around the 60 GHz oxygen band because its purpose is temperature sounding. SSM/I has seven channels in four frequencies. There are both vertical and horizontal polarizations at 19, 37, and 85 GHz. 85 GHz is the new channel and is generating a lot of interest because it senses ice at the upper levels of clouds. SSM/I has only vertical polarization at 22 GHz.

AMSU-A is going to have twelve channels for sounding. Four AMSU-A channels essentially duplicate the MSU channels. AMSU-A channels 10-14 look at two bands on either side of the 57.29 GHz central frequency. AMSU- A is going to have a 23.8 GHz channel near the water vapor line and a 31.4 GHz channel in the window. These last two channels can be used to retrieve column-integrated water vapor and liquid water. AMSU-A will also have an 89 GHz channel. AMSU-A, by the way, is being built by Aerojet. The British Met. Office is supplying AMSU-B.

AMSU-B will have ground resolution comparable to that of SSM/I; it will have three times the ground resolution of the AMSU-A. AMSU-B will have five channels. The 89 GHz channel is essentially the same as the AMSU-A 89 GHz channel, but it will have three times the resolution. One channel at 157 GHz will be in a window, and three channels around the 183 GHz line will measure water vapor profiles. The 157 channel and all the 183 channels are new to space, at least for earth sensing instruments.

Table 2
Channel Frequencies (GHz) and Polarization*

Channel	SSM/T	MSU	AMSU-A	AMSU-B	SSM/I	SSM/T-2
1	50.5H	50.30R	23.8R	89.0R	19.35H	183.3±3V
2	53.2H	53.74R	31.4R	157.0R	19.35V	183.3±1V
3	54.35H	54.96R	50.3R	183.3±1R	22.235V	183.3±7V
4	54.9H	57.95R	52.8R	183.3±3R	37.0H	91.7V
5	58.4V		53.6R	183.3±7R	37.0V	150V
6	58.825V		54.4R		85.5H	
7	59.4V		54.9R		85.5V	
8			55.5R			
9			57.2R			
10			57.29±.217R			
11			57.29±.322±.048R			
12			57.29±.322±.022R			
13			57.29±.322±.010R			
14			57.29±.322±.0045R			
15			89.0R			

*V= vertical, H=horizontal, R=rotates with scan angle.

Meteorological Applications

Sounding

Figure 5 shows the weighting functions for the MSU and the Stratospheric Sounding Unit (SSU). The lower four are the MSU weighting functions. Figure 6 shows the AMSU-A weighting functions. The SSU will not be present on the NOAA-K,L,M. We still need information on the stratosphere, of course, because if we don't have the stratosphere right we're not going to get the troposphere right either. The reason for having those funny channels on either side of 57.29 GHz is for stratospheric sounding. These are very good channels made possible by recent advances in microwave sensing. We can see that the AMSU-A channels cover fairly nicely the range of 1-1000 mb but the channels are still broad. They are slightly broader than are infrared channels.

Water Vapor/Liquid Water

Sounding is one of the primary things that we want to do with the microwave instrument, but there are other things that we are going to be able to do as well, things we cannot do with the current generation of satellites because they only have the temperature soundings channels on them. Two things we will be able to look at are water vapor and liquid water. There are two water vapor lines, one about 22 GHz, which will be used mostly for retrieving column-integrated water vapor, and one about 183 GHz, which will be used for water vapor profiling. For water vapor/liquid water retrievals the basic idea is that the 23.8 GHz channel is more sensitive to water vapor than is the 31.4 GHz channel. On the other hand, absorption by cloud droplets (Figure 7), which is proportional to the liquid water content, has a nearly linear increase in the absorption coefficient with frequency. The 31.4 GHz channel, therefore, is more sensitive to liquid water than is the 23.8 GHz channel. The combination of these two channels will let us retrieve water vapor and liquid water, at least over the ocean, where the low emittance of water (0.4 or so) provides a very cold background.

Andy Jones of Colorado State University has just completed a Masters thesis (Jones, 1988) in which he used SSM/I data and GOES data to retrieve soil emittance and liquid water over land[8]. Liquid water was retrieved in an iterative procedure. Rawinsonde soundings, were used to determine atmospheric temperature and moisture structure. SSM/I and GOES data were used to retrieve surface emittance (see below), which was assumed constant. With a knowledge of surface emittance and atmospheric temperature and moisture, it is possible to calculate the microwave brightness temperature which would be observed if skies were clear. Liquid water was then added to the sounding (between cloud top, estimated from GOES IR data, and cloud base, estimated from the sounding) until the calculated brightness temperature matched the observed brightness temperature. The 85.5H SSM/I channel was used in this calculation.

The 183 GHz channels will let us retrieve water vapor profiles in the microwave region for the first time. Although clouds may present some problems at this frequency, there may be ways to retrieve water vapor profiles even in the presence of clouds, which current IR moisture channels cannot do.

Precipitation

Another thing we would like to measure is precipitation. Figure 8 shows brightness temperature as a function of rain rate for three frequencies and two backgrounds. Clouds are nearly transparent to microwaves but precipitation is not because precipitation-size drops are large enough to interact strongly with the microwave radiation. Over the radiometrically cold ocean, brightness temperature increases with rain rate until a saturation point, after which brightness temperature decreases. Lower frequencies are better for precipitation estimation over the ocean because they have a higher saturation rain rate. Unfortunately, neither AMSU-A or AMSU-B will have a 19 GHz channel, but SSM/I does. Over radiometrically warm land, brightness temperature decreases with rain rate. Higher frequencies are generally better for precipitation estimation because of the steeper decrease with rain rate. However, the higher frequencies tend to be more sensitive to ice at the top of the cloud, which means that precipitation estimation at higher frequencies, which will be present on both AMSU-A and AMSU-B, are more indirect. When it comes to channels, I agree with Bill Smith that more is better. AMSU will have 20 channels, which will allow multifrequency precipitation-estimation algorithms.

Soil Moisture/Antecedent Precipitation Index

Satellite-measured microwave radiation is a function of soil moisture. You should know that microwave radiance is proportional to the temperature of the emitting surface. The Planck function isn't necessary because at microwave wavelengths radiance is directly proportional to temperature. The brightness temperature measured by a satellite in a window channel will be simply the emittance of the surface times its thermometric temperature times an atmospheric transmittance (plus a small atmospheric term). Water surfaces, such as oceans or lakes, have a

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[8] During the talk, slides of SSM/I data were shown. They are not reproduced here; the reader is referred to Jones (1988).

very low emittance in the microwave region, about 0.4 to 0.6 depending on frequency. Land has a very high emittance, about 0.95 or so, just as it does in the infrared. It turns out that if water is added to the soil, emittance decreases. So wet soil has a lower brightness temperature than dry soil. Of course, different soil types have different emittances, which means that a knowledge of soil type is necessary to retrieve soil moisture. Figure 9 shows aircraft-measured brightness temperature versus soil moisture, that is percent moisture by weight, of some soils near Phoenix. Emittance decreases and the brightness temperature of the soil decreases as soil moisture increases.

Andy Jones (1988) divided the SSM/I 19.35 GHz brightness temperature by the 11 µm brightness temperature from GOES to estimate the emittance of the surface[9]. We will be able to do this with NOAA-K,L,M using the 11 µm AVHRR brightness temperature, for example, as the thermometric temperature of the soil and microwave brightness temperatures from AMSU. Of course Jones's technique will only work if skies are clear. However, microwave-only techniques can work under cloudy conditions.

This is very exciting stuff. In addition to being useful to estimate soil saturation for flood forecasting, it may allow us to determine something about precipitation between passes of the satellite, a sort of antecedent precipitation index. The best that we can hope for is four passes of the satellite per day, assuming two polar-orbiting satellites. Even if we could exactly measure the rain rate at the time of satellite passage, the six-hour gap is too long to permit integration of daily rainfall. But the soil moisture approach may be useful for integrating between satellite passes.

Hurricanes

I guess everybody knows by now that hurricanes can be sensed with microwave data. Hurricanes have warm temperature anomalies, which is what cause the low central pressure. It turns out that the peak of the temperature anomaly is at upper levels, around 250 mb in this case for Hurricane Inez (Figure 10). With a microwave instrument, these anomalies can be measured through the clouds. Knowledge of the temperature anomalies yields estimates of surface pressure anomalies and surface wind speeds. AMSU will help us with this because it is going to have much higher resolution, twice the resolution, of the MSU. Velden and Smith (1983) have retrieved soundings inside the hurricane. One must be careful about precipitation in the lower channels, but retrieved soundings give a better picture of what the temperature anomalies look like and therefore better estimates of the intensity of the storm and the central pressure. With reconnaissance aircraft flights into tropical cyclones decreasing, this will be an important application of the new instruments.

Sea Surface Wind Speeds

Microwave data can also be used to look at sea surface wind speeds not associated with hurricanes. With fairly strong wind speeds, higher than about 14 m s^{-1}, you start to get foam on the surface, white caps. The plane water surface has a fairly low emittance, resulting in a low brightness temperature. Foam, on the other hand, has a high emittance. So if you cover part of the surface with foam, then you get a higher emittance, and that means you get a higher brightness temperature. Above a certain threshold, brightness temperature tends to increase with wind speed

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[9] Again, during the talk, slides of SSM/I data were shown. The reader is referred to Jones (1988).

in a fairly linear fashion (Figure 11). So on the ocean surface, places of high wind speed can be located through clouds. This is important to shipping in other interests. With AMSU we should be able to make a multiparameter retrieval to correct for liquid water, water vapor, and precipitation.

Ice and Snow

Let me briefly mention ice and snow. You can see ice on the sea surface because ice has a high microwave emittance, water has a low emittance. We may also be able to get some information about snow cover on land even through clouds. That will be an interesting application if it works.

Synoptic Analysis

Finally, I want to talk about synoptic analysis. Jim Purdom and others at CIRA have made images from limb-corrected MSU radiances in which synoptic patterns are quite evident. Because of the linear relationship between microwave radiance and thermometric temperature, brightness temperature maps are essentially maps of layer-mean temperature. That is, they show weighted vertical averages of temperature using the weighting function as a weight, which means that they represent fairly thick layers. Simple maps of brightness temperatures may be useful in weather analysis and forecasting. A problem with this technique is the coarse resolution of the MSU instrument, which necessitates interpolation and smoothing of the data to yield interpretable patterns. The AMSU-A will have about twice the resolution of MSU, which will make smaller-scale features detectable.

Conclusion

Some of the things I have talked about are in the official list of products that we're supposed to get from Day 1, but many are not. There is a lot of room for research with these new channels.

Discussion

PURDOM: Is there a chance of using soil moisture measurements to look for wet versus dry soil for differential heating mechanisms?

KIDDER: Yes.

VONDER HAAR: Since the models are beginning to carry liquid water as an explicit variable, it looks like microwave measurements are going to be very useful for model studies of all types, and of course they are of interest for icing forecasts. The detail that you can see in Andy Jones' liquid water retrievals is pretty exciting, especially in comparison with current aviation icing forecasts.

McCLATCHEY: I just want to make the same comment that I made relative to infrared data. I think that in the microwave case, too, the question needs to be asked, Is anybody using the data, and if not, why not? How are we going to make sure that when we put the next-generation system up there that we've got a direct tie-in with the guy who's going to use the data? If the data are used and wanted and needed, then our job of getting these kinds of improvements in space in the future will be easy.

KIDDER: I talked to some people at NMC. I asked them the questions, what soundings get in the models, what winds get in the models? My impression is that they would like to use the data, but they are overwhelmed with all of them. They have tried to make tests to see whether or not the soundings improve the model performance, and their results were not great. So they weren't terribly excited about including satellite soundings. Other people, however, have found very good results when satellite soundings were used in models. NMC does put satellite soundings in models depending on when they get the soundings. It turns out the soundings mostly go into the first guess because the soundings are not available soon enough to go into LFM or into RAFS, but they do go into the global run, which is six hours after map time. Since the global run provides the first guess for the next LFM and RAFS runs, satellite soundings do affect the models. The cloud track winds get in the models. Exactly how well they are assimilated I don't know. It's our duty, I think, to point out to modelers just how good satellite data are and what they need to do to properly assimilate them.

PURDOM: And we need to point out the uses also. You just can't give them the data. Storm forecasting is a good example. If we could tell somebody absolute instability of the atmosphere, every single square kilometer, so what? Does that really relate to how strong the storms are going to be, where they are going to develop, what's going to happen to them? We would like to think so, but if we gave somebody that information raw, so what?

DRIEDONKS: I think it is a very important point that we should concentrate on improving the services, not only on the technique. It is the service of the meteorological community through society that in the end is the most important thing and not whether the sounder has seven channels or eight channels.

LYNCH: The improved imagery is a real advantage of AMSU as compared with the MSU. We were disappointed when we were getting rapid intensification of some tropical cyclones and finding that when we collocated the MSU and the AVHRR data, the storm fell in the gaps in the MSU coverage.

KIDDER: That is a very big problem. AMSU has much improved resolution, but the gaps between consecutive passes will still be present.

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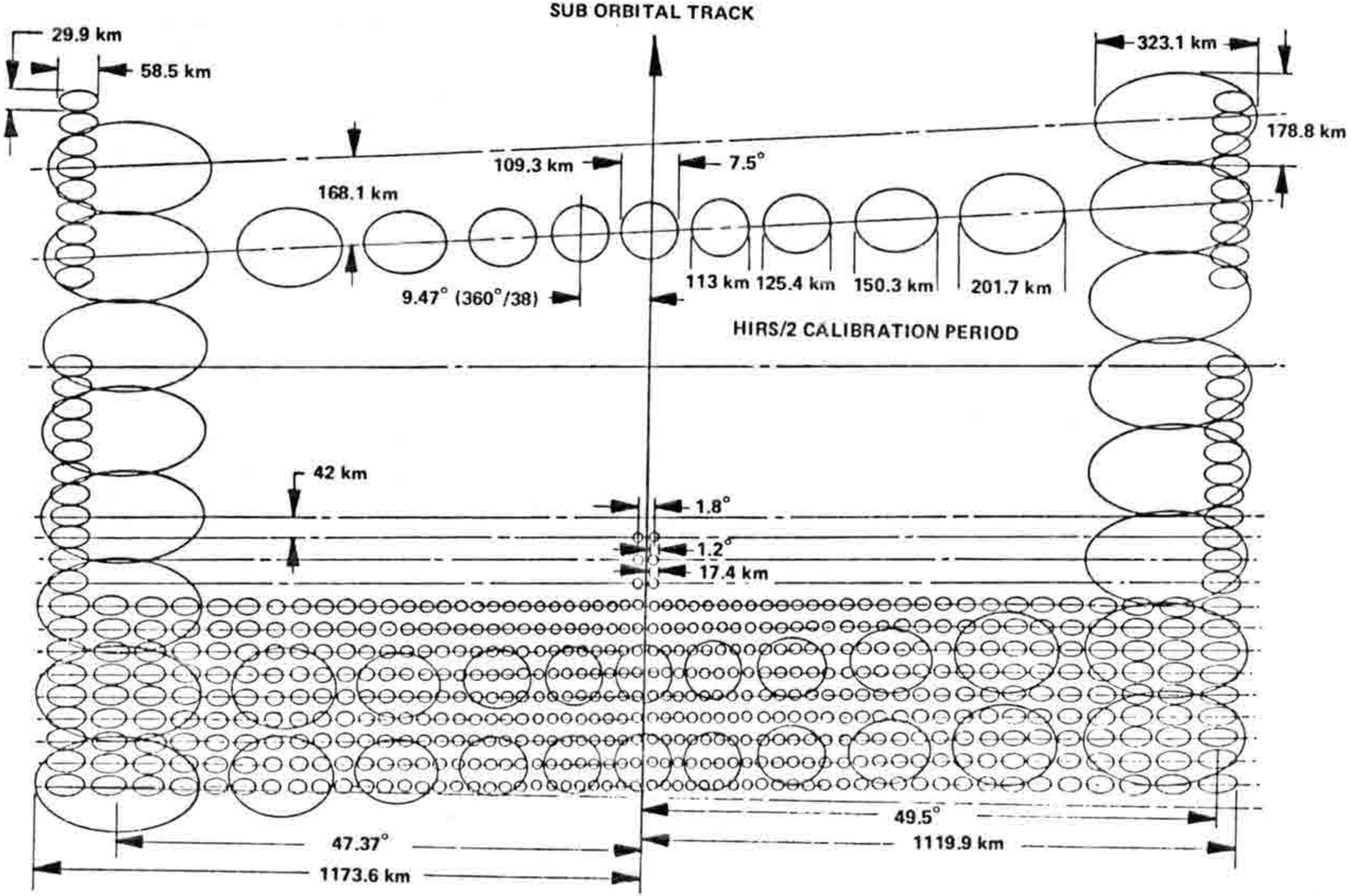

Fig. 1 MSU scan pattern. [After Schwalb (1978).]

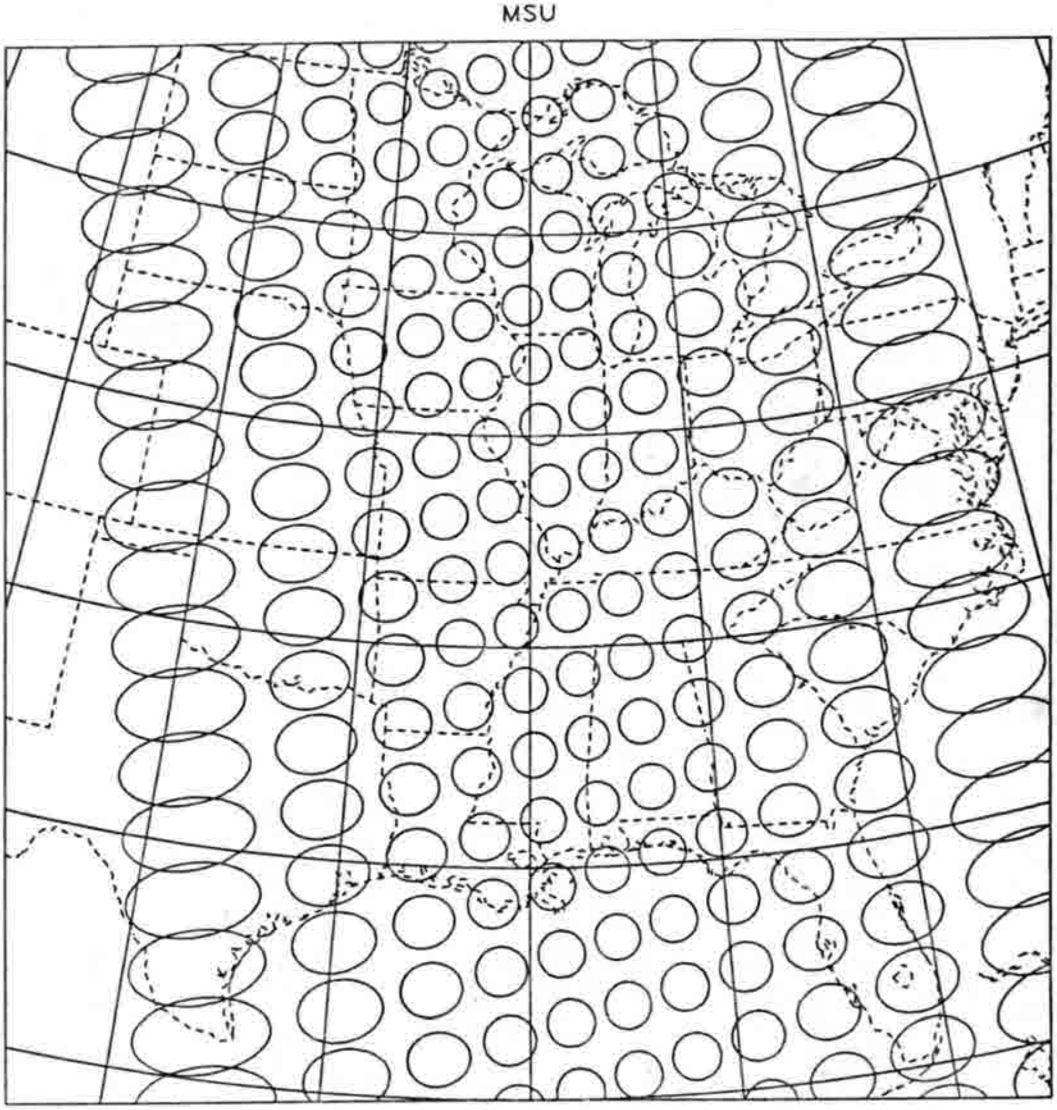

Fig. 2 MSU scan pattern superimposed on a map of the U.S. [After Kidder and Vonder Haar (1989).]

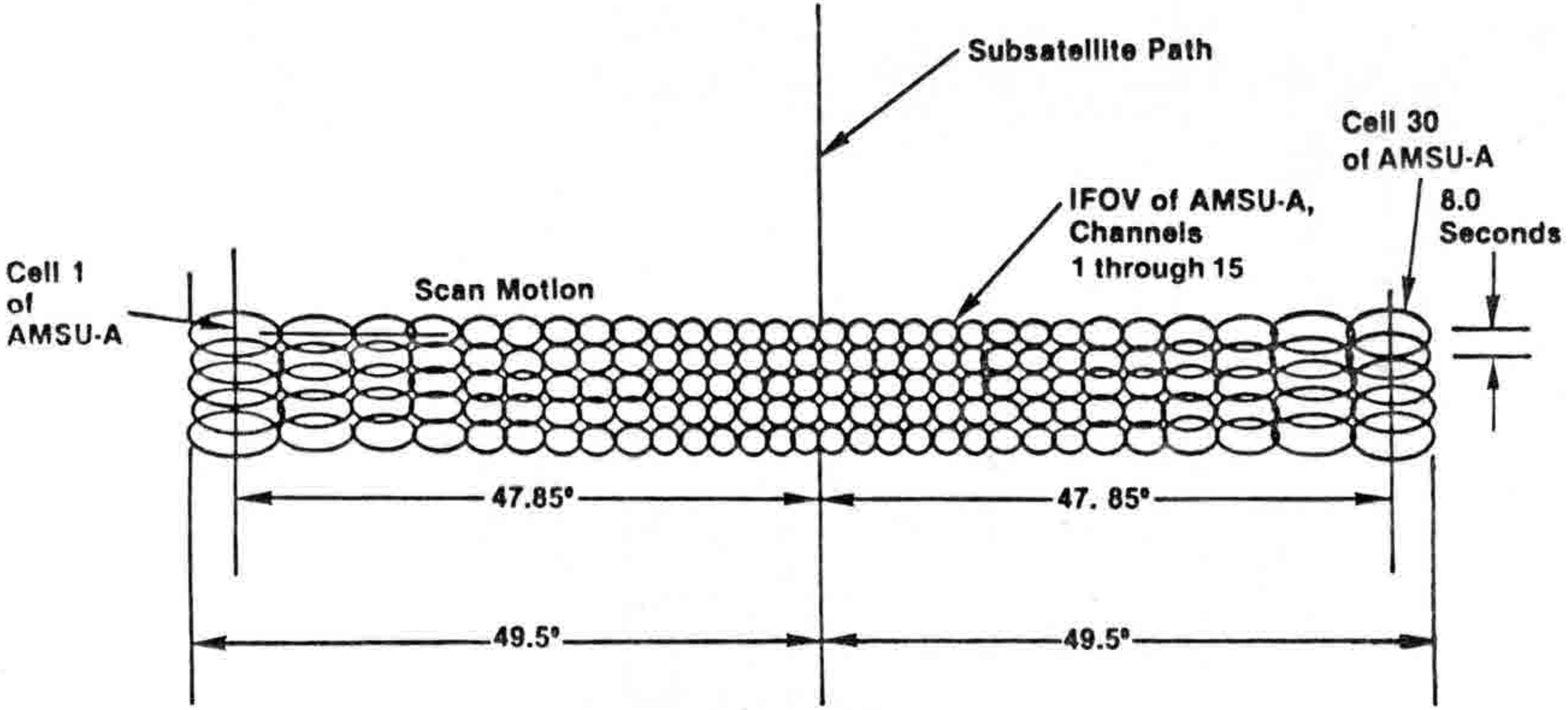

Fig. 3 AMSU-A scan pattern. [After Zenone (1987).]

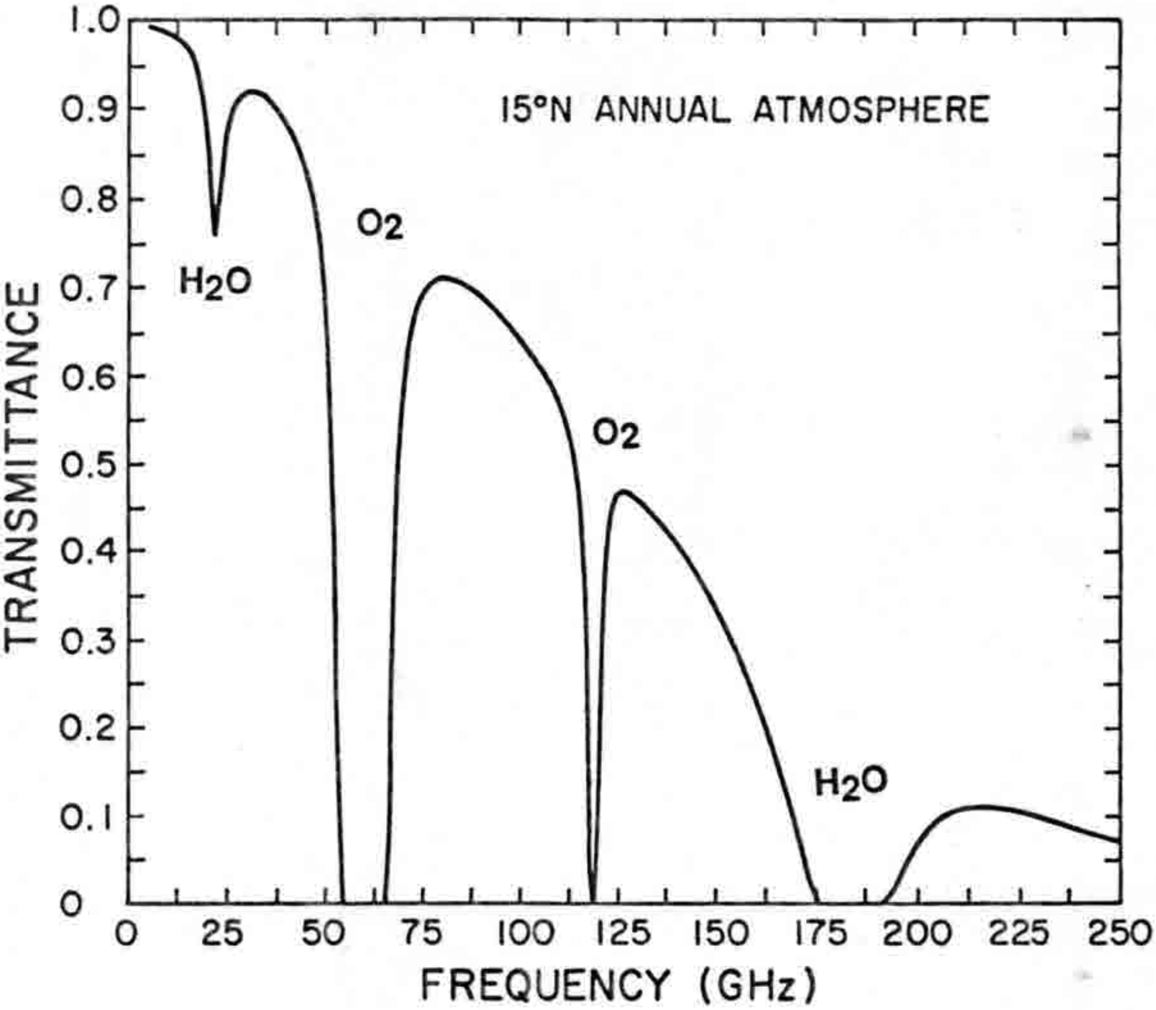

Fig. 4 Microwave transmittance of the tropical atmosphere. [After Kidder (1978).]

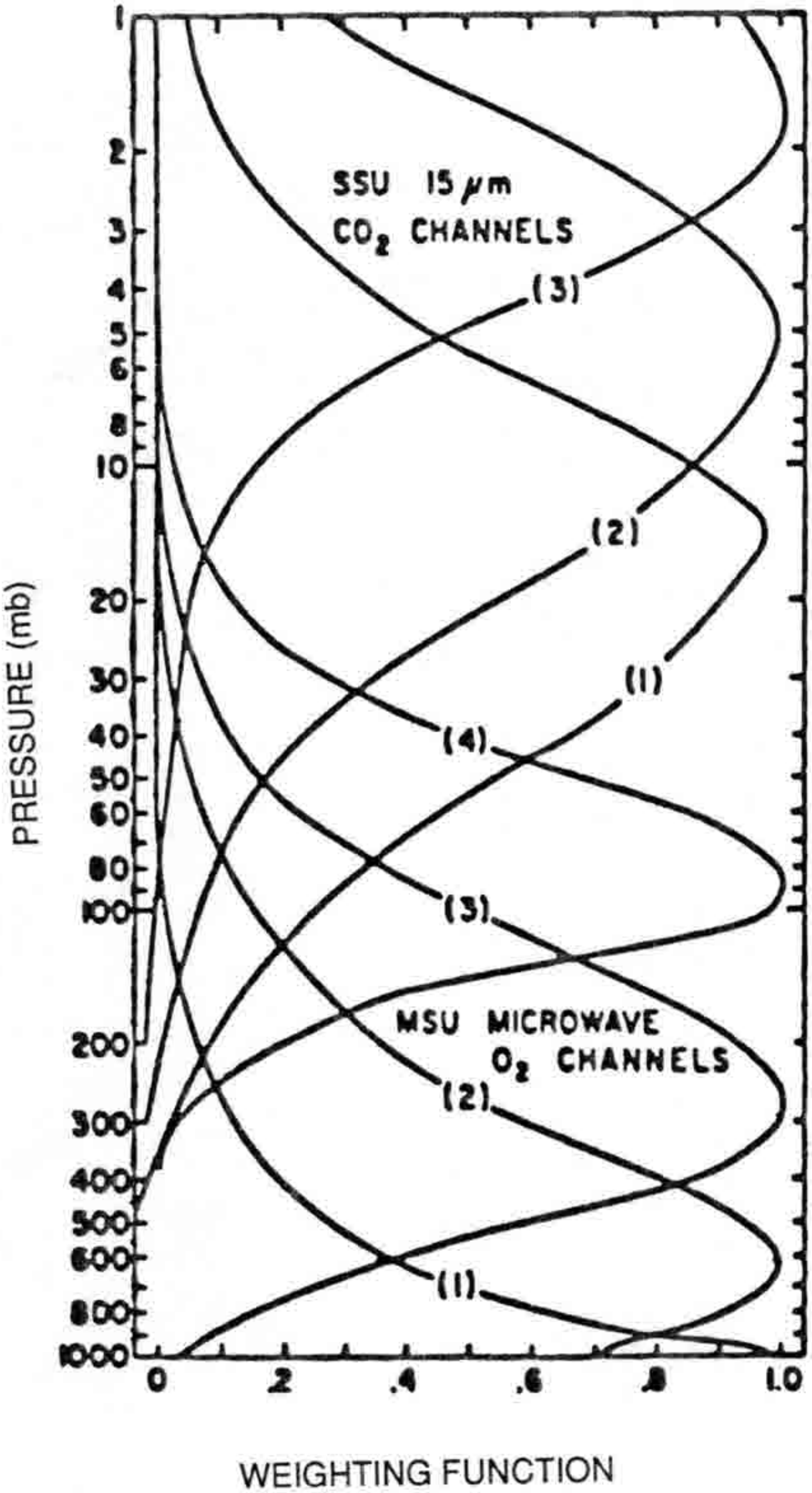

Fig. 5 MSU and SSU weighting functions. [After Smith et al. (1979).]

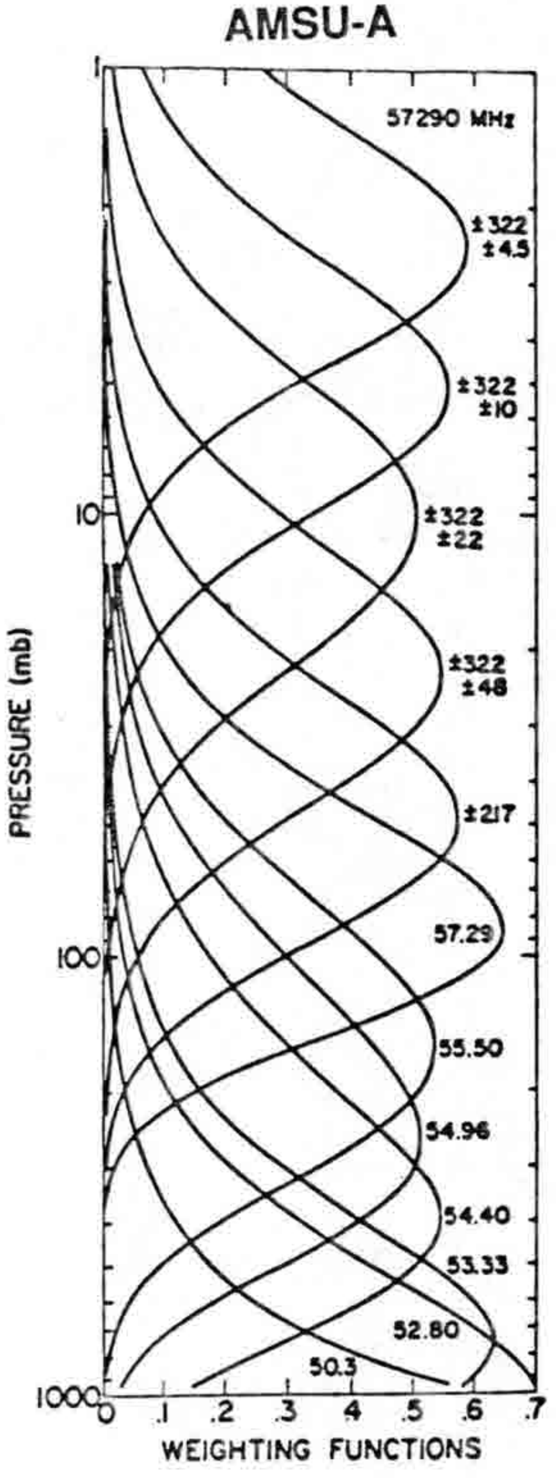

Fig. 6 AMSU-A weighting functions. [After Staelin (1983).]

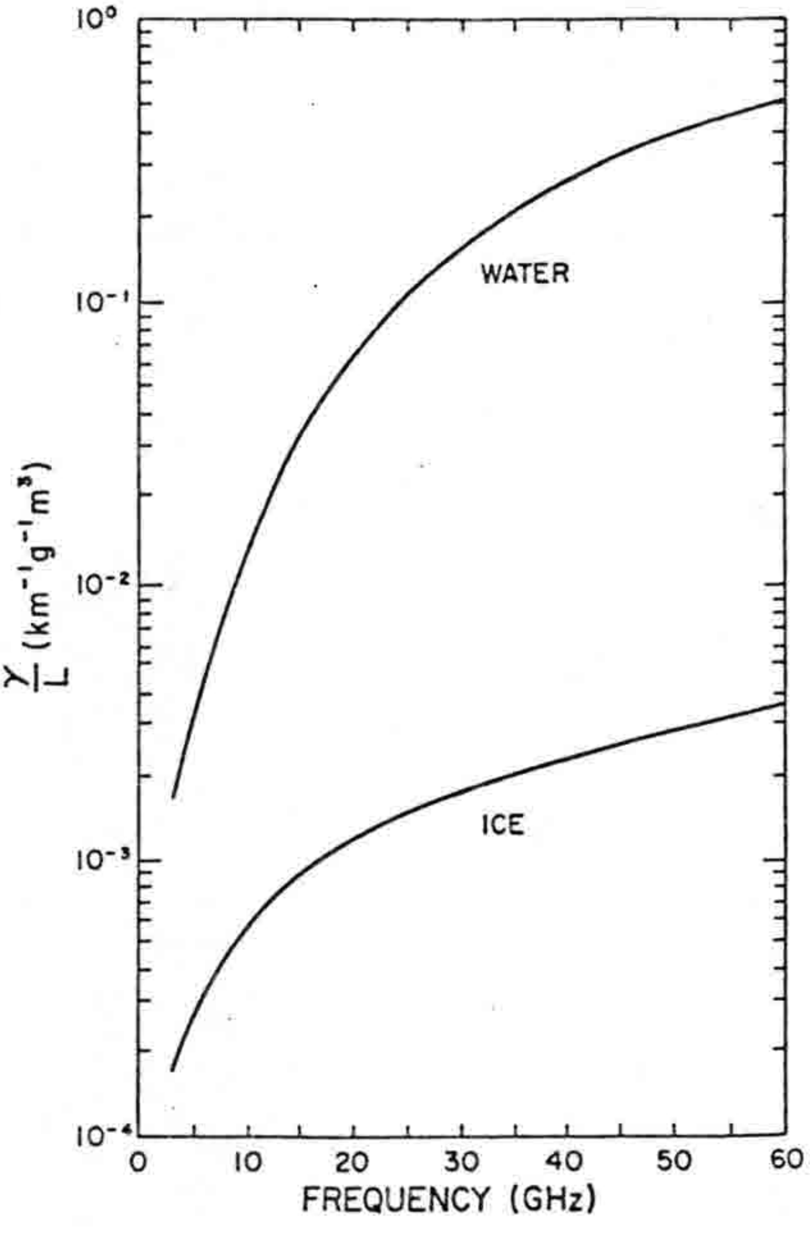

Fig. 7 Absorption by cloud droplets. [After Westwater (1972).]

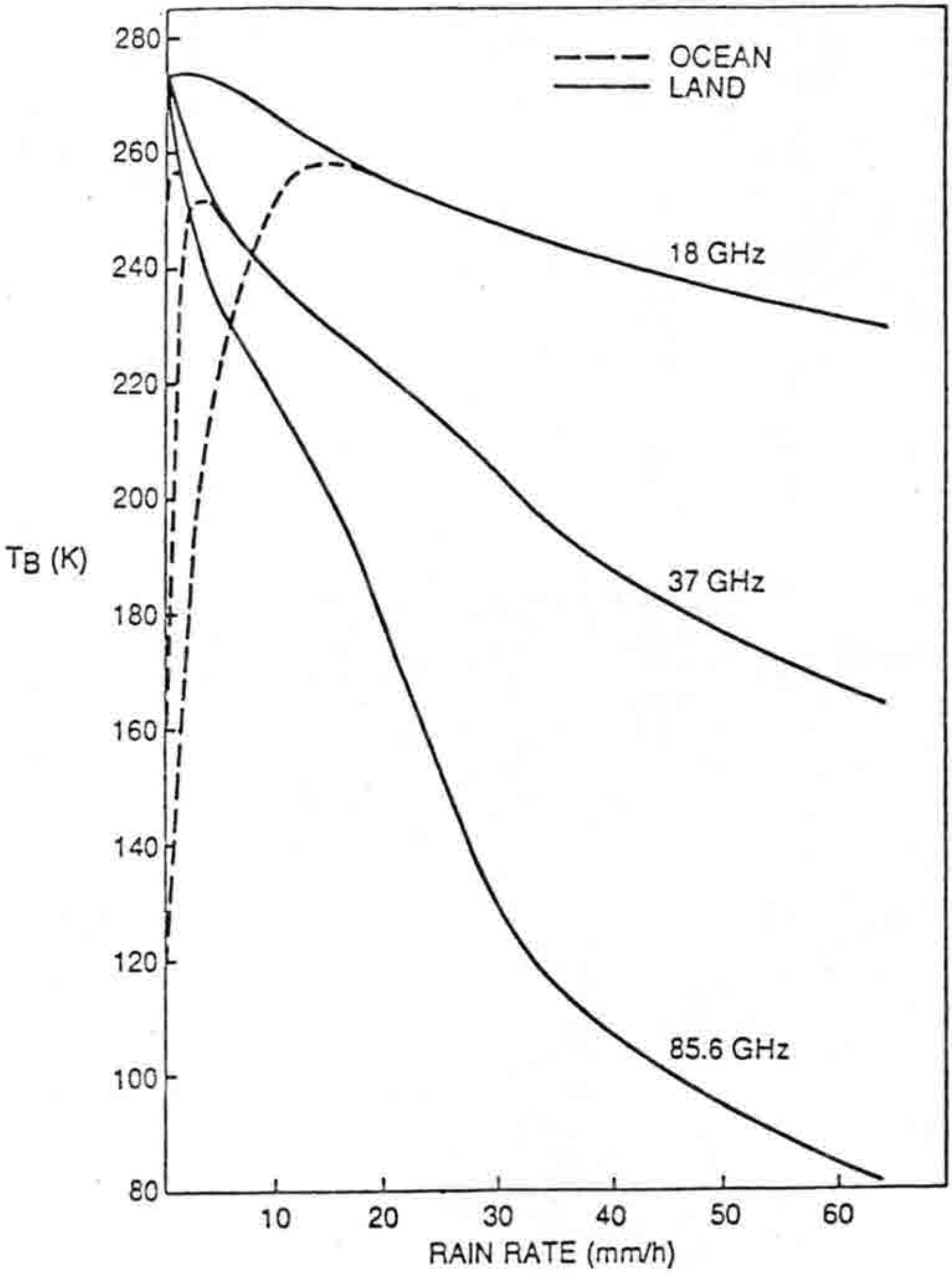

Fig. 8 Brightness temperature-rain rate relationship. [After Spencer et al. (1989).]

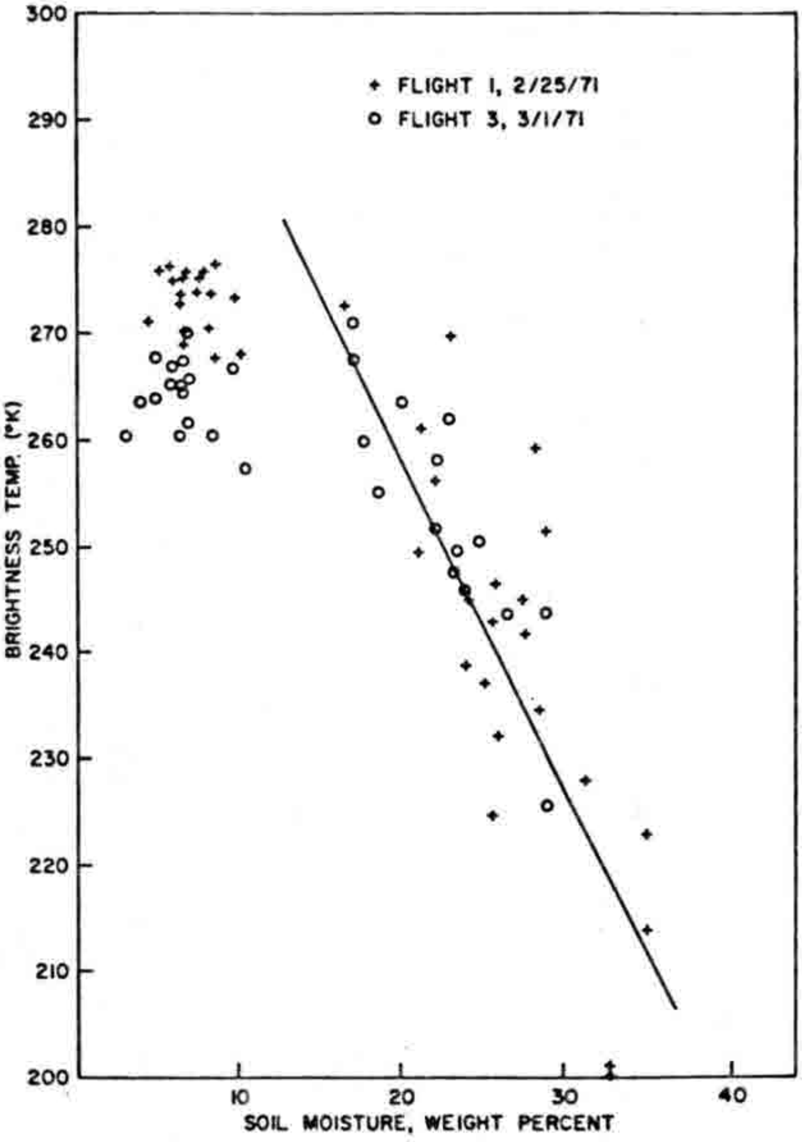

Fig. 9 Aircraft-measured 19.35 GHz brightness temperature-soil moisture relationship for bare fields with clay loam soils near Phoenix, AZ. [From *The Nimbus 5 User's Guide.*]

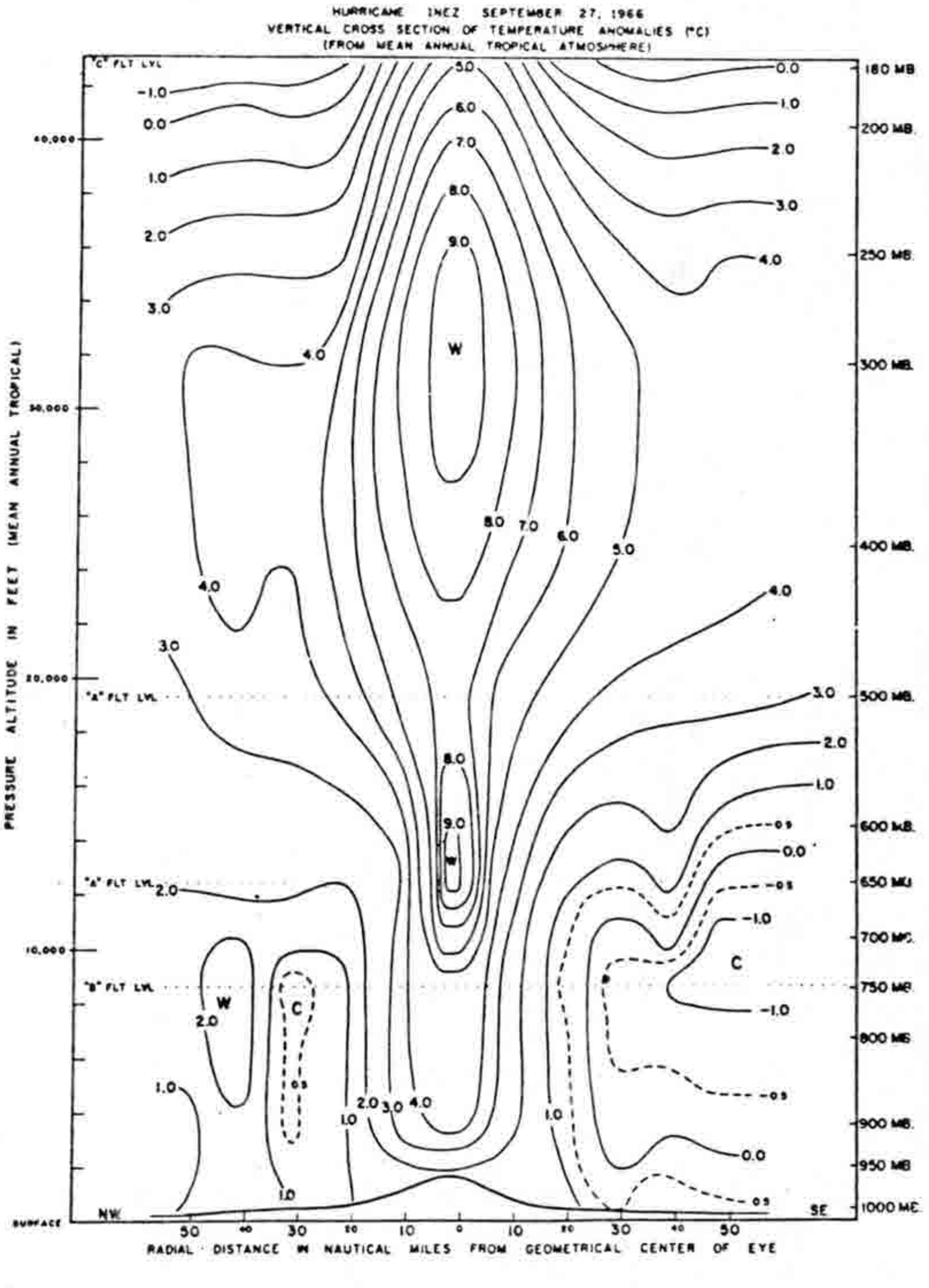

Fig. 10 Temperature anomalies in Hurricane Inez. [After Hawkins and Imbembo (1976).]

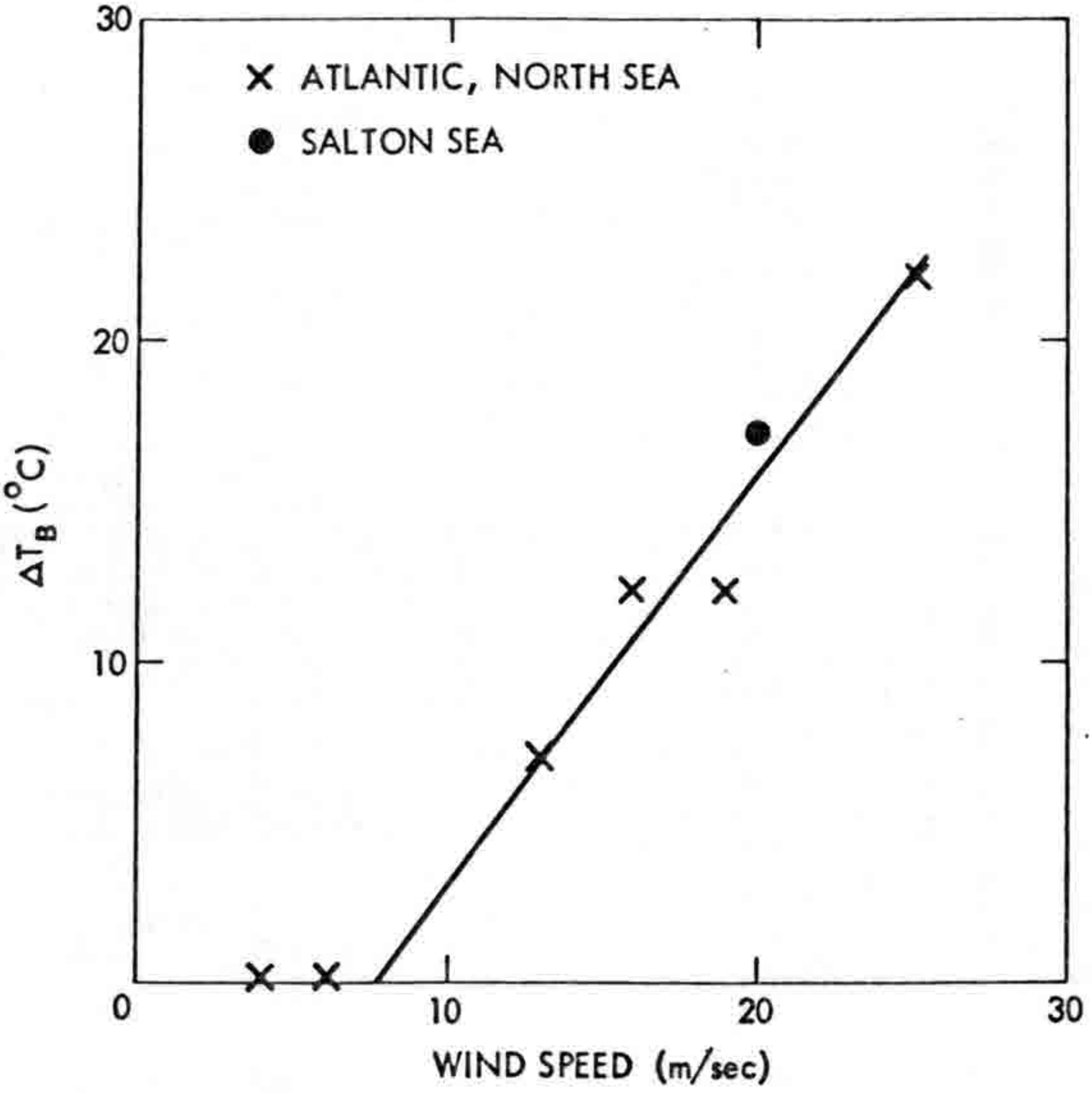

Fig. 11 Brightness temperature change versus wind speed over water surfaces. [From *The Nimbus 5 User's Guide*.]

Mesoscale Modeling

Talk given by Roger A. Pielke
At the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

Ever since I became involved in numerical modeling, I've worked with satellites. The reason is that, for the scales I work on, there is no other routine data source, with the exception of radar, to either initialize, or validate models. What I want to talk about now is where I think we are with respect to the use of numerical models as analysis and/or prediction tools.

One can break down the use of satellite data into at least three categories. The first one I'll mention is the use of satellite data for mesoscale model validation. Satellite is an effective platform, because it gives you either volume average or area average data. This is more closely attuned to what the model gives you, than a rawinsonde which is essentially a line measurement, or a surface observation which is a point measurement.

Secondly, we've found that satellite data is extremely valuable for field experiments. It can help with nowcast guidance, with such questions as where to fly your airplanes, for example. Two such examples occurred during experiments that we conducted during the last year and a half. One of them was designed to look at mesoscale circulations caused by the juxtaposition of snow and bare soil areas. The second looked at mesoscale circulations that are generated by irrigated areas next to nonirrigated areas. In fact, when Stan Kidder presented the previous talk, he mentioned using microwave data to locate wet ground areas. That is a valuable source of information if you want to go out and fly an airplane to see whether there are associated mesoscale systems.

The last category I will mention concerns land use maps. We know there are mesoscale systems caused by snow next to no snow, irrigated areas next to nonirrigated areas, areas that had rain one day adjacent to areas that didn't, urban areas next to nonurban areas, etc. These regions of differential heating appear to have circulations as intense as sea breezes. Since we know that sea breezes exert a major effect on local weather, we would expect the terrain inhomogeneities to have a similar effect on local weather. Satellite provides a natural platform to monitor these surface characteristics. It's an area of weather analysis and forecasting that I don't think has been utilized very effectively.

I'm going to have some rather critical things to say toward the end of my talk regarding the limitations of satellite data for mesoscale model initialization. I will have some rather definitive statements to make and some proof to give you. But, on the positive side, we have found spatial mappings and surface temperature fields from the satellite IR measurements to be extremely useful in terms of forcing mesoscale models. Alan Lipton is here in the audience and just completed a Ph. D thesis where he combined satellite moisture data and mesoscale model simulations to come up with better estimates of moisture profiles. I won't have much to say about that here, but Alan is in the working groups and will certainly present some of his work.

I've got to set out some definitions because not everyone means the same thing when they say mesoscale. I'm going to divide up scales into three ranges (Table 1). The synoptic scale is an ECMWF scale, for example. Those systems are in hydrostatic balance, the winds are essentially in gradient wind balance, and any vertical motions that you produce are essentially a quasi-geostrophic response. They are an adjustment of the fields to the balance between the pressure and the temperature, and a balance between the instantaneous temperature field, and the velocity field. The microscale is for very small scale features. Those are features which are not hydrostatic, in which dynamic pressures are important. Mesoscale lies between these two. The mesoscale is hydrostatic. The advantage of being hydrostatic is that if you measure the temperature field you're directly obtaining the pressure field. However, it's different from the synoptic scale in that an instantaneous snapshot of the temperature field does not give you the velocity field. You need to look at the time evolution of the temperature field in order to construct the velocity field. The models that are being used by NMC are predominantly synoptic. There's a little bit of mesoscale included, but they're predominantly synoptic. This means that if you look at the NGM analysis, for example, winds tend to be parallel to the height contours above the friction layer, and, therefore, it is an easier task to initialize those models with synoptic information than it is on the mesoscale. And again I will have a little bit more to say about that later.

Now I would like to show you some satellite pictures. We originally started modeling in Florida, where we were trying to explain why thunderstorms occurred where they do over the peninsula. We found from numerical model simulations that there were enhanced regions of vertical motion which occur because of the curvature of the coastline, so that when a sea breeze develops, there are preferred areas of thunderstorms. When we did that original work, we validated the model using radar information. However, it would seem that a more natural, or perhaps a less biased, platform would be geostationary satellite data. At the time, though, we only had the ATS geostationary satellite which was not the quality of GOES. What I want to show you here (Fig. 1a), is an example of how we use satellite data in model validation. This is a summation of deep convection as measured by the brightness in the visible and cloud top temperature in the IR for 23 days during the summer of 1984 at about noon local time. We find that there are very clear and very distinct signals that occur. When we compare this to a model simulation, we can come up with a skill level as to how well the model explains this climatologically averaged pattern.

The model does a credible job. For example, it appears that the enhanced activity east of the Lake (Fig. 1a), is due to the convergence of the lake breeze and the sea breeze. In the southwest portion of the peninsula, the enhanced convection results because of a wet Everglades and a convergence zone associated with this particular bulge in the peninsula. Over the next two hours we find that the convection became more widespread. This is two o'clock in the afternoon (Fig. 1b). We see some hint of a line of convection inland from the east coast, and a particularly enhanced region over the southwest part of the peninsula. The next figure (Fig. 2a) shows the area consolidating more in the southwestern part of the peninsula. The last figure (Fig. 2b) shows the convection dissipating with some activity moving off the west coast. Again, this climatological use of the satellite information has been shown to be a very valuable tool in explaining or validating the model, since the model is giving us the most likely pattern.

Now I want to show you that the satellite is consistent with the radar. Figure 3 illustrates the summation of radar return for a period during the summer. Included are hours in which there was radar return with a VIP of three, or greater. We see that, as you would expect, satellite and radar are consistent. This radar composite has a much larger number of cases, but the satellite data apparently captures the essence of what occurs over Florida, and the model replicates it, which gives us some confidence in the model.

Audience: It's interesting that the west of Cape Kennedy activity seems more frequent than along the Banana River, and along the eastern part.

Answer: Yes, and we've been puzzled by this maximum. It appears to be related to the fact that there is a very large difference in land use in this part of the state. There is the sea breeze, or gulf breeze, that develops here. There is also an area that is agricultural next to a swamp area. We think, that this maximum is related to land use.

Audience: We found that in our satellite climatologies, (we just finished doing some work over Florida), and looked at that pronounced maxima in that area. I was trying to figure out why it was there.

Answer: We haven't run the model to test that, but we think ....

Audience: I'll show that slide tomorrow.

Okay. Well, as I mentioned, satellite data are very useful for field experiments in terms of using a model. I want to show you a generic model run in which we impose a snow surface. We use a wintertime sounding and we run the model through the course of a day. This happens to be a simulation (Fig. 4), for about two o'clock in the afternoon. Potential temperature lines are labeled. We see an east-west wind velocity which looks just like a sea breeze. But, in this case, it is generated by the difference in heating between snow and bare soil. Well, this is what the model says, but who knows if this has anything to do with reality. So Moti Segal, Jim Cramer and I conducted a field program in which an integral part was identifying snow boundaries. Figure 5 is from the SERS system in the CIRA building. La Junta and Pueblo in southeastern Colorado are indicated. This is an area where it had snowed. We targeted the airplane to fly a cross section across the snow/no snow boundary. Look at the type of temperature gradients we get when we use the McIDAS system to process GOES IR data for our estimates (Fig. 6). We find that the temperature gradients across this snow boundary are 10°C, or so, which is certainly close to what we find associated with a sea breeze.

Figure 7 is another example of this type of analysis for a different day. The stippled area is snow covered. The white area is bare soil. We have a large gradient across the boundary, in which the airplane was flown on that particular day. The value of the satellite in this case, was in targeting the airplane. We couldn't have done the field program without this very valuable satellite data. Figure 8 shows an example of what we found when we flew the airplane. This happens to be a plot of radiometer-sensed surface temperature flying at 95 meters. The dark area is the snow covered area. This is the Arkansas River which was frozen. We see the higher albedo of the snow. The snow was somewhat patchier over on the right. You can see that the surface temperature as measured by the aircraft radiometer shows a very large gradient in temperature.

Snow/bare soil comparisons are not the only such information we can get from satellite. Here is a plot of vegetated areas and nonvegetated areas over eastern Colorado (Fig. 9), where Fort Collins, Greeley, and Fort Morgan are indicated. Again, the information on surface temperature is from the GOES satellite. The irrigated area near Greeley lies right next to a short grass prairie to the east. We see skin temperatures of 38°C in the prairie region as opposed to 28°C where its irrigated. Again, there is a 10°C difference in temperature. Modeling results have suggested this could generate a circulation as intense as the sea breeze. In addition, it appears that the available buoyant energy is much greater over an irrigated area than it is over the dry area. Aircraft measurements have shown differences of dew point temperature of 15°C between Greeley and out over the plains. We are hypothesizing that perhaps the increase of apparent frequency of severe storms in the Denver metropolitan area and northeast along the front range may be related to the

fact that both the irrigated areas, and the suburban areas, are increasing in size, and therefore the potential for severe storms is changing because of this change in land use.
Audience: Would you say that the moisture contribution more than compensates for it being colder?

Answer: Yes. That's correct.

This is the same kind of analysis for south central Colorado (Fig. 10). Alamosa and the Sangre de Christo Mountains are indicated. The irrigated area is stippled. Here we find 28°C regions adjacent to areas of 40°C in terms of their skin temperature. So the satellite has been extremely useful in terms of identifying regions with different surface characteristics.

All right now, what about some pessimistic results? I'll show a very simple analysis, but one I think that has some rather significant ramifications. Consider the east-west equation of motion (Fig. 11). What I want to do is integrate this equation across a distance equal to one model grid distance. Suppose I obtain data from a satellite at both sides of a grid box, and feed that directly into a model. What kind of errors could I get, if in fact there were errors in these measurements? Well, when you integrate it in this flux form, you can write it in terms of the kinetic energy coming into the west side, minus the kinetic energy going out the east, and the pressure at the one side minus the pressure at the other. Suppose we interpret these differences as errors. Let's say that in reality, there is no gradient; there is no difference. Then we can see what kind of accelerations would result over this distance due to such an error. Table 2 shows how the error changes as our grid increment changes from 1000 km down to 10 km. Now, suppose we can tolerate no more than a 1 meter per second per hour error. Well, we find that we can tolerate almost a 3 mb error across a 1000 km distance. However, if I bring my resolution down to 10 km, we can only tolerate a 0.3 mb error which, if you use a thickness relationship over a depth of 6 km, means an error of 0.06°C! So what that says is that unless you can give me satellite temperature measurements with 0.06°C accuracy or better over that depth, I cannot initialize my model from your satellite data at 10 km resolution.

Audience: Can you initialize with anything with that sort of resolution requirement? That's the question.

Answer: I will come back to that point.

Audience: I think it's a very valid question. I think it leads into the question of how do we initialize mesoscale models.

Answer: Okay, I'll come back and address that problem later. Right now, let me just say that I think what one has to do is, 1) either use surface forcing at that resolution, because we can get it accurately at a very high resolution, or 2) we have to input features that might have a scale of several hundred kilometers and through the nonlinear characteristics of the model, i.e., the nonlinear advection, create smaller scale features. But if we have a propagating system that's not surface forced, we cannot initialize the models with mesoscale data.

The accuracy requirements for winds also becomes much more stringent as you move downscale. If you had a 20 meter per second wind on the left side of your grid, you could only tolerate a 0.1 meter per second error on the right. The reason I think that satellite data has been useful on the synoptic scale is that you're at a larger grid spacing. Also, you don't put the data in at 100 km resolution. You put it in at a larger scale, because the data has been smoothed. So you would, perhaps, be putting in features that represent a 500 km length scale. That's why you can get away with it in synoptic model, whereas in a mesoscale model you can't.

Audience: You're saying that your model screws up if you don't have that accuracy. Are you also saying that the atmosphere will screw up if you have that sort of a change?

Answer: Of course the atmosphere doesn't screw up. The model equations are consistent. It's just that you're inserting the data inconsistently ..... , well, I'm getting ahead of myself a little bit.

Audience: What I'm getting at is that if I go outside, and chug a cup of coffee, I'll increase the temperature by 0.008° C over one square meter. Will that ....

Answer: No, because there it will adjust locally. In a model, you can't explicitly resolve subscale features so the adjustment will occur on your resolvable scale.

Let me give an analogy. Suppose you have a ramp with a fixed Δh. If the ramp extends over a very short distance, and I put a ball on it, it's going to roll down very fast. However, if I have a very long ramp, but with the same Δh, the ball is going to roll a lot slower. That's the way to visualize the error that results in a model. In the true atmosphere, there are no "errors." If you really had this kind of a gradient on this scale, you would get a certain response and that would be consistent and so the model is not wrong. What it is, is that you're feeding in subscale data that's not representative of the larger scale atmosphere.

Audience: But no atmospheric data, no matter how it's measured, is probably not going to be that accurate, so you're really saying you can't use atmospheric data in your model. Isn't that what you're saying?

Answer: Yes, you can. Things get better after this. (Laugh, laugh)

Audience: A related question. What would happen in mesoscale situations, say a severe storm situation, if you had very strong gradients over short distances. Does that influence your analysis in terms of the error you can tolerate?

Answer: If you have a gradient of 10°C per 10 km, and the wind is in balance, let's say, and you impose a 0.08°C error over a depth 6 km, you will still get a 1 meter per second per hour error. You have to decide whether that is significant or not.

Audience: That's what I'm saying. Perhaps a 1 meter per second per hour error, if you're dealing with a rapidly evolving thunderstorm complex that's changing 10 meters or 20 meters per second per hour over local areas, would not be an intolerable situation.

Answer: Okay, let's say we could tolerate 10 meters per second per hour. The temperature tolerance goes up to 0.8°C. Maybe then you're getting close to the point where you can handle it. Let's say you can tolerate an error up to 20 meters per second per hour. Then you can get up to 1.6°C error, so it's a function of the magnitude of the system. This is a simple analysis, but it's complete in making the point about the resolution requirements. You can make your output tolerance requirements less stringent, but once you pick that number, the input tolerances are still going to decrease as you go to a smaller horizontal scale.

Audience: Roger, there is another example from synoptic scale, I'm sure you are aware of. If we took the winds as we observe them right now and tried to compute a divergence, then from that get a vertical motion for the models, we totally foul it up. What synoptic experience has taught us we have to do, is to use those winds but filter them. I think in mesoscale models you're going to have to do the same.

Answer: Exactly. Let me show you how we propose to do it.

Here is a simulation (Fig. 12) of a sea breeze in which the system is surface forced. We used very high resolution, in fact, in this case it was 6 km grid increments. We have a land mass next to a water mass and we integrate the model out in time. We'll show you the results here at two o'clock in the afternoon. Shown is the potential temperature field. We have onshore flow, with the vertical motion and the specific humidity as indicated. This is our control experiment, if you would, which is what we want to be able to monitor by satellite, by profiler, or whatever. Notice that the spatial scale of the temperature field is fairly large, the horizontal velocity field is somewhat smaller, and the vertical velocity field is very small.

What happens if we had a sensor that could sample only at 90 km intervals? Well, we see that at 90 km intervals (Fig. 13), we don't see the vertical motion at all. We still see some representation of the potential temperature field, although the gradient is somewhat smoothed. What happens if we just stagger the sampling by 45 km, in other words, just shift the analysis by 45 km (Fig. 14)? We get a drastically different vertical motion field, in that we see some of it now. We see a different horizontal velocity field, and we see a potential temperature field that still has some of the essence of the control experiment, in that it's cool over the water, warm over the land.

Well, one could look at these results in time, and look, for example, at the maximum and minimum U values we get. Figure 15 shows the U max, U min, W max, W min up to a depth of 4 km. Our control experiment is given by the line with the circles. We look at it with different resolutions down to the 90 km system which is the curve that has the little X marks. Without getting into the details of each individual curve, the conclusion is you can't replicate the controlled experiment in terms of the maximum U, minimum U, the maximum W, and the minimum W. That's pretty discouraging.

However, when we look at our moisture and potential temperature field (Fig. 16), we find that, irrespective of the resolution, we can do a pretty good job of resolving the maximum and minimum in these fields. So, perhaps, we should try to initialize our model even if it is at 90 km resolution, interpolate it down to the 6 km resolution, and then run our model over time and see if we can reconstruct the U and the W fields. We've done that and, Figure 17 shows the difference field. We find that, except very near the very tight part of the convergence zone, we can do a pretty good job of reconstructing the velocity field when we have a coarse temperature field, along with a model which has nonlinear interactions that will permit the generation of a tighter temperature gradient on a smaller grid, and we have strong surface forcing.

I think the conclusion of this analysis is that if you try to initialize a propagating mesoscale system in a numerical model, you can't do it with current resolution capabilities, unless you have a very dynamic system in which you can tolerate fairly large errors. If, however, the system is forced by surface heating such as a land/water contrast, snow/no snow, vegetative/nonvegetative, etc., we know the surface forcing with very high accuracy and very high resolution. In this case, all we need are background temperature, and background moisture fields at a relatively coarse resolution. We insert that into a model, run the model for a couple of hours as an analysis tool to let it construct a finer scale wind and vertical velocity field which then is a pretty good estimate of what actually is occurring. The reason you have to integrate in time, of course, is that mesoscale systems vary in time and, as stated at the beginning of my talk, the mesoscale wind field develops in time in response to the temperature field.

So I want to close with that. I think there is some room for both optimism and pessimism. I think that systems such as propagating squall lines will never be resolved with the accuracy you'd like or at least not using any platform that I'm familiar with. But for trying to resolve land-sea breezes, mountain-valley circulations, I think there is more room for optimism.

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TABLE 1. Summary justification for the three scales of atmospheric motion (from Pielke *et al.* 1989).

SYNOPTIC	MESOSCALE	MICROSCALE
hydrostatic	hydrostatic	nonhydrostatic
$\vec{V}_{H_\circ} \cong \vec{k} \times \nabla\psi_\circ$	$\vec{V}'_H = \vec{k} \times \nabla\psi' + \nabla\chi'$	$\vec{V}''_H = \vec{k} \times \nabla\psi'' + \nabla\chi''$
$w_\circ$ from quasi-geostrophic theory	w' from the anelastic continuity equation	w'' from the anelastic continuity equation
instantaneous temperature field provides the velocity field $[T_\circ(x,y,z,\bar{t})$ $\Rightarrow \vec{V}_{H_\circ}(x,y,z,\bar{t})]$	instantaneous and time evolution of temperature field provides the velocity field $[\int_{t_\circ}^{\bar{t}} T'(x,y,z,t)dt$ $\Rightarrow \vec{V}'_H(x,y,z,\bar{t})]$	temperature field by itself is insufficient to characterize wind field
Rossby number much less than unity	Rossby number not much less than unity	Rossby number not much less than unity

TABLE 2. The values needed to generate a grid-averaged acceleration at level z of $1\ \mathrm{m\ s^{-1}h^{-1}}$.

Grid Distance (km)	$(u_E'^2 - u_W'^2)/2$, ($\mathrm{m^2\ s^{-2}}$)	$\alpha_\circ(p_E' - p_W')$, ($\mathrm{m^2\ s^{-2}}$)	$\Delta u'$ ($\mathrm{m\ s^{-1}}$) for $\hat{u}_W$ ($\mathrm{m\ s^{-1}}$) of 0	5	10	$p_E' - p_W'$ (mb)
1000	278.0	278.0	23.6	19.1	15.6	2.78
100	28.0	28.0	7.6	4.0	2.5	0.28
10	2.8	2.8	2.4	0.5	0.3	0.03

A value of $\alpha_\circ = 1\ \mathrm{m^3\ kg^{-1}}$ was used in computing the pressure gradient force. Adapted from Pielke (1984, page 364).

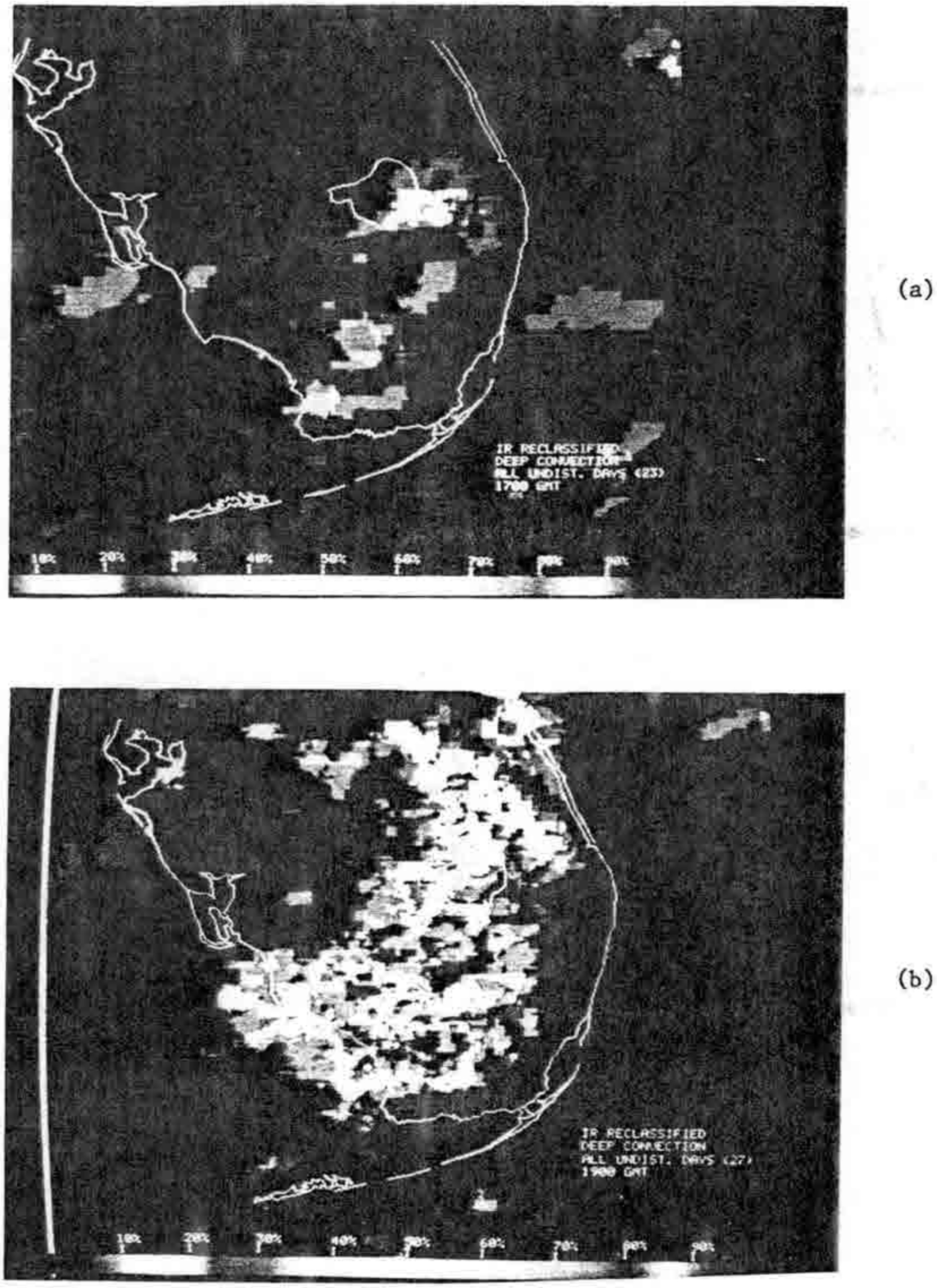

Fig. 1 All undisturbed days composite for deep convective clouds for (a) 1200 EST and (b) 1400 EST. Bar on the bottom of image relates shading to cloud frequency (on originals, a color bar is used). The number in parentheses on each image label indicates the number of images which went into creating the composite. (Color slides of all the composites are available, which more clearly illustrate the cloud composite frequencies; costs prevented reproducing these figures in color for this report). (From McQueen and Pielke, 1985.)

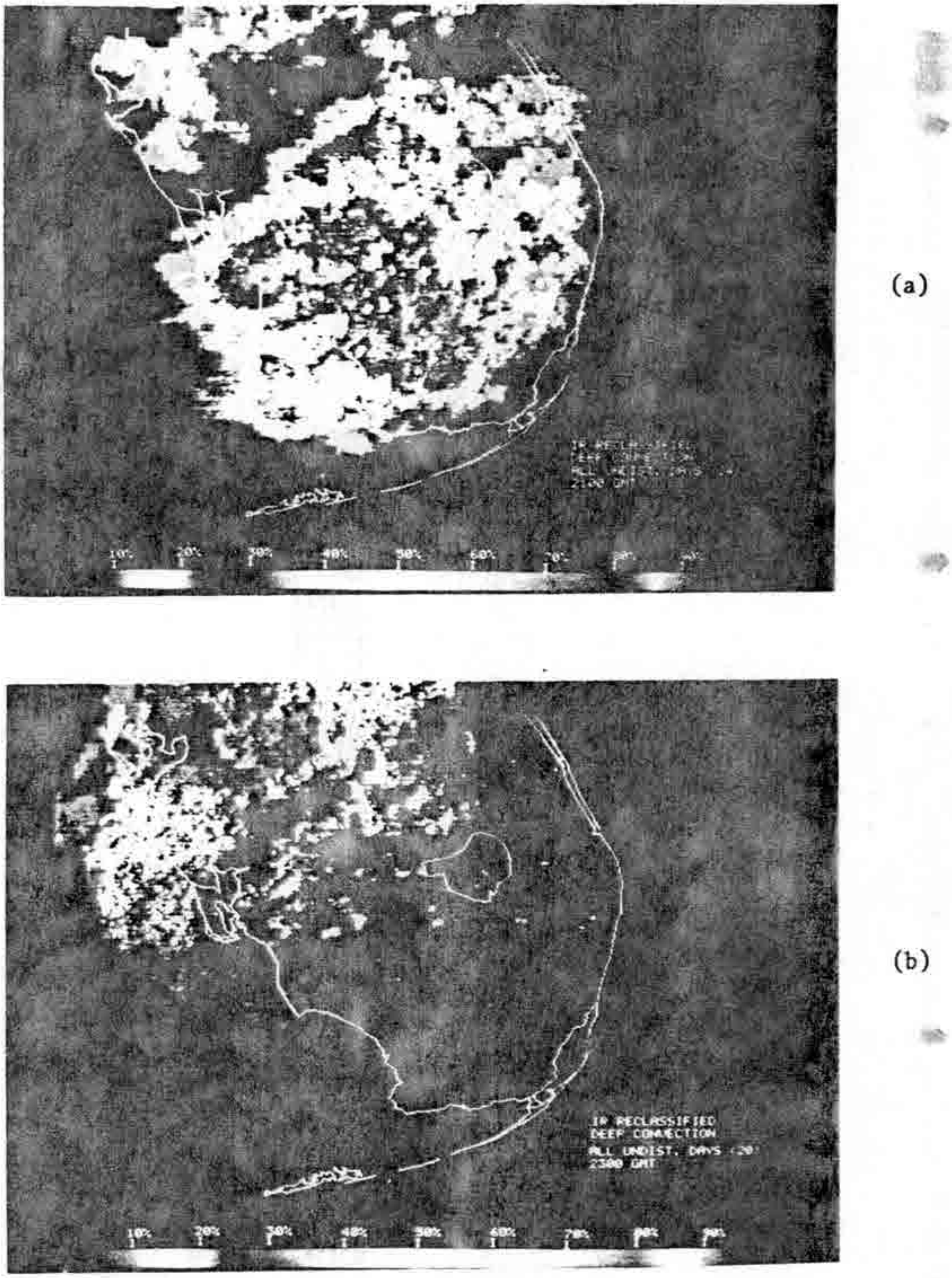

Fig. 2 All undisturbed days composite for deep convective clouds for (a) 1600 EST and (b) 1800 EST. Bar on the bottom of image relates shading to cloud frequency (on originals, a color bar is used). The number in parentheses on each image label indicates the number of images which went into creating the composite. (Color slides of all the composites are available, which more clearly illustrate the cloud composite frequencies; costs prevented reproducing these figures in color for this report). (From McQueen and Pielke, 1985.)

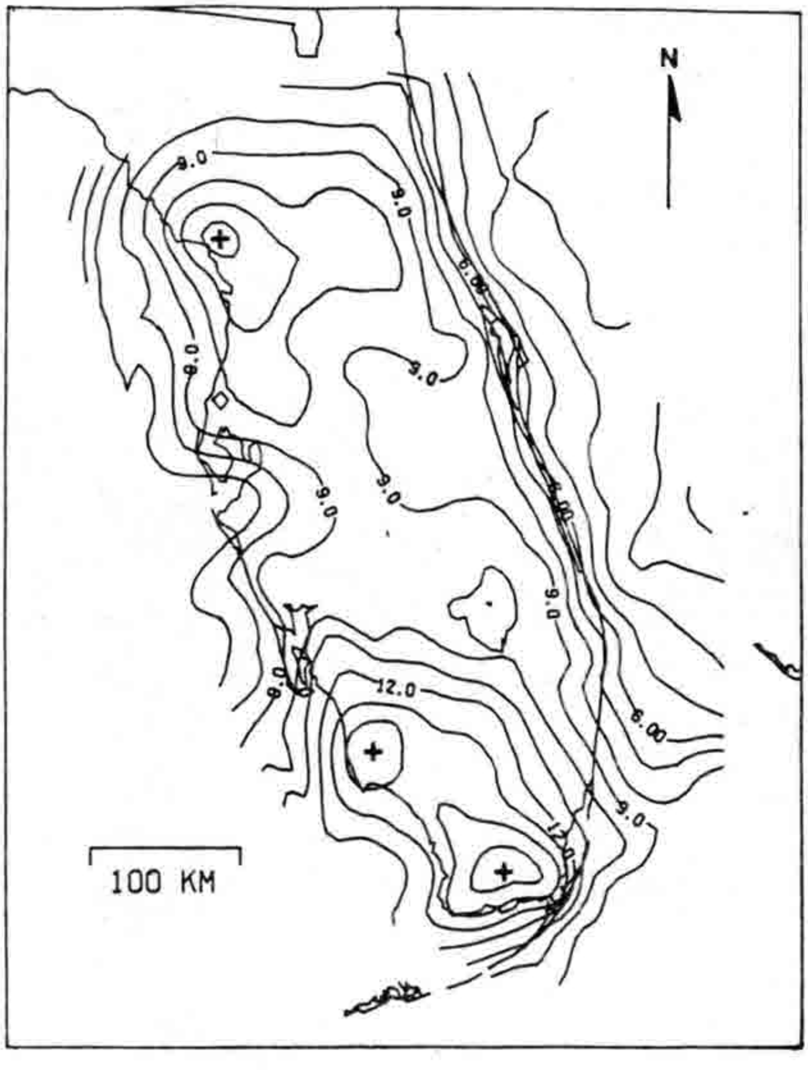

Fig. 3 Mean percent of hours that MDR VIP return of 3.0 or greater is observed. (From Michaels et al., 1987.)

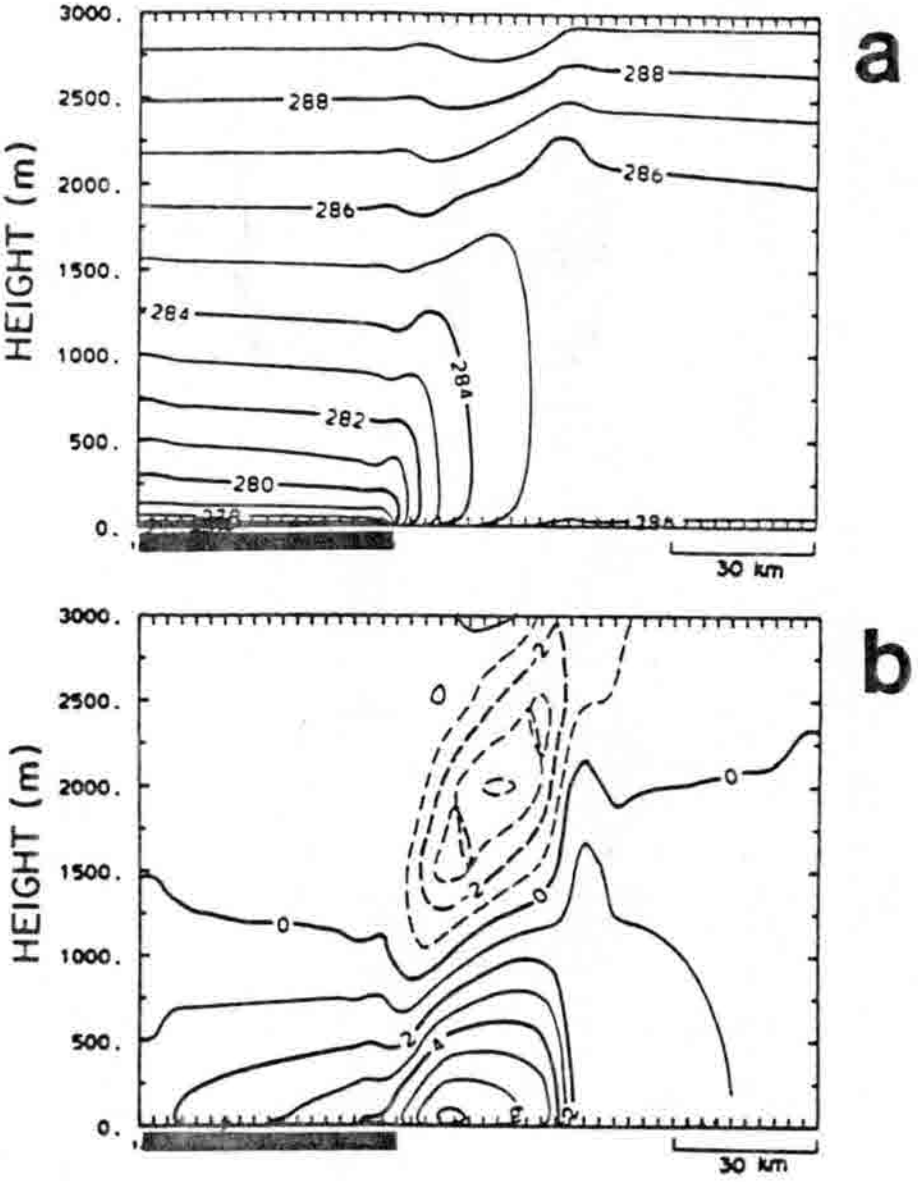

Fig. 4 Vertical cross sections of a simulated snow breeze. a) potential temperature; and b) u-component of horizontal wind. Solid lines indicate positive values, dashed lines indicate negative values, dark line at base of figures indicates snow cover surface. (Performed by M. Segal, 1988.)

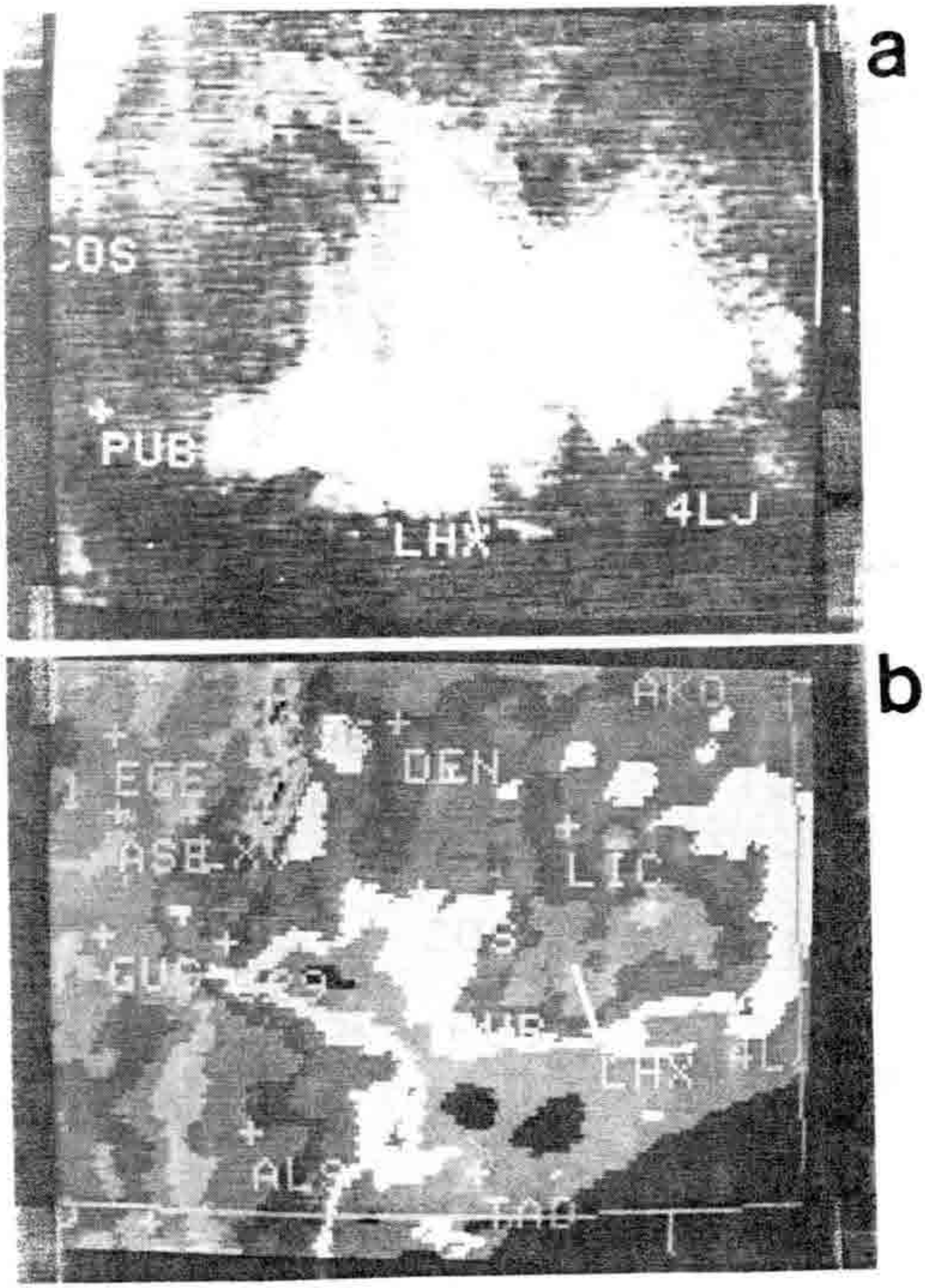

Fig.5 GOES satellite imagery from 12 Feb. 1988, photographed from NOAA SERS workstations. (a) Visible image, 1132 MST; (b) Infrared (IR) image, 1315 MST. Contour interval is 5 °C. The boundary between the green and orange colors represents the +5°C isotherm. (The line superimposed on the top figure indicates the flight transect.) (From Cramer, 1988.)

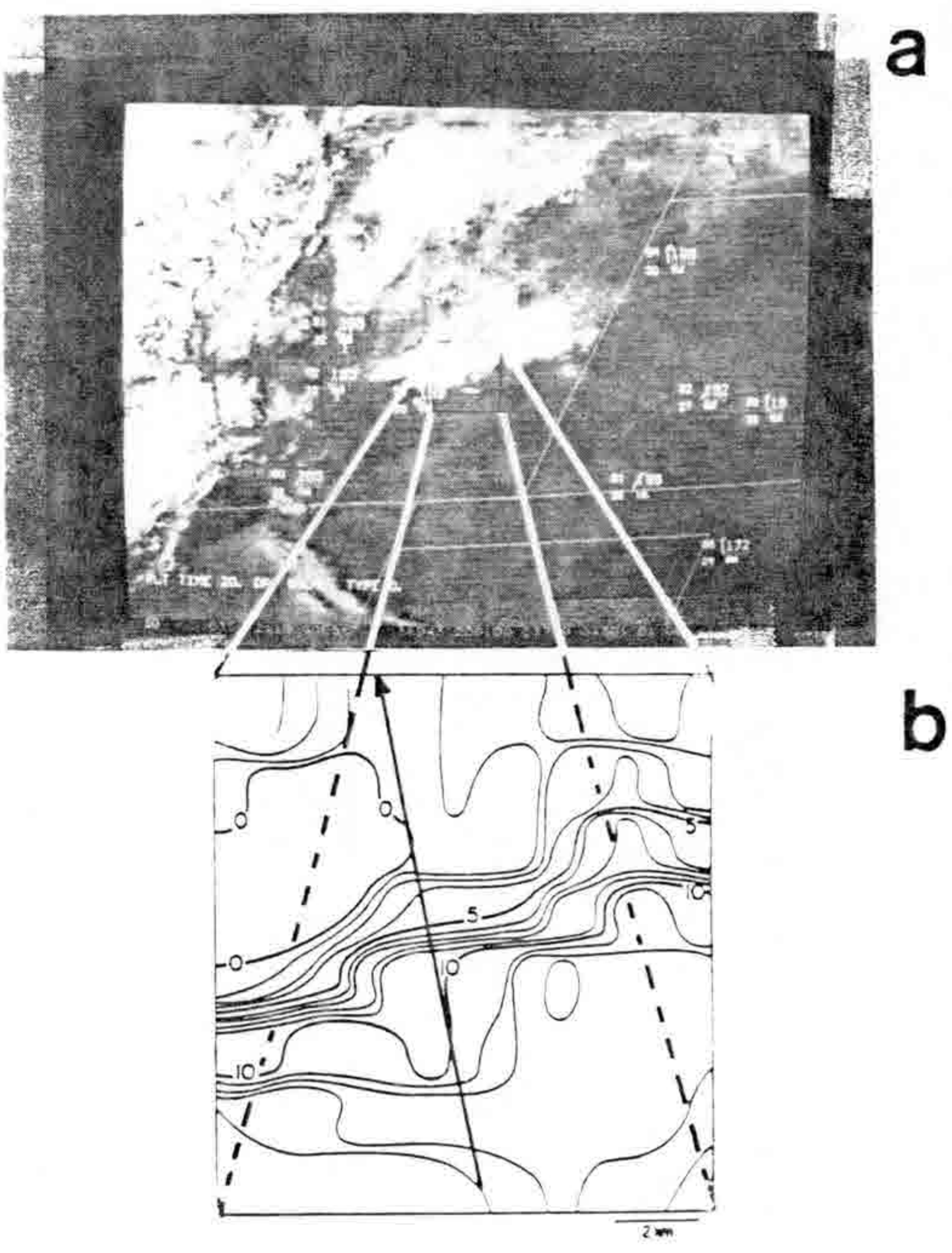

Fig. 6 (a) GOES IR satellite image for 12 Feb., 1988, 1301 MST and, (b) isotherm analysis (based upon MCIDAS image processing for area outlined by small square in (a)). Contour interval is 1°C. Heaviest line indicates portion of flight transect. (From Cramer, 1988.)

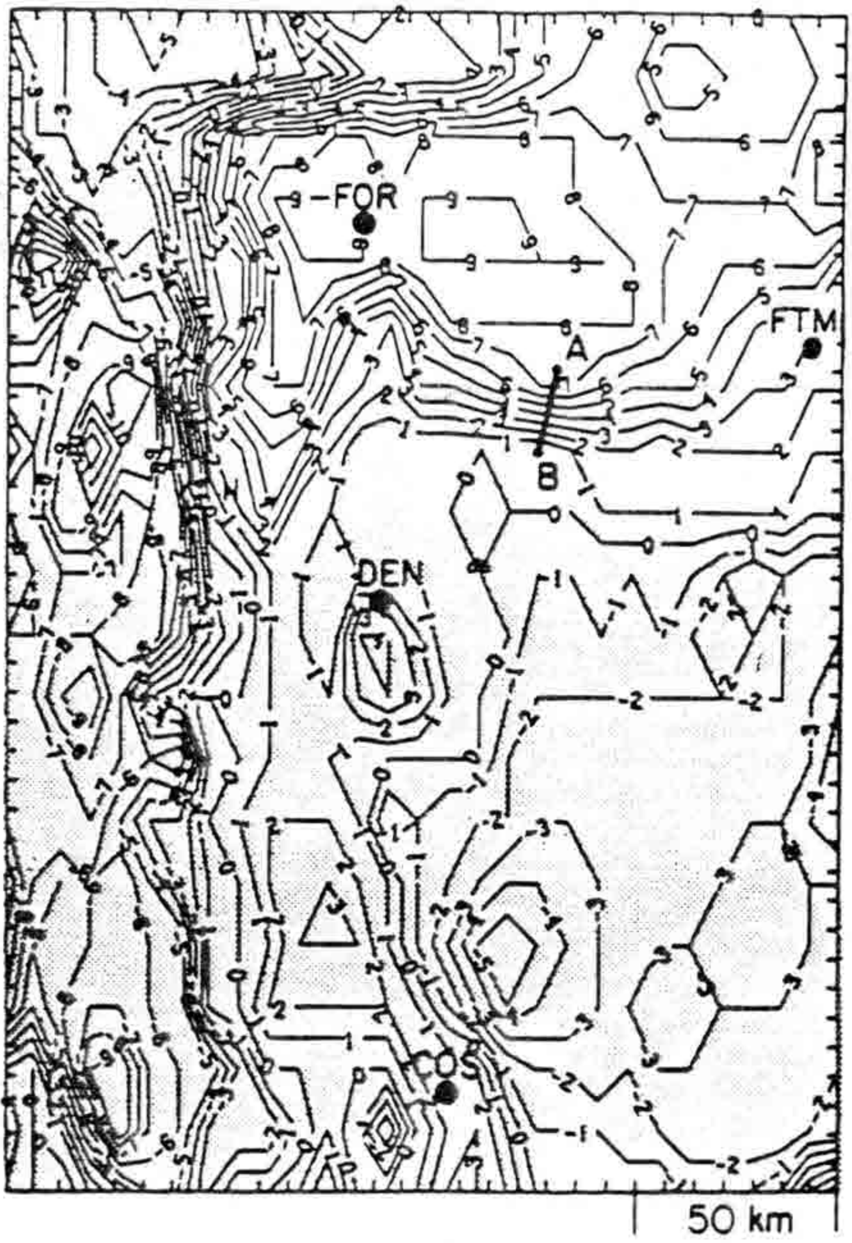

Fig. 7 Skin temperature (°C) analysis for 1300 MST, 10 January 1987. The stipled area indicates snow cover as seen by GOES visible imagery. Contour interval is 1°C (From Cramer, 1988.)

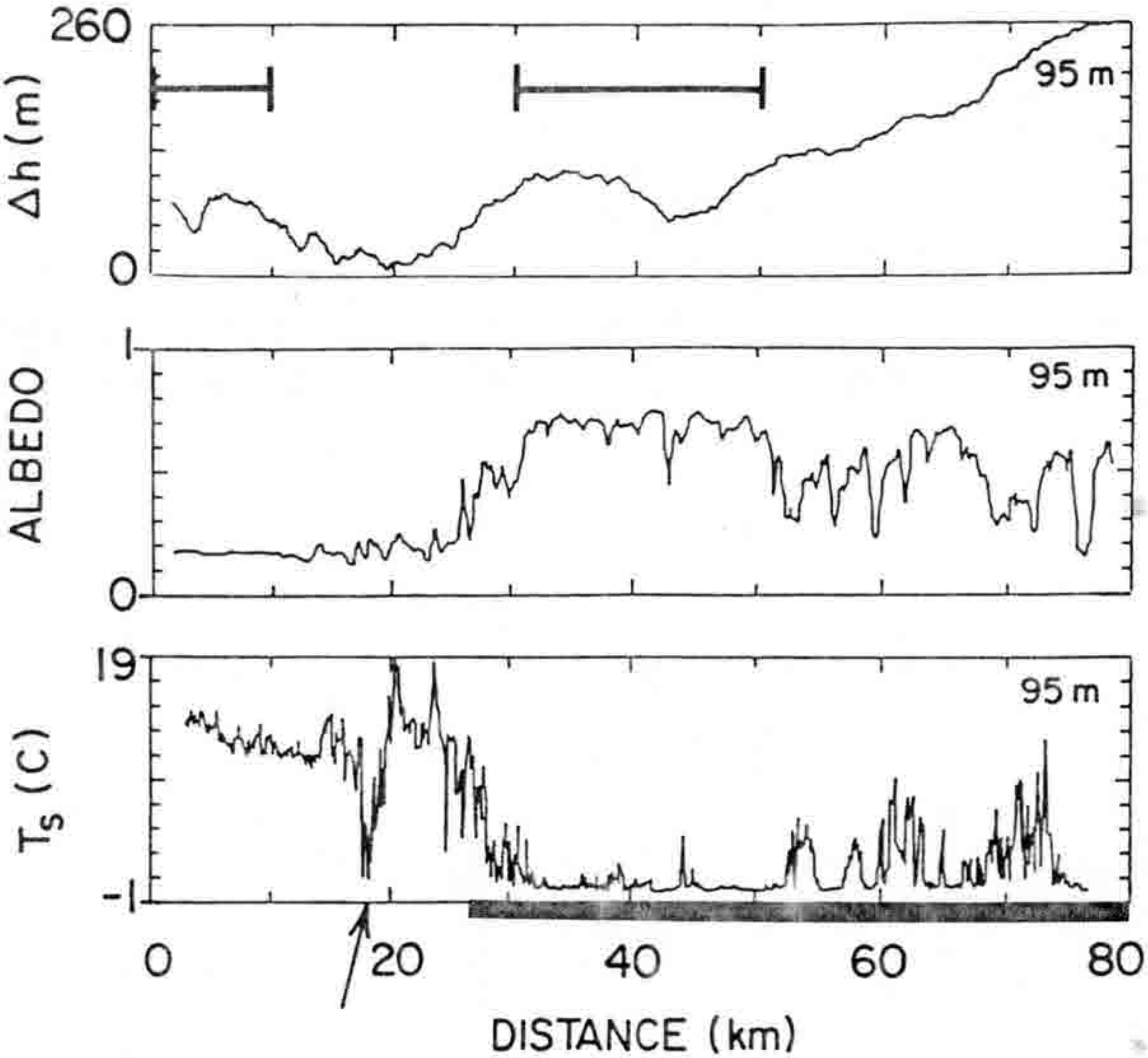

Fig. 8 (a) Plotted aircraft measurements from flight #1 horizontal transects are shown in Figs. (a-i). 95 m data measured from 12:48:48 - 13:13:24 MST. 170 m data were measured from 13:16:20 - 13:32:00 MST. 340 m data were measured from 13:34:18 - 13:48:30 MST. 710 m data were measured from 13:52:10 - 14:08:00 MST. Dark line indicates snow-covered portion of transect. The Δh plot was derived from measured air pressure. The arrow associated with the lowest terrain height depicts the location of the frozen Arkansas River. The bars on the Δh plot indicate the locations of the vertical profile measurements.

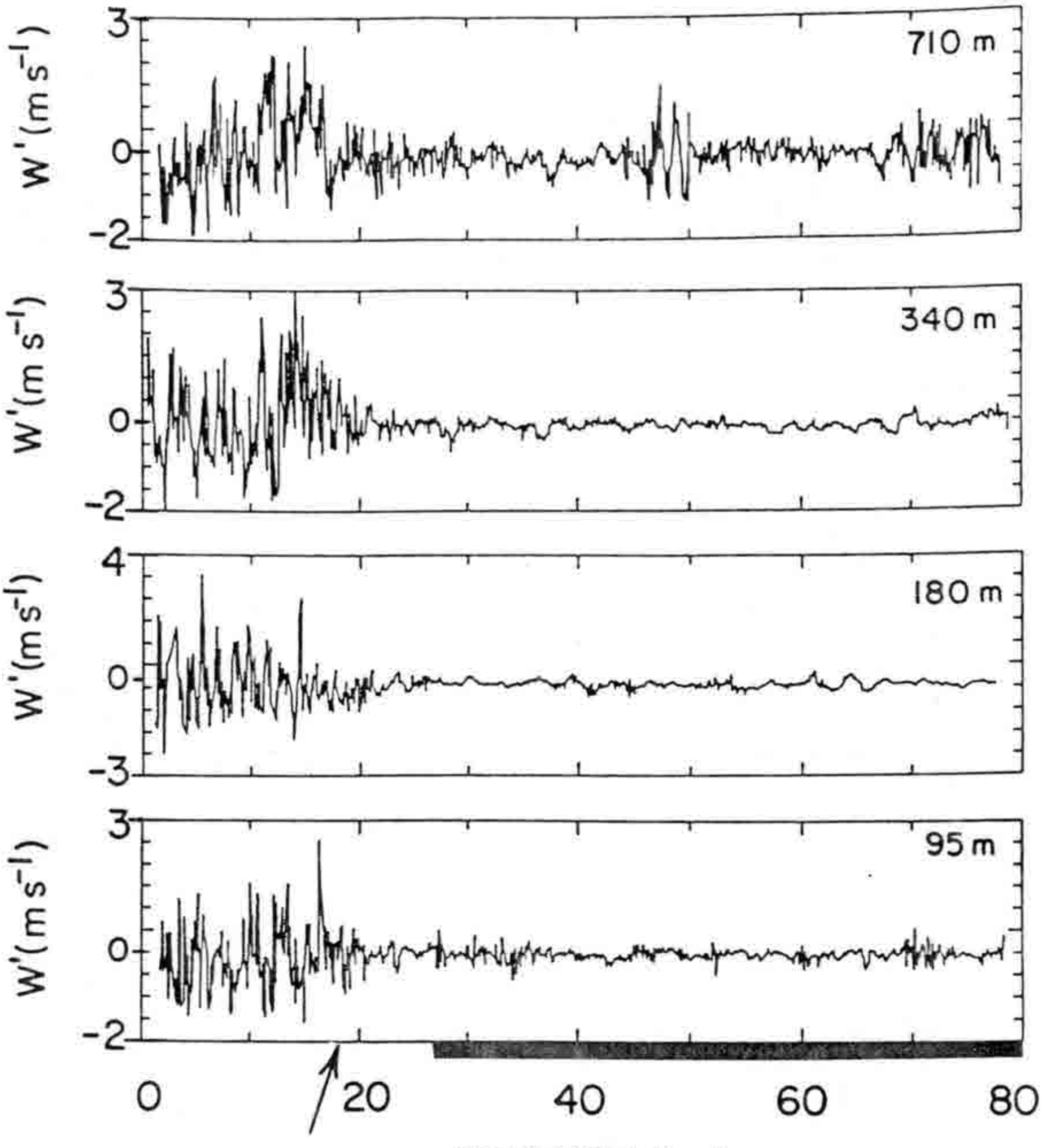

Fig. 8 (b) Tick mark interval of w' for 95 m, 340 m, and 710 m data is 0.5 m s^{-1}; for 180 m data it is 0.7 m s^{-1}.

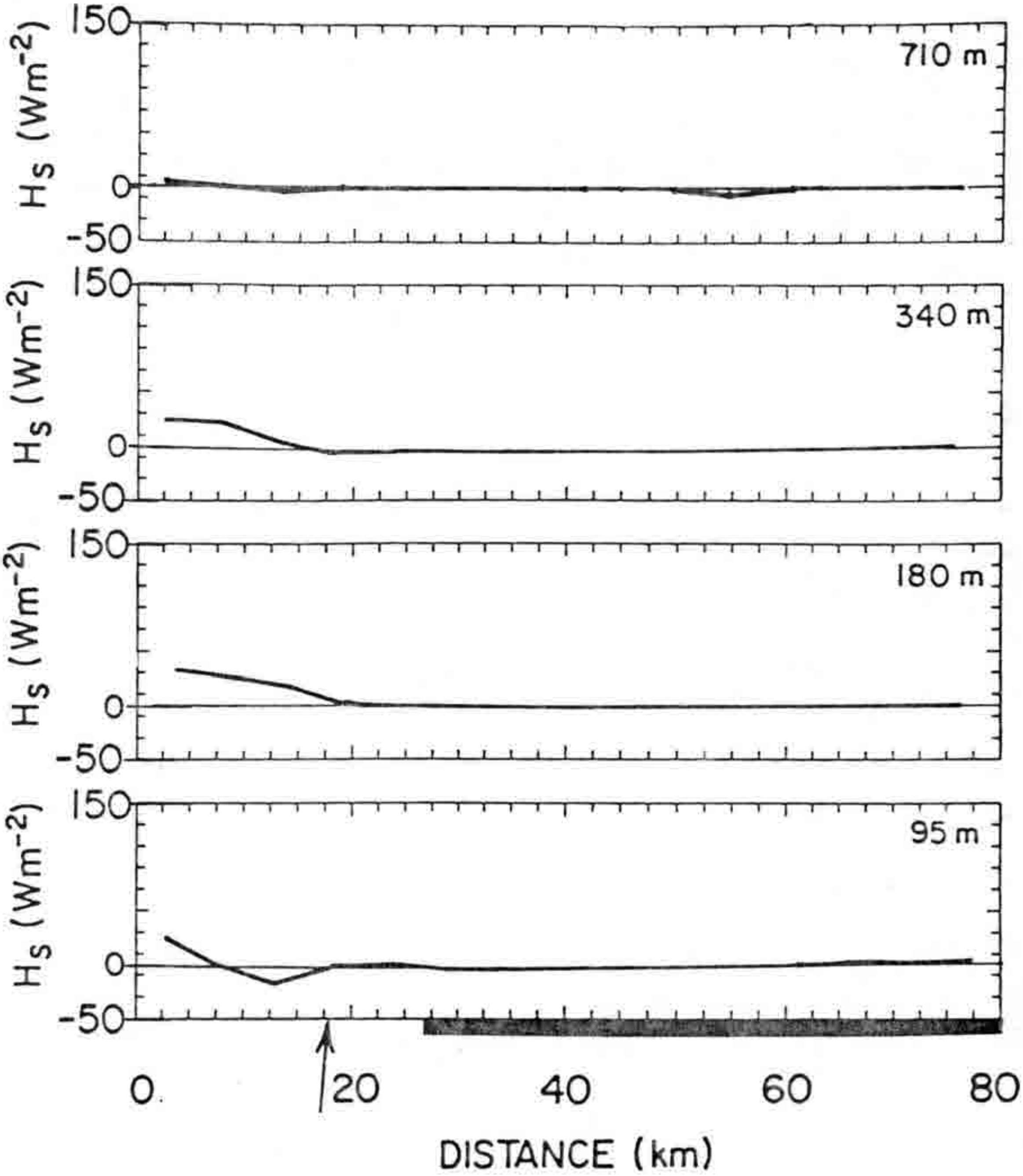

Fig. 8 (c) Tick mark interval of H_S is 20 W m^{-2}.

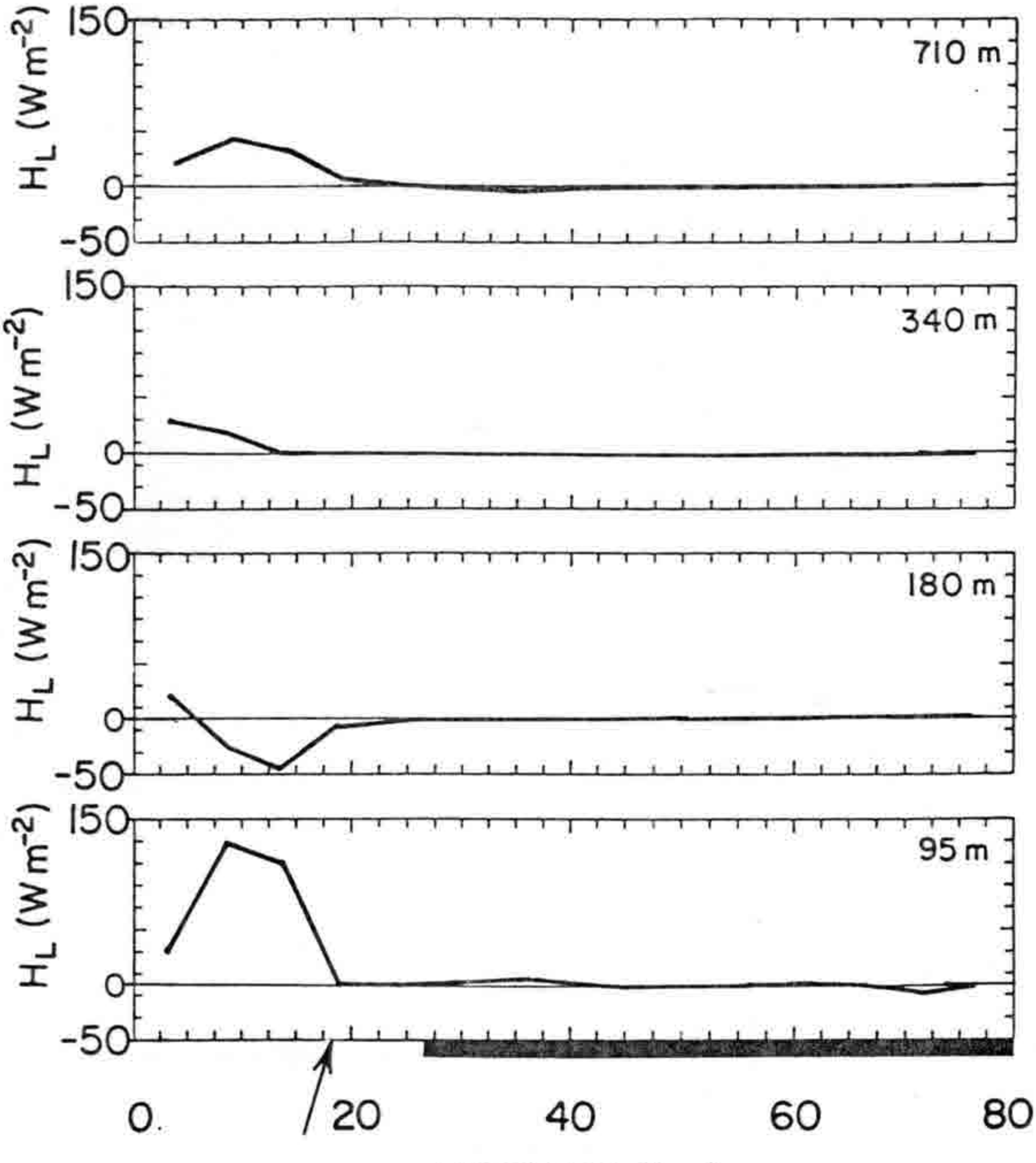

Fig. 8 (d) Tick mark interval for H_L is 20 W m^{-2}.

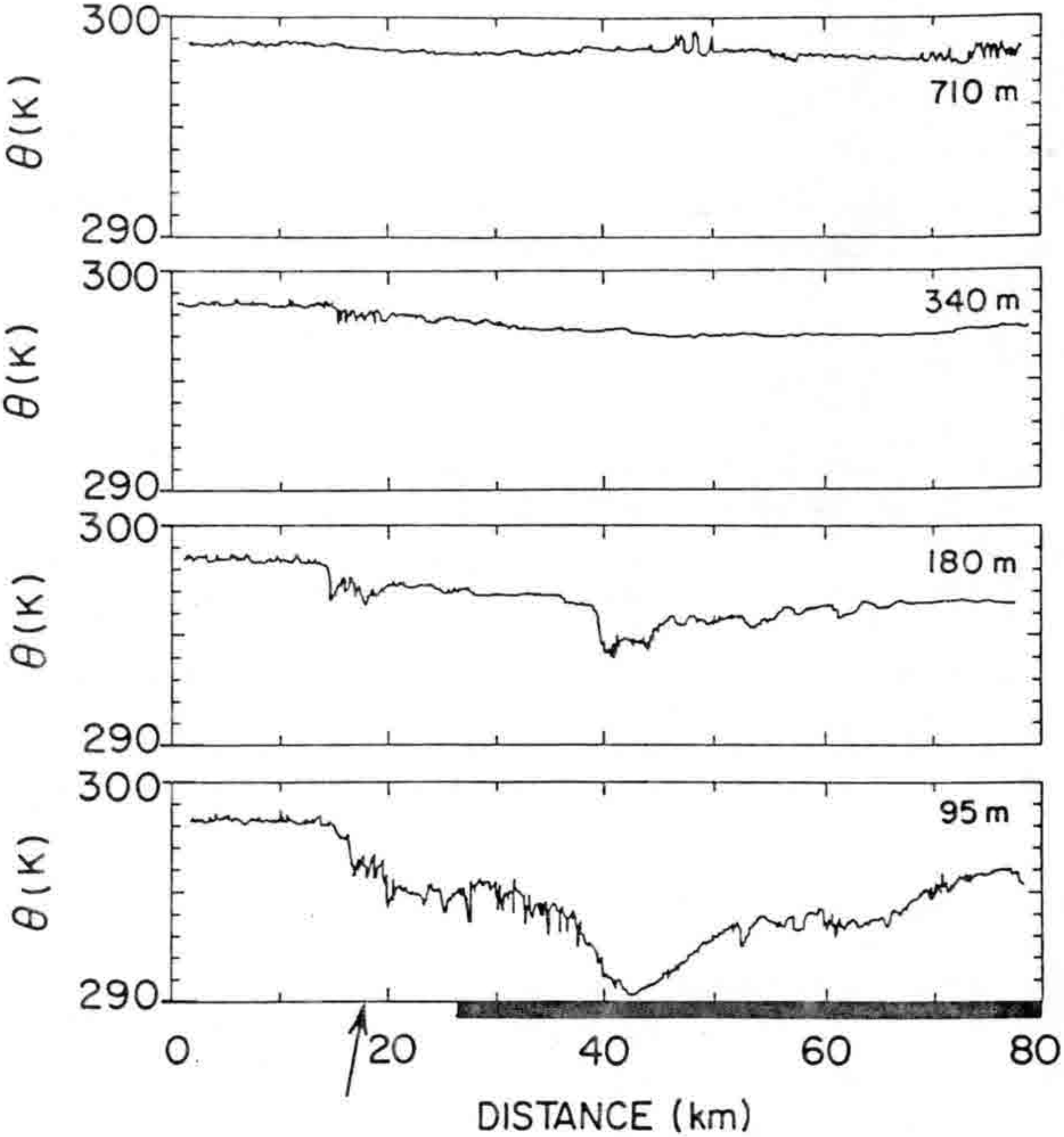

Fig. 8 (e) Tick mark interval for θ is 1 K.

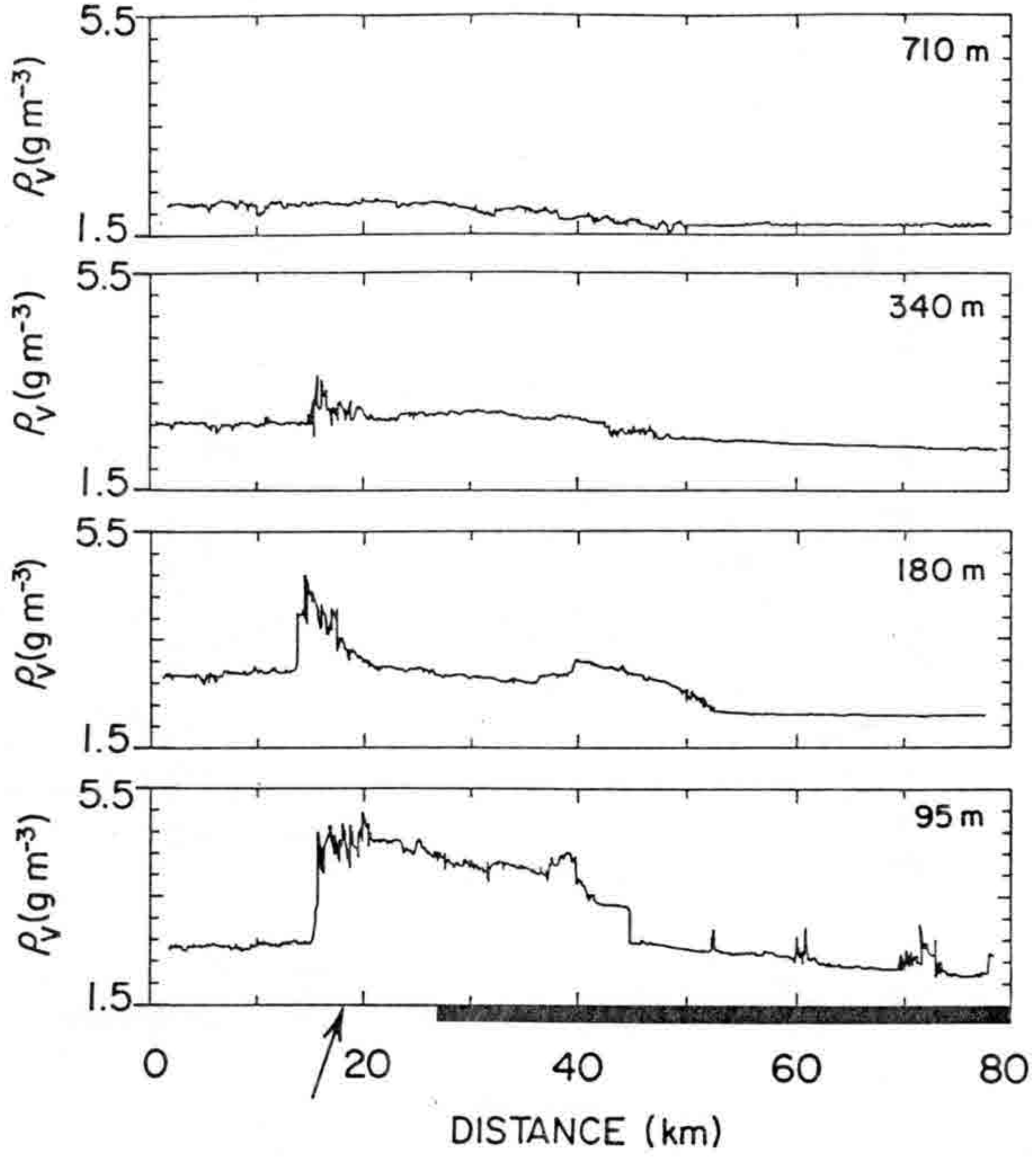

Fig. 8 (f) Tick mark interval for ρ_ν is 0.4 g m^{-3}.

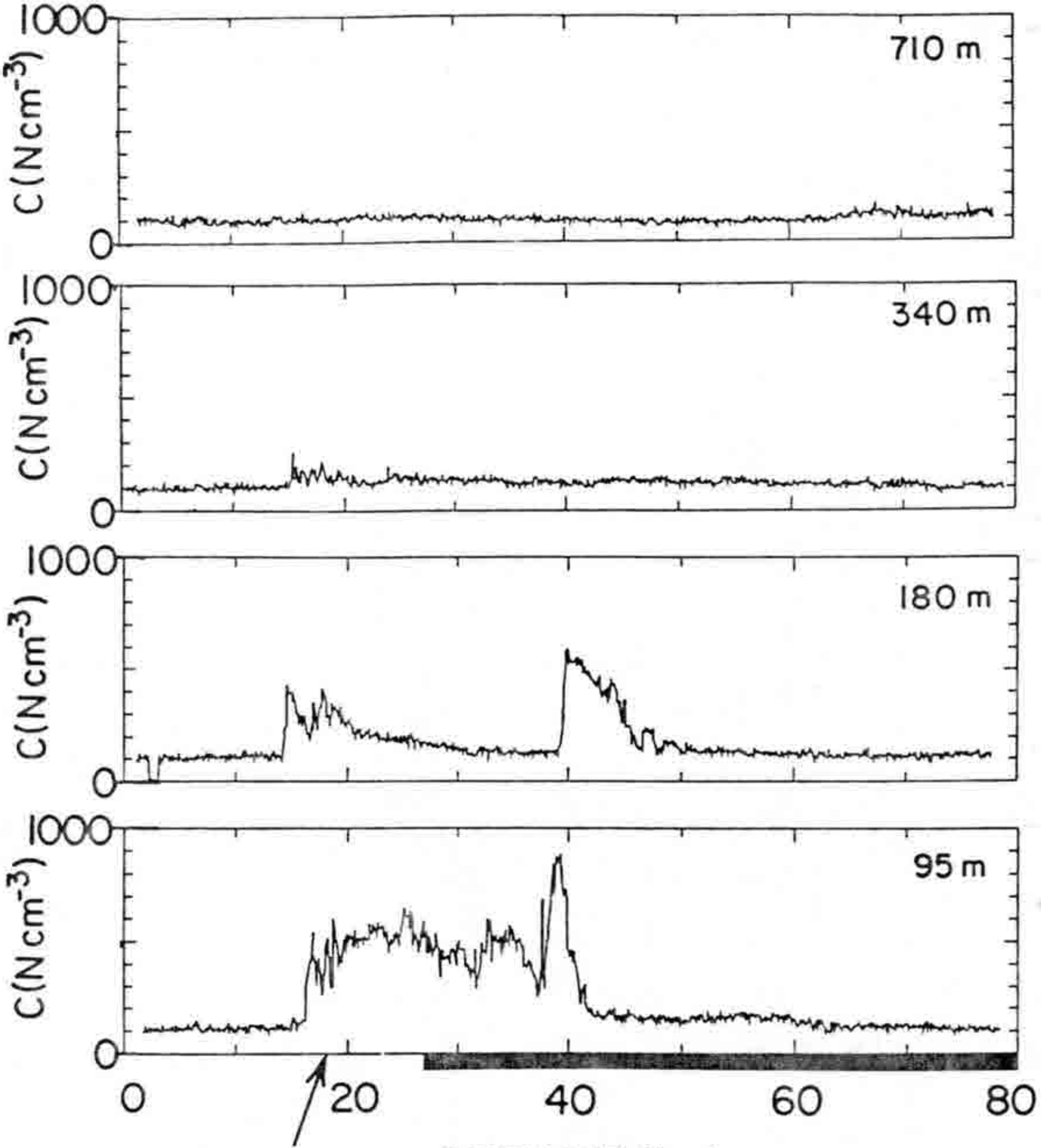

Fig. 8 (g) Tick mark interval for C is 100 particles per cubic centimeter.

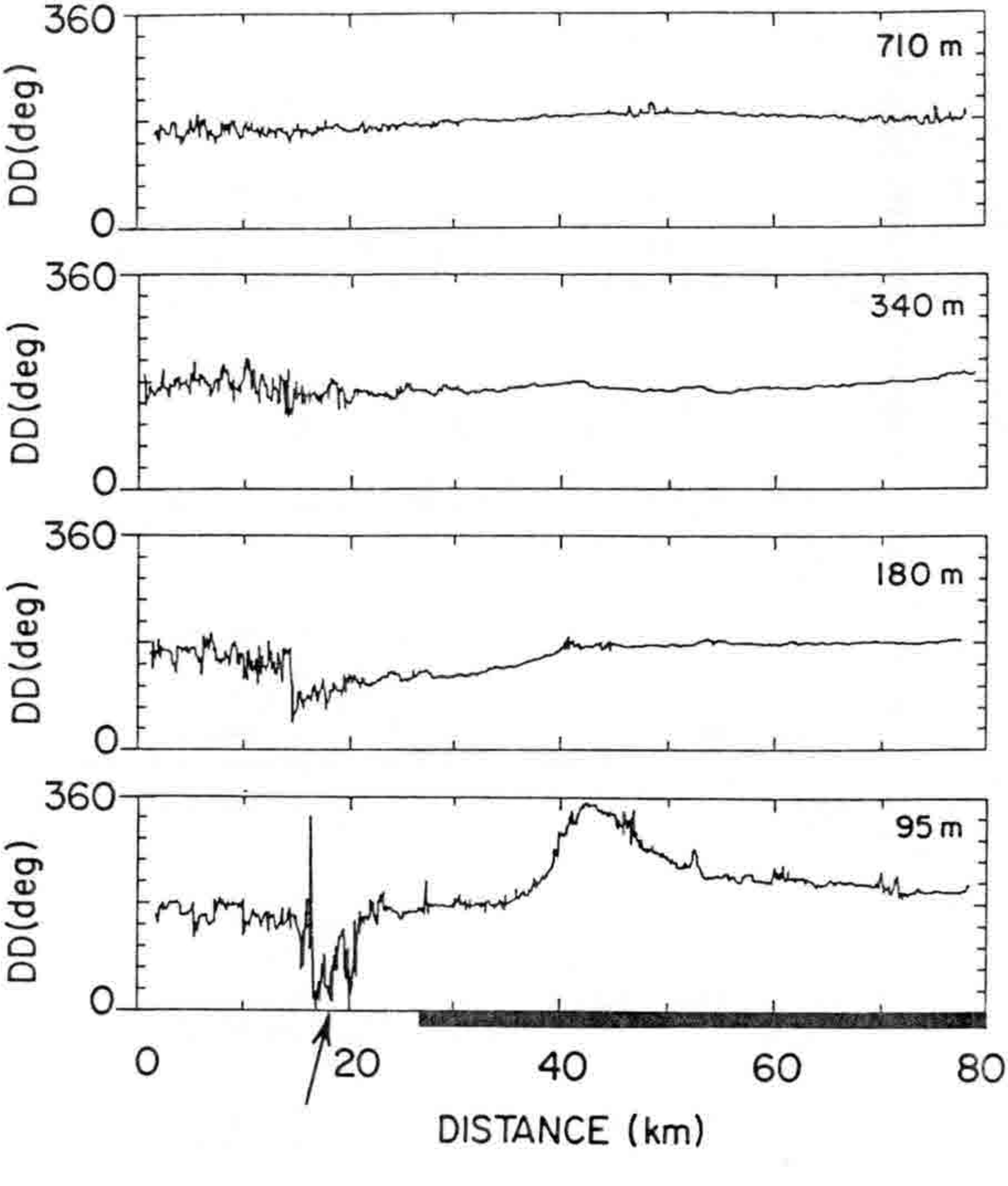

Fig. 8 (h) Tick mark interval for DD is 36°.

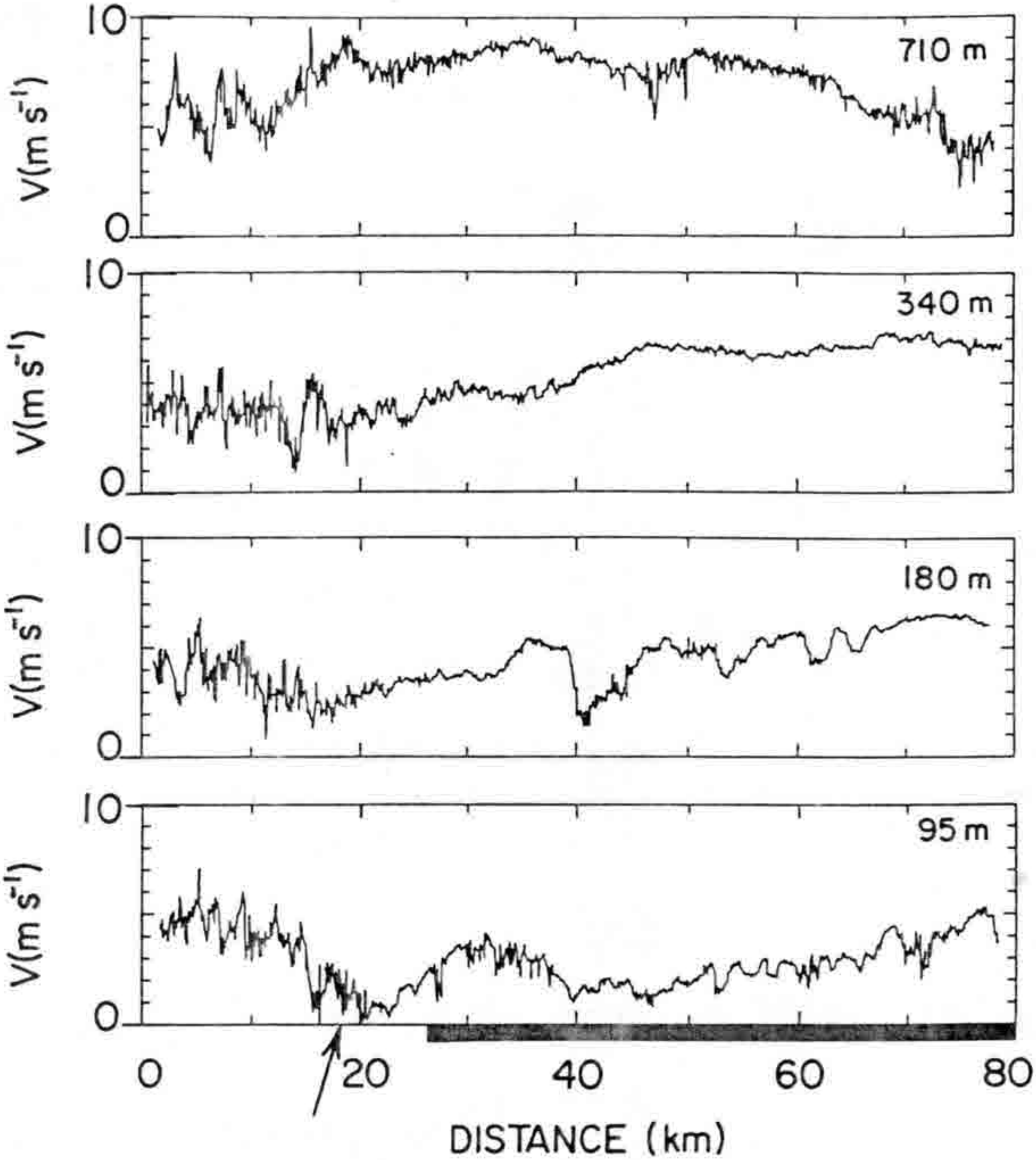

Fig. 8 (i) Tick mark interval for V is 1 m s-1.
(From Cramer, 1988.)

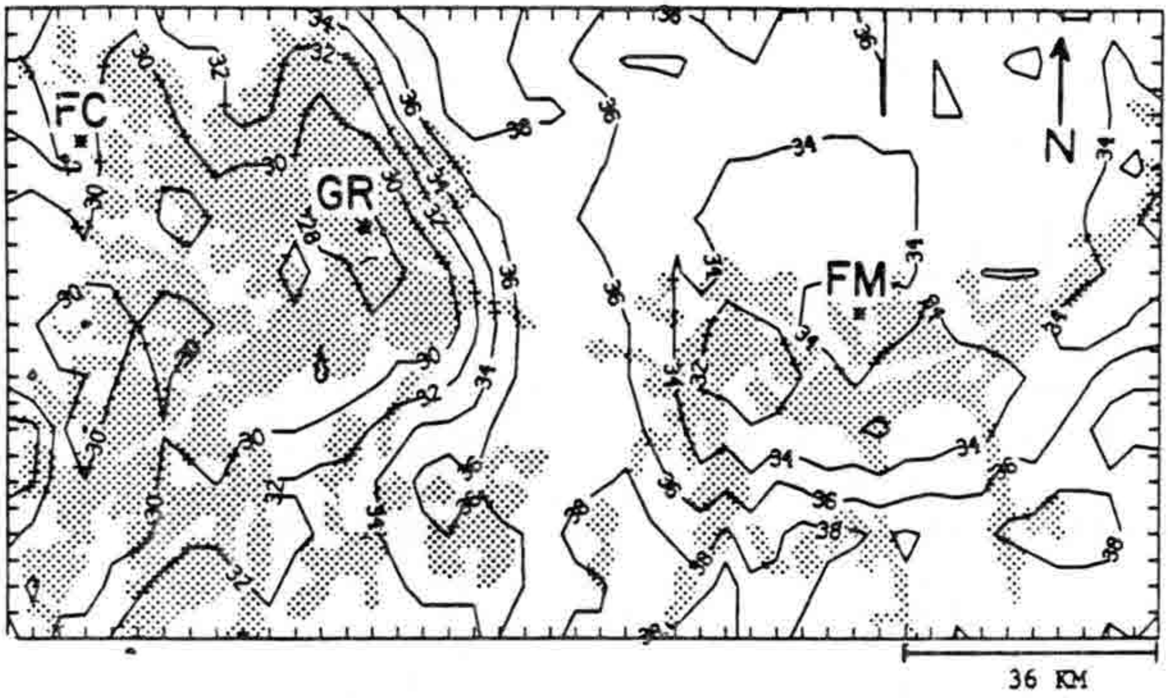

Fig. 9 Composite of GOES derived surface temperature at 1300 LST for the period 1 August 1986 to 15 August 1986 for northeast Colorado (FC-Fort Collins; FM-Fort Morgan; GR-Greeley). (From Segal et al., 1988.)

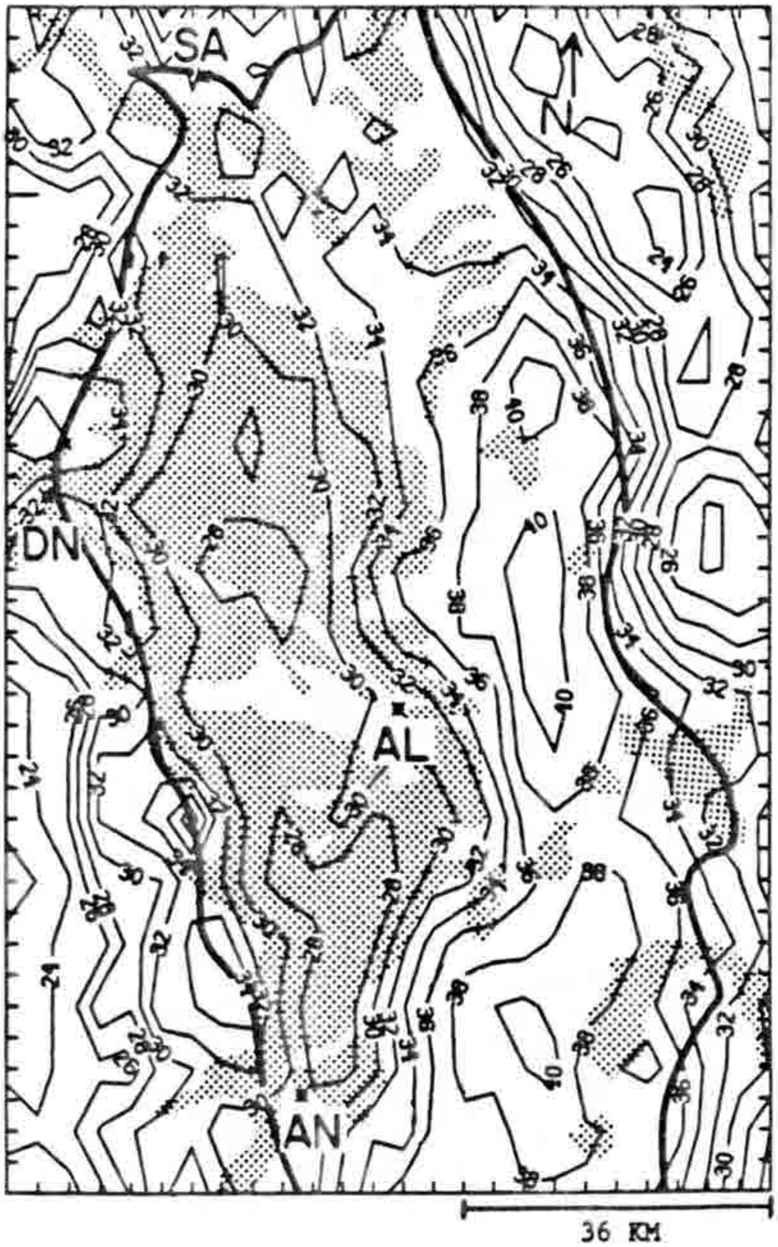

Fig. 10 Same as Fig. 9 except for the San Luis Valley in Colorado (AL-Alamosa; AN-Antonito; DN-Del Norte; SA-Saguache). The lower valley is outlined by a dark line separating it from significant elevated terrain. Irrigated areas are shaded. (From Segal et al., 1988.)

$$\frac{\partial u}{\partial t} = -\frac{\partial}{\partial x}u^2/2 - \alpha_o\frac{\partial p}{\partial x} + R$$

$$\int_{D_x}\frac{\partial u}{\partial t} = \frac{\partial u_o}{\partial t} = \frac{-1}{D_x}\left[\frac{u^2}{2}\,|_w - \frac{u^2}{2}\,|_e\right] - \alpha_o(p_w - p_e)/D_x + \int_{D_x} R$$

Fig. 11 East-west equation of motion.

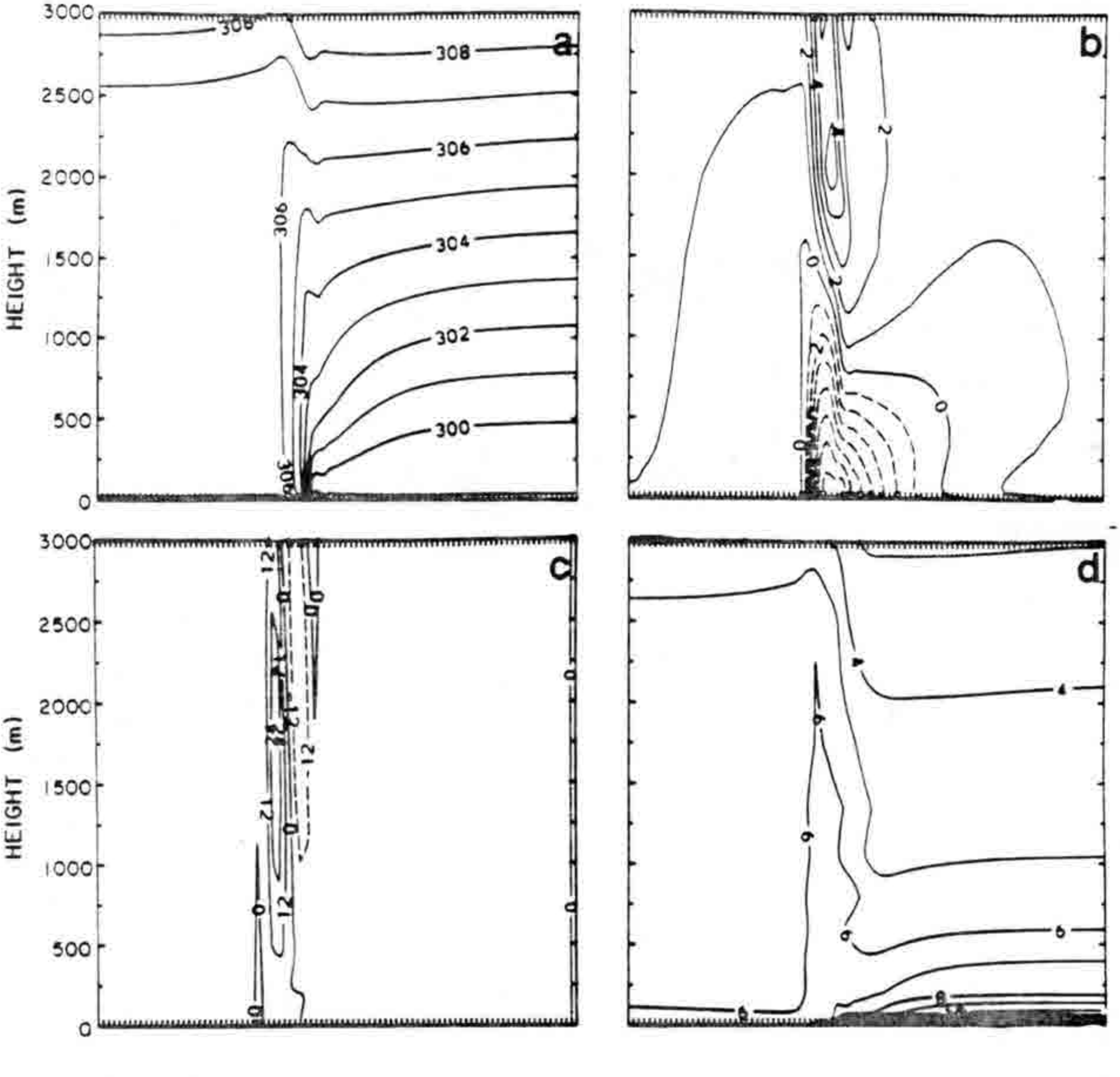

Fig. 12 Simulated sea-breeze fields in a vertical cross-section at 1400 LST for the contour case: (a) potential temperature, θ, (K) (b) cross- shore wind velocity component, u , (m s^{-1}), (c) vertical velocity, w, (cm s^{-1}), (d) specific humidity, q, (gr/kg). The sea segment is indicated by dark. (From Pielke et al., 1989.)

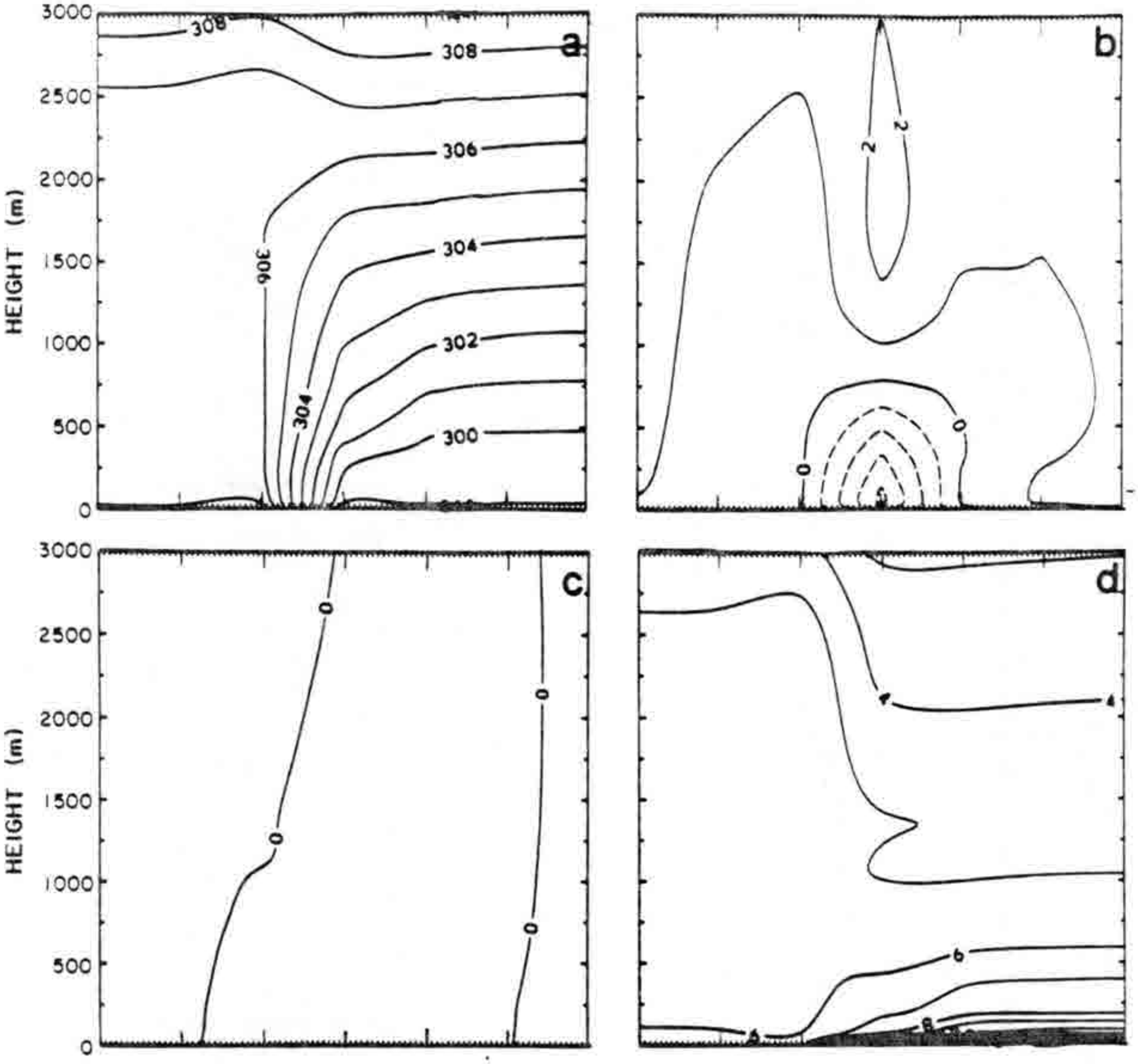

Fig. 13 Interpolation of the fields described in Fig. 12 using the simulated vertical profiles every 90 km (at points 1-7) as a data base. (From Pielke et al., 1989.)

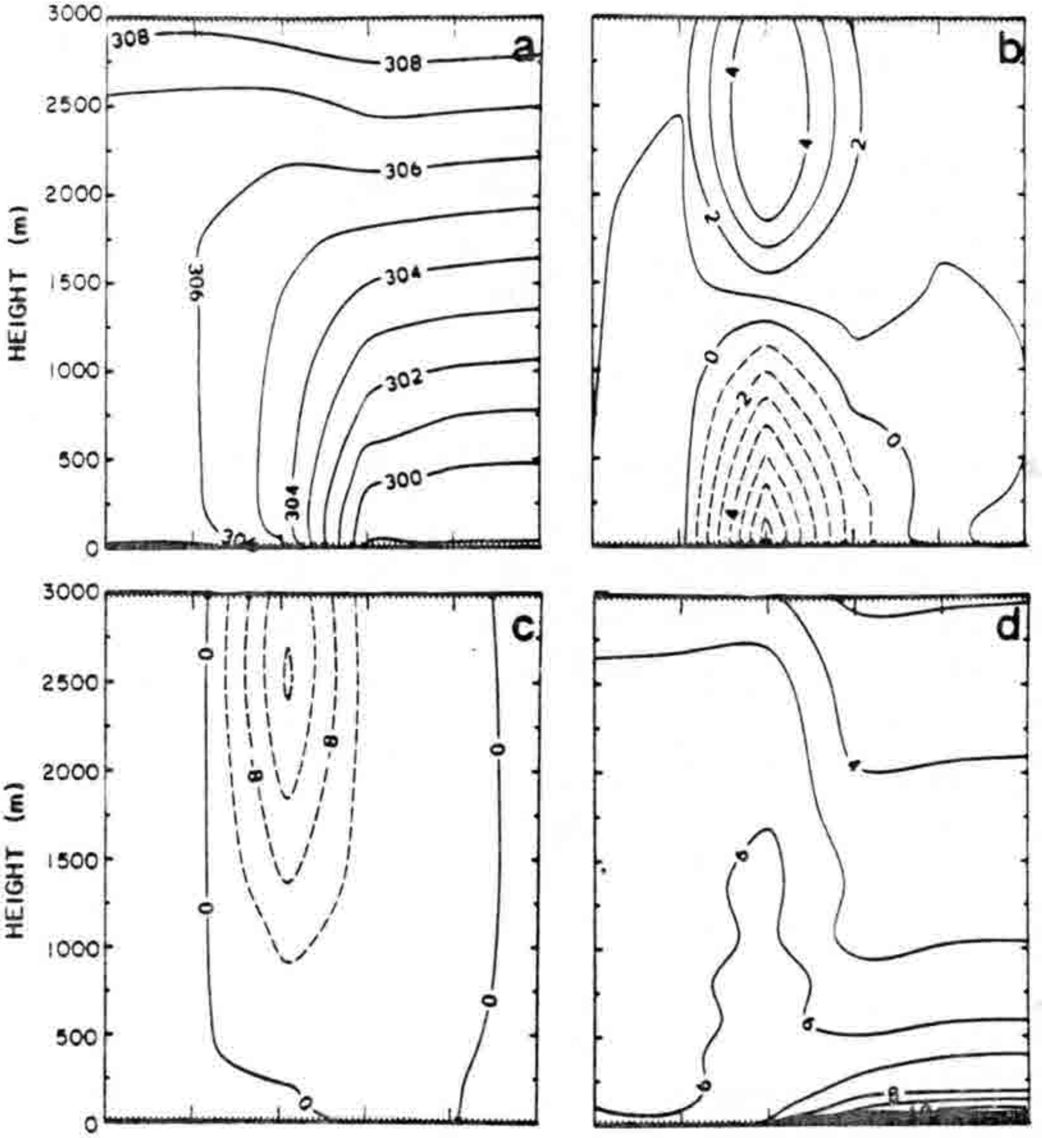

Fig. 14 The same as Fig. 13 except for shifting the vertical location of the profiles used in Fig. 13 by 45 km. (From Pielke et al., 1989).

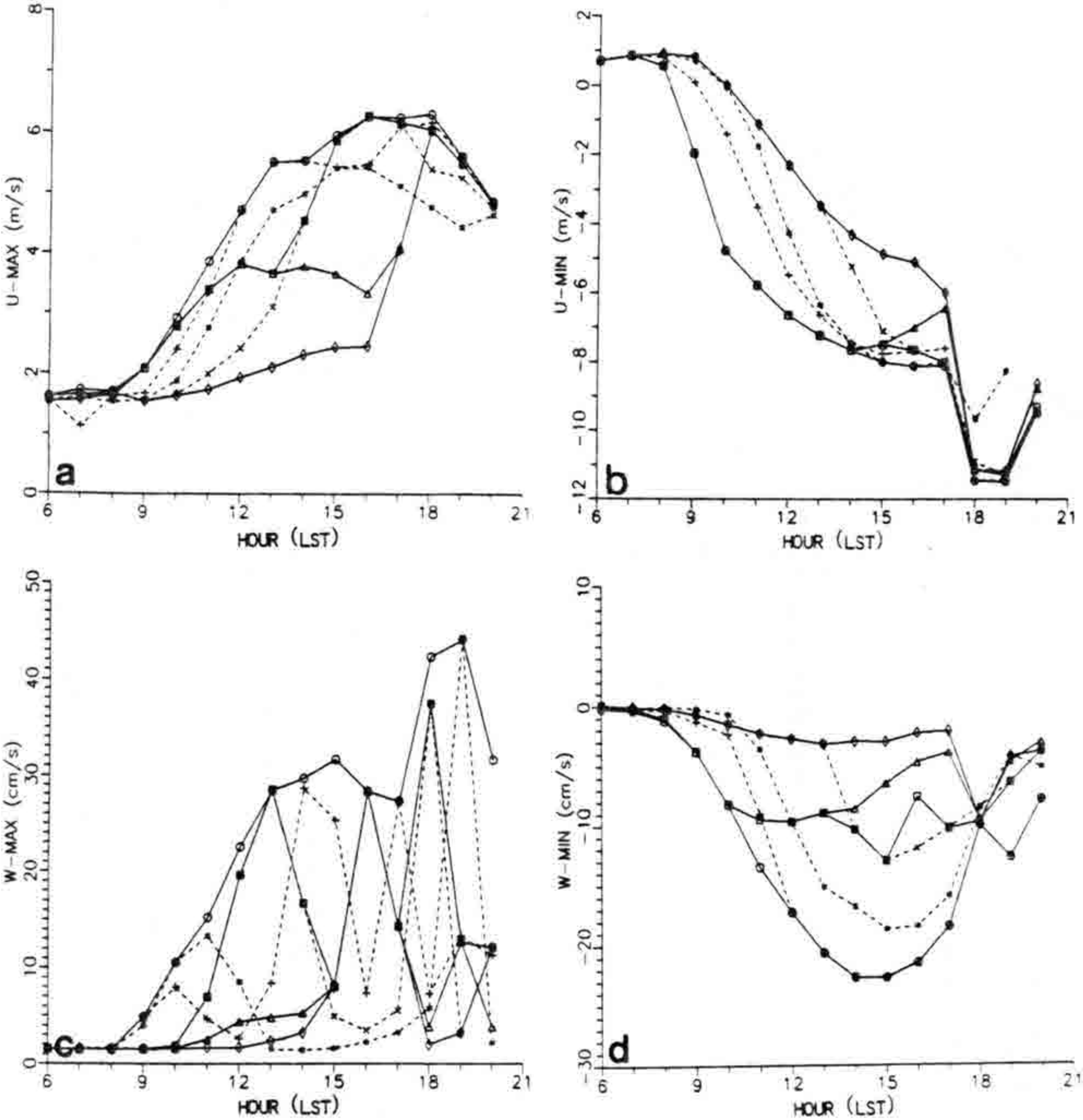

Fig. 15 Simulated maximum and minimum *u* and *w* within the lowest 3 km of the vertical cross-section of the analysis domain as dependent on time (0-6 km resolution; 30 km analysis; Δ 60 km analysis; ◇ 90 km analysis; + 30 km analysis with 15 km shift; x 60 km analysis with 30 km shift; * 90 km analysis with 45 km shift). (From Pielke et al., 1989.)

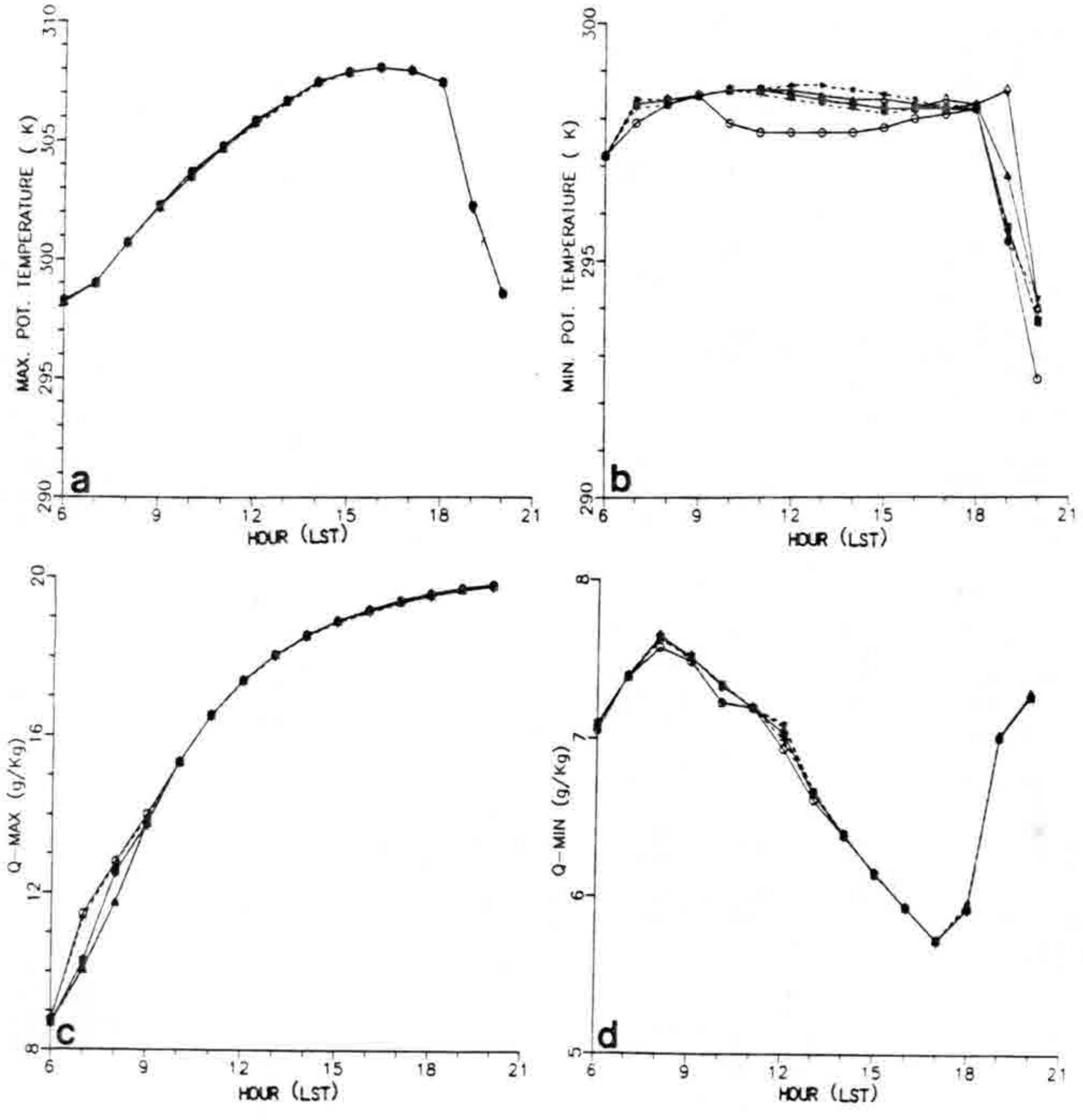

Fig. 16 Simulated maximum and minimum values of θ and *q* along the simulated and analysis domain at a height of 5 m as a function of time (curves indicated as in Fig. 15). (From Pielke et al., 1989.)

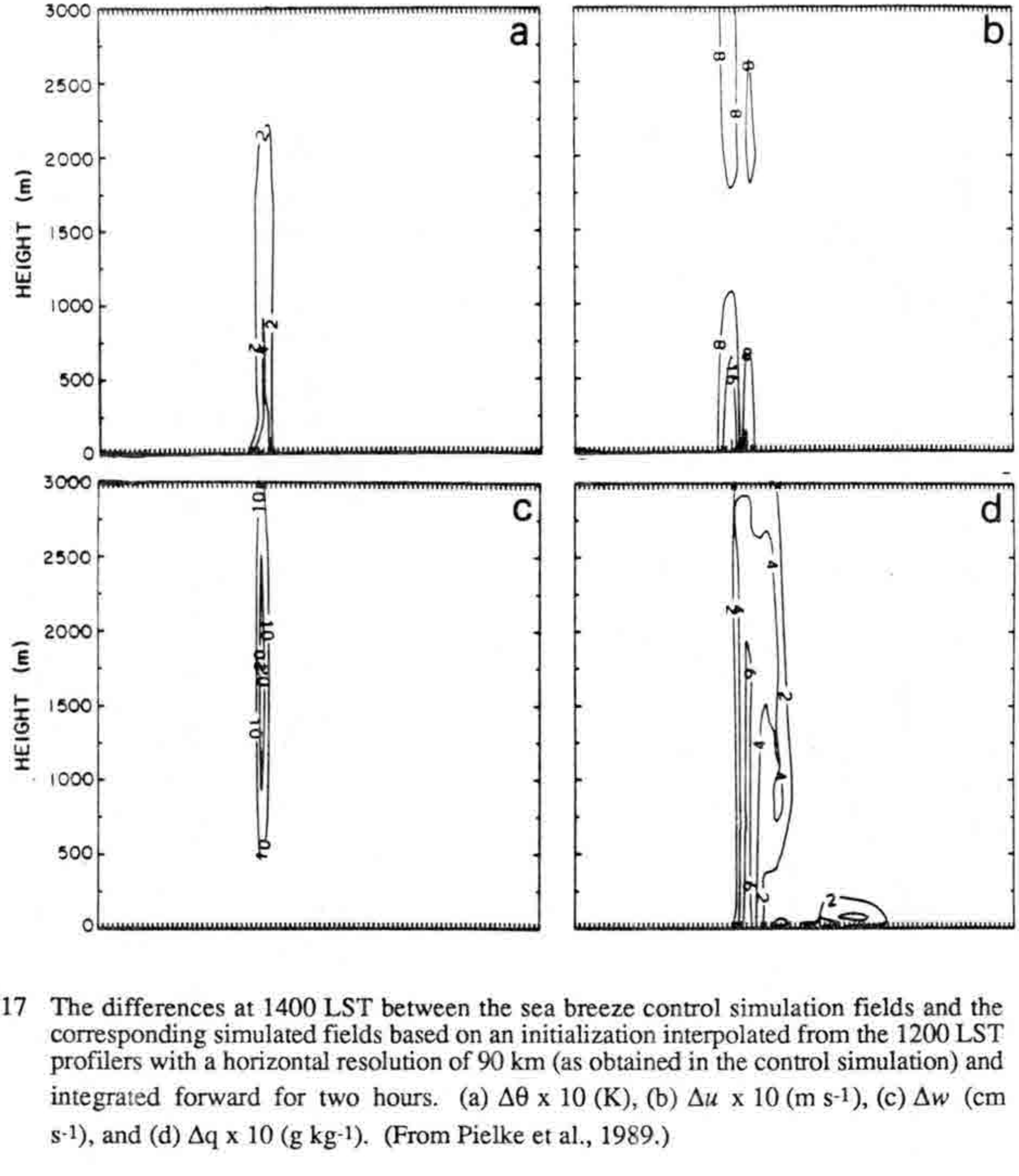

Fig. 17 The differences at 1400 LST between the sea breeze control simulation fields and the corresponding simulated fields based on an initialization interpolated from the 1200 LST profilers with a horizontal resolution of 90 km (as obtained in the control simulation) and integrated forward for two hours. (a) $\Delta\theta$ x 10 (K), (b) Δu x 10 (m s^{-1}), (c) Δw (cm s^{-1}), and (d) Δq x 10 (g kg^{-1}). (From Pielke et al., 1989.)

Tropical Cyclones

Talk given by William Gray
At the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

I can remember when Vern Suomi came to Chicago in 1957 and told us how weather satellites would revolutionize the science of meteorology. It is questionable as to how large the impact of satellites has been. But, perhaps with tropical storms, because they exist over the data void oceans, it's been a remarkable tool. It finds storms, it tracks them well, it tells rainfall. Dvorak has developed a nice scheme to tell how intense the storms are and the new computer capabilities can do a lot. They're a remarkable tool and of course we don't want to take them back. Tropical storms is a specialized area, of course. I can remember being at Miami in 1961 and 1962 and our old trusty forecasters down there had been forecasting for years without satellite data. The satellite experts in Washington would look at their satellite pictures and call Miami, "Look we've got a storm out here." And there was a great big white area and it wasn't a storm. It hadn't gotten organized yet. Anyway, the pictures weren't so good. Neil Frank was the only young kid in our shop that would look at the satellite pictures. All the old guys said, "I don't want to see them!" So from that date on there's always been this sort of give and take between the satellite people, who have done great things with tropical storms, and the forecasters.

Now how have the forecasts been since 1960? Track prediction has improved since the satellite ...... about 1/2% per year. The big jump forward that occurred with the satellite and so many other things, hasn't really come in the tropical storm field as much as we might have hoped. Although I think there is a lot that can be done. I'm giving a little background here. Anyway, there has been this give and take back and forth between the satellite people and the tropical storm people. Speaking from a tropical storm person's point of view, they have implied that satellite can do a bit more with tropical storms than it really can. And what the forecasters can do I would say has often not been appreciated. You see it's one thing to say data is not used. The satellite can do this and that but what you've got to do is really go to a number of cases and show by having the satellite data there you could forecast better than you otherwise could without it. You can't do it with one case or two cases. You've got to do it with 15 or 20 cases or so. So there's sort of been this back and forth thing. Now, that's all been healthy. We had this Bangkok workshop two or three years ago. Vern Dvorak was there and Bob Sheets was there, and Bill Smith was there too. All during the two weeks there was sort of a back and forth tug of war there. Bob Sheets was saying, "Look, the satellite can't do this; It can't do this." And Vern Dvorak would say "Look we're doing this and the foreign people who happen to not have reconnaissance are doing this with satellite data. You know the U.S. is the only country that hasn't. There is no other tool like it." So when you've got the satellite and you don't have aircraft reconnaissance, it's a remarkable tool. But having the satellite and aircraft both is a marvelous combination.

Because of the budget cuts now you all know that in the northwest Pacific the recon program as of August of last year has been dropped. It's been dropped basically on the grounds that the satellite can pretty well do it. And it's recon flying into nearly every typhoon that was out there that started right after the second world war and went for over 40 years that has been discontinued. Now the question is how much did the satellite data improve forecasting. Well, that's one thing, but now the DoD people that want to save 20 million dollars by dropping the West Pacific recon squadron now say the satellite can pretty well do it by itself. So there are some plans now that the Air Force stop flying in the Atlantic, eventually. I think there will be so much political pressure that the flying will go on and they probably won't stop it in the near future. We will see there's great pressure to stop this flying and the reason for it is the satellite can do it all or it can do it close enough, that what actually the aircraft can do, is not worth the 20 million dollars per year or 25 million dollars per year or combined 40-50 million dollars per year that was going on. In all fairness the satellite people aren't devoted to doing away with the aircraft. But, that's the excuse that's being used. As a satellite meteorologist of course you want the aircraft data because that's the validation for the satellite techniques that you develop. However, I think talking from the tropical storm point of view, there have been the implications made over the years that the satellite can do a little bit more than it really can do. Usually that claim is made in all honesty. I don't think anyone wants to pull any wool over anybody's eyes but you see the satellite people are usually not making the forecast. They're not down on the line doing that and they don't realize some of the problems involved there and so on.

Let me just mention a few things. We've got satellite people at Miami making forecasts but not the official one. I don't know why they don't trust satellite people to make the forecast, but they don't. When you have a tropical cyclone with a well-defined eye, satellite images do quite well in locating the center. However, with weaker systems, it is often less obvious where the center is as revealed by satellite imagery. You don't know, but if you fly the plane out from Guam to find the closed center and along with navigation you can be certain where the center is. Now in other times of course, when you have a well-developed eye, it's a different situation. Probably everyone can tell where the center of a well-formed system is. So satellites are very useful to tell where the center is, but how intense is it? They do a pretty good job. Dvorak has developed a very good scheme and you can estimate how intense this storm is and be reasonably close most of the time, but some of the time you won't be. And it's that "some of the time" that the plane fills in and has an important impact.

There's been a flurry of activity now because of the possible impending loss of reconnaissance planes to be able to justify the decision to drop or continue aircraft reconnaissance. But the question is just how accurate is the satellite in locating and telling how intense a tropical storm is. You see, before, satellite analyists used the recon data to help them in their satellite interpretations so you never really knew how independent the satellite analysis was and when there wasn't aircraft data you didn't have the ground truth to tell how intense the storm really was. I've had a very good graduate student, Joel Martin, working on this subject for a year and a half or so. We've been working with the center fix data in the West Pacific. Typically we have satellite pictures every 4 to 8 hours and then look where the satellite found where the storm center was and look at the differences between aircraft center fixes and satellite center fixes. Well, of course there are differences. There are systematic differences. The aircraft fixes were interpolated to the satellite times so we could see how different they were (Fig. 1). There were no geosynchronous satellite loops involved in this. Although, there probably were some GMS images. A variety of different satellite images were used (DMSP, NOAA, GMS) (Fig. 2). At Miami they have had the satellite looping capability for many years and their results, (Bob Sheets results) and other recent results that they've done show results similar to ours. I'm not saying looping doesn't help out the fixing of a storm in many cases a good deal, but on average when you have a whole group of data where it's been looped and a whole group that has not been looped, the mean differences are not much. This is data for Northwest Pacific for the years 1979-1986.

I just want to go on and show some more things. You see, you get some cases where the satellite and aircraft are different by a large amount. Now for most data, of course, they're reasonably close but it's this occasional one that is pretty far out. The more intense the storm, the closer the agreement in fixes. Of course, you see the eye better and track the intense storms better, but still the mean differences for very intense storms are about 15 nautical miles and you've got

15% of the cases even for the intense storms that are 30 nautical miles (Fig. 3). That's a large difference. Now, Bob Sheets in the Miami group was doing the same sort of study there which gave roughly the same answer.

Now we take another approach to this and we call this SISO measurements of where a tropical storm is. SISO is Simultaneous Independent Satellite Observations. That is, you have two satellite people at the same time looking at the same picture and telling where the storm is. This is showing just how consistent the independent satellite observers find where the tropical storm center is. In other words you would have satellite observers in Clark, Kadena, Guam, Hawaii, etc. independently using the same satellite data at the same time fixing where the storm is. There was a lot of this data. Again, it was done for this period 1979-1986. 54% of these cases were DMSP, 39% NOAA, 5% GMS. There were 2900 of them, 69% in a day, 31% at night (Fig. 4). The intensity of the storms were roughly a normal distribution as shown here. So the question is what sort of differences showed up? Well, there were reasonably large differences. At times independent satellite observers fixed the storm at different places and these differences, even for relatively intense storms, 15% of the time they were off by 30 nautical miles or more. So the more intense it got, the less the difference, but this is the same satellite people independently fixing it. So there is a limit to how well the satellite people can independently fix the centers (Figs. 5-6).

Now an interesting thing was when you ask the question, how large are the differences if you have the aircraft in the storm fixing it at 12 hours prior to the independent satellite fix? When you didn't have aircraft, the differences were 15% or so greater meaning that the aircraft is undoubtedly biasing the satellite fixes. Remember it is not a completely independent satellite position when they have an aircraft telling them where the storm is at a certain place, they tend to shift where they judge the center to be. So that's going on. Now, does it matter much? Yes, it does. Because center fix errors affect the forecast of where the storm goes, because the most important thing to forecast where the tropical storm goes is to know how it is moving now. And to know where it is moving now, you need two good fixes. You've got to know how the storm moves to get a conservative motion vector to extrapolate that into the future, particularly these storms that are impacting the southeast coast and are only 24-36 hours out. It is very important to know where the storm is but also how the storm moves. Now the aircraft helps through multiple aircraft fixes to make a better short-range forecast.

One of the major problems with tropical storms is, the typical track. Now after the fact, when you get all the positions you get an aircraft shows the storm is here, a satellite here, and the satellite fixes jump. The aircraft fixes jump and so on and the typical track of the storm is not always this way, but some of the time you get typical storm wiggling like this. With time what you do after the fact you draw a nice smooth curve through it. This is the "best track". It is smoothed. Now, when you reconstruct these best tracks, when you don't have reconnaissance data, the difference between the working best track and the post-analysis best track is usually roughly twice as much as it is if you have aircraft fixes. When you have multiple aircraft fixes every day, the experienced tropical cyclone forecasters can just tell where that storm is better and its motion better. So there is a problem of degree here. The aircraft does make the situation better. But how much better? The satellite also has this navigation problem. There's a limit to how well the satellite can earth locate where the center is. There's some inherent navigation problem (maybe it's 10 nautical miles or so). You may ask the question why would you have very intense storms and you know where the center is, still the measurements you're 10 nautical miles or so off. This can be attributed to navigation errors.

Now a big point with all this was: "Is this difference due to any particular satellite system?" No, there was no difference. When you stratify by satellite (NOAA, DMSP, GMS, the earlier TIROS), the same sort of differences show up for each. It didn't matter what satellite we used. Also, when you looked at the different satellite systems with time, going back from 1979, there was basically no difference. In other words, things (satellite capabilities) aren't getting better and better. They are about the same and I think this has been true since early 1970. There's been no difference. The satellites did just about as good in the late 1960's, early 1970's as they are doing now and as what I hear, and what people tell me, there is not going to be a basic improvement in the resolution of the satellite system in the near future, (the next two or four years.) You're going to get new things like Bill Smith's talk which help measure the upper-level, the warm core. If they can be interpreted to fix the storm better, I don't know. Probably not. Using the conventional way of doing things, there apparently is no improvement coming up in the next few years. Now maybe new systems will make it better. That's fine, bring the new systems on and let's test them operationally for a year or two and see if they really do as good as the aircraft ... if they can, then, fine, the aircraft goes.

Another surprising thing was it didn't seem to matter whether IR or visual images were used for the fixes. There wasn't that much difference. In other words, we expected the satellite system would do better in the day with the visual or when they use visible and IR pictures but there wasn't that much difference. In other words, you use a superior daytime picture to carry you over at night. One of the big nightmares is, without the planes, of course, a weak system rapidly intensifying near a coastal site where the IR night time pictures are not good enough to find that center and when it's found it's so intense and so close to the coast.

Okay now, that's motion. Intensity. What is that like? Well I've got a bunch of things here. What came out is that Vern Dvorak is a genius at devising ways of telling how intense the storm was. It was such a consistent pattern, in other words, simultaneous independent satellite observations agreed on intensity generally very well. However when there was a plane there, there were bigger differences. In other words, they go in Dvorak numbers. In T-numbers there are certain standard T-number changes and they tend to make those relatively well. Two independent satellite people tend to agree reasonably well on that, however, if there is a plane up it happens that both intensity estimates can be off some. In other words, there was a bigger difference in how the satellite observers told how intense the storm was and when there was an aircraft out there, because they didn't know whether to go with the Dvorak scheme or to use the aircraft observation (Fig. 7).

Another factor that came out was that the aircraft data did not influence the 36-72 hour forecast. So where the center is, how it moves now, and how intense the storm is doesn't help much to improve the 36-72 hour track forecast.

Another type of aircraft reconnaissance flights are called "synoptic tracks". Rather than flying to fix the center position, the flights measure surrounding environmental winds to help in the storm motion forecasts. There have been some synoptic tracks flown, but they probably haven't been utilized as much as they should. They go to the poleward side and measure the subtropical ridge and can tell breaks in that ridge and whether the storm can turn to the right, and recurve through the mean ridge position. You want to measure the basic flow field, typically on the poleward side. How does the satellite measure middle tropospheric winds? Now, one thing Bill Smith brought up, he showed the nice winds they were not using in lower and upper tropospheric levels. That's not the steering ... Those winds may throw you off. It's the middle tropospheric winds or the deep layer mean winds that are important. The water vapor winds, however, might prove useful, but they are not available in the West Pacific. If you got some mean layer steering or middle tropospheric steering, they're helpful, but the low and the upper tropospheric winds themselves do not help us.

You've got to realize that now in the West Pacific without reconnaissance data you don't have any tool to validate your satellite measurements. All these great things are now coming forth. Jim Purdom's shop and Ray Zehr are getting all these nice digitized satellite products from the GMS data, and the VDUC system. We see great potential there from what the satellite can do, but there is no ground truth. So where we are going to work a lot is with data in this narrow window region from 1983-1986 when the aircraft data is there and the digital GMS satellite data is there too. We're working very hard to develop techniques where we can utilize the satellite data independent of the aircraft data but to develop those techniques we need the aircraft data to validate them. You've got to know the outer wind distributions, how intense it really was and all these things that the aircraft measure more directly. But now that opportunity is missed in the West Pacific. We either do it in this period of the 1980's or we've lost all this. The planes are gone.

The big worry now is in the Atlantic where the planes are going to largely go eventually except for the NOAA P3 planes, of which there are only two. It's important that we stress the importance of combined aircraft and satellite research data sets for tropical storms. We're developing different techniques on intensity and things that satellites can see. Will those aircraft go a few places where we'd like to send them? Could we ask the aircraft to go a particular places that will optimize their intercomparison with the satellite? The P3's probably could do that type of thing, but the Air Force planes are more operational, so that would be hard. We'd support them even if they kept doing what they've been doing for 20 years but we're exploring some new territory trying to develop new techniques and so we need perhaps different types of aircraft flights, different levels and things of that kind. The Air Force in the NW Pacific flew very nice center fix patterns like this that went 2.5° radius out. So we've got consistent nice tracks and they flew about two flights every single day for most storms. So you've got a whole pile of standardized tracks there that we want to use, and to me, it's crazy that there's been no money.

The Air Force, the military, NSF, etc. have never really gone in and sponsored research with the flight data until now, with me, the last couple of years. Yet, there's millions of dollars for satellites. Satellites do a lot more, sure, but the aircraft programs cost much less. If you talk to Neil Frank, Bob Sheets, and experienced tropical storm forecasters who year after year work with satellite data and have come to grips with this, they all say we need the aircraft reconnaissance. It's indispensible particularly along the US coast where the population has gone up and all the condominiums have sprung up along the shore lines. It's just crazy that this country that could afford to fly into these storms for 40 some years suddenly can't, when the population is going up, the GNP is going up and so on. One problem is the administrators. It's all or none. Aircraft or no aircraft. They don't think how can we most effectively use the aircraft.

The other problem is there's research and there's operations. The satellite people draw on research results which are beyond what's done operationally using more channels, landmark navigation, very careful navigation, experimental sensors, things like that. They draw conclusions from those. But, those data aren't available to operational forecasters and even if they were, they wouldn't know how to use them. There's generally several years of transition period needed to introduce new data types and new techniques. We thought they might set a five year program out in the Pacific. We're going to cut back two planes every year or three planes every year. It's going to go on this date. You've got five years to develop this program. But that didn't happen.

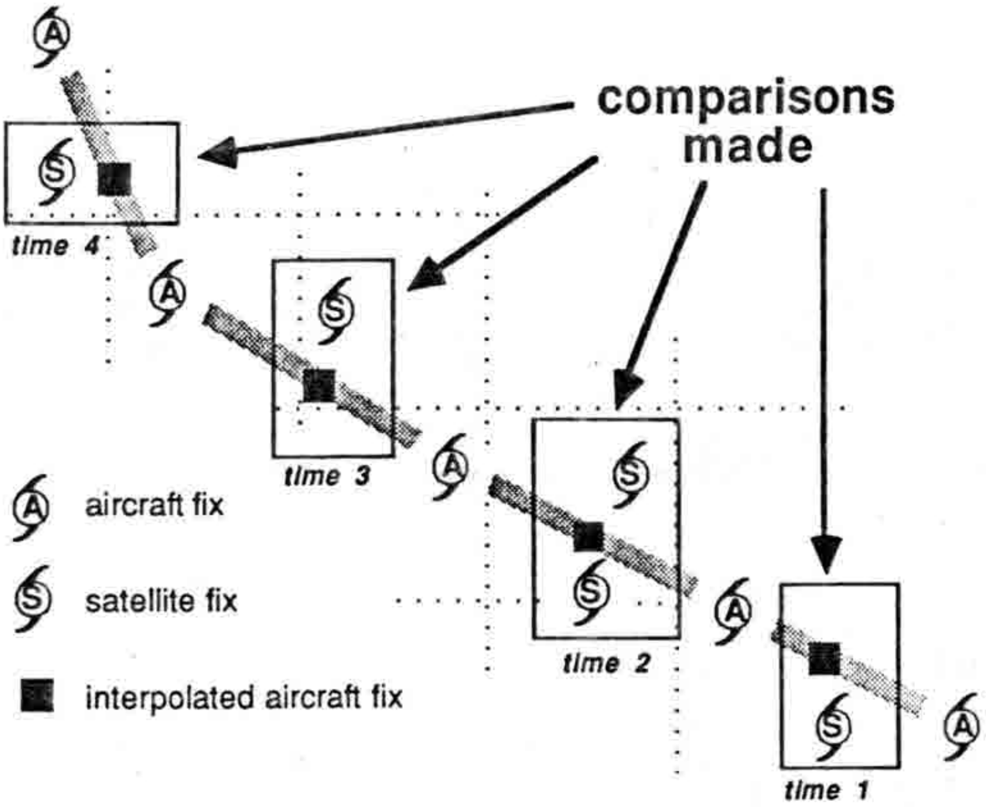

Fig. 1 Typical satellite and aircraft fixes for a tropical cyclone moving towards the northwest. small black squares are aircraft data interpolated to satellite fix times. The aircraft interpolation path is along the thick gray-shade line. Satellite and Aircraft Measurements Comparison (SAMC) data is collected at each satellite time--boxes are drawn around data comparisons. Time 2 illustrates two Simultaneous Independent Satellite Observations (SISO) of a TC fix.

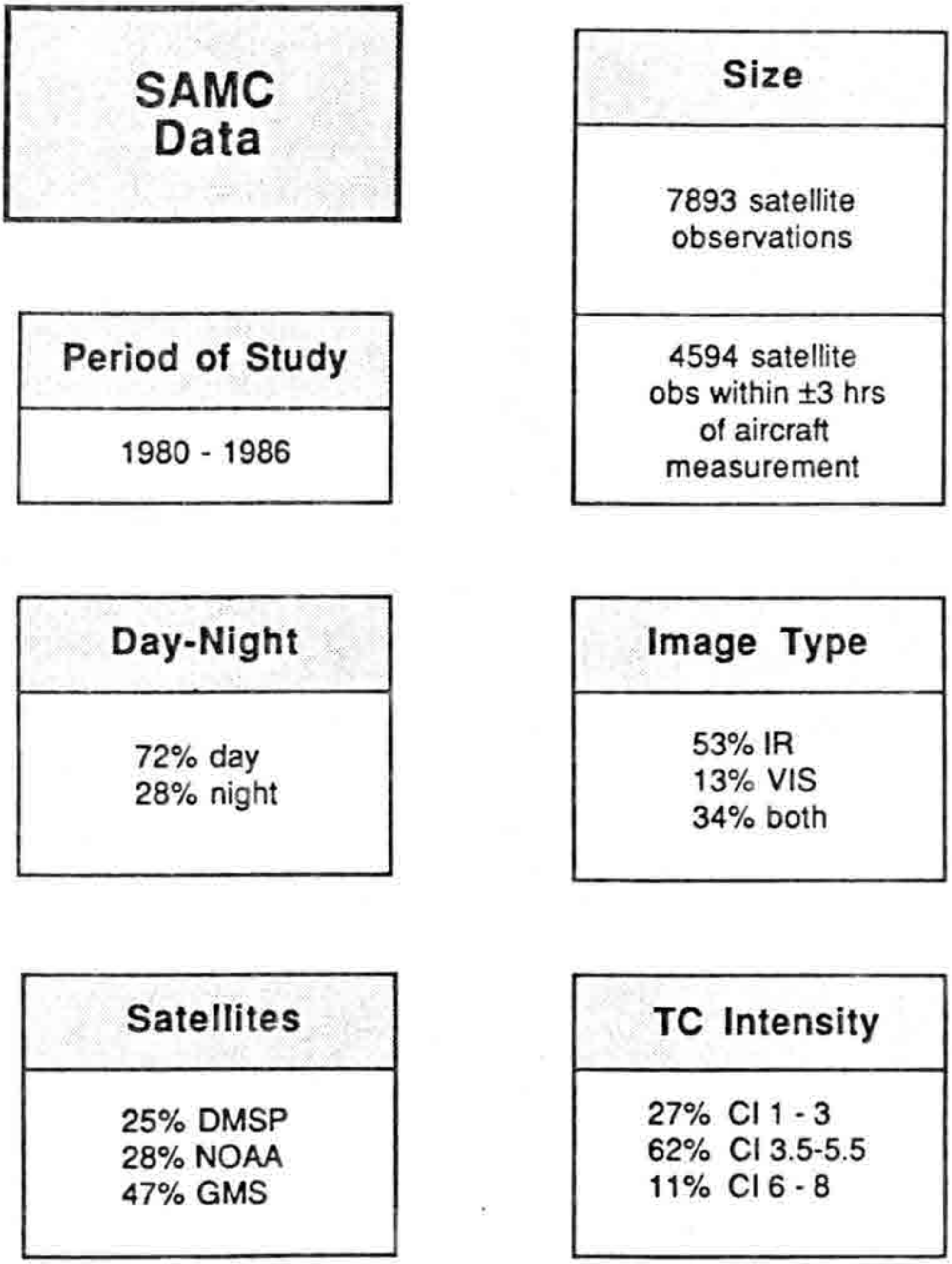

Fig. 2 Characteristics of the Satellite and Aircraft Measurement Comparison (SAMC) data set.

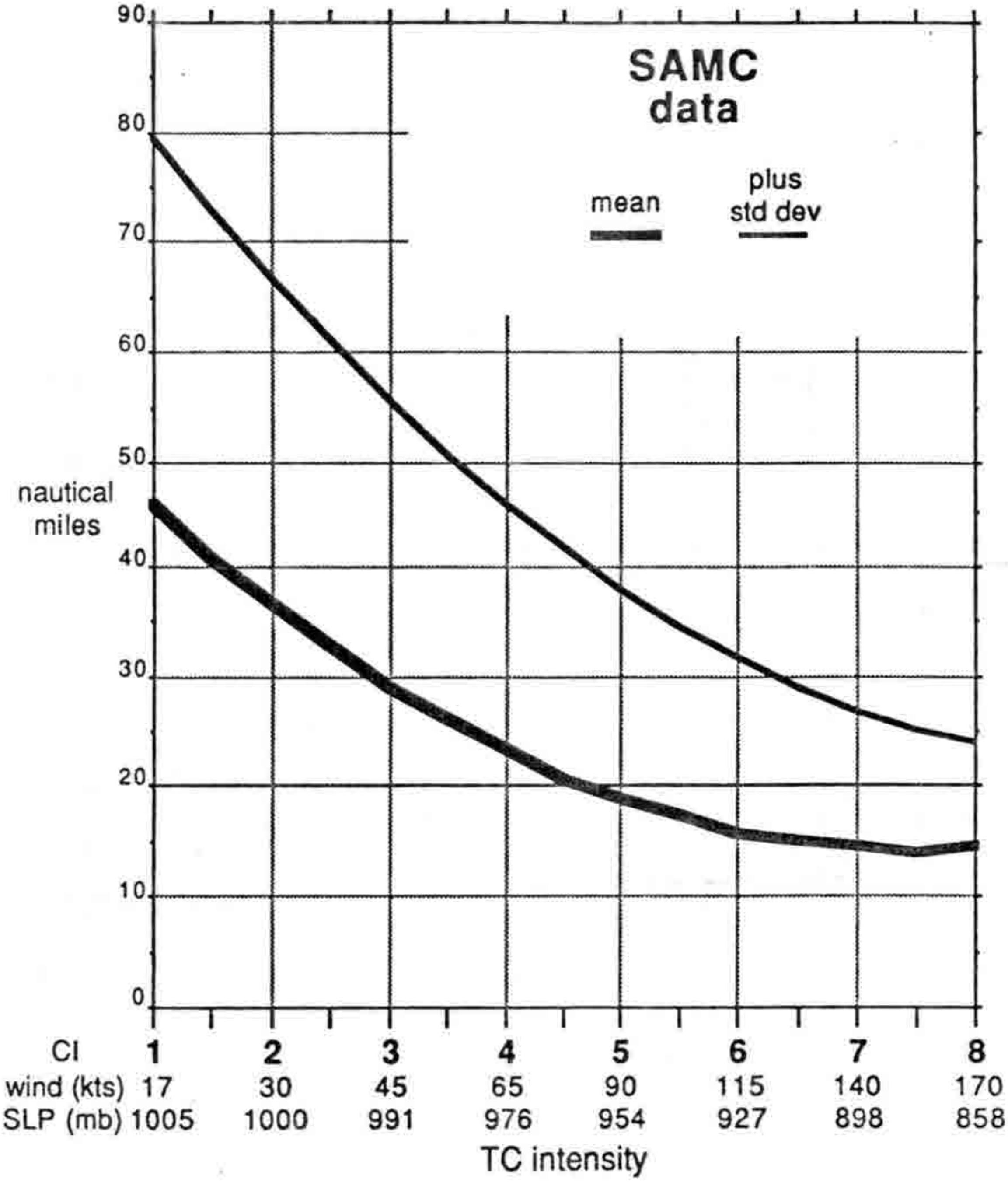

Fig. 3 Mean plus one standard deviation of Satellite and Aircraft Measurement Comparison (SAMC) TC positioning differences as a function of cyclone intensity.

SISO Data

Period of Study
1979 - 1986

Day-Night
69% day 31% night

Satellites
54% DMSP 39% NOAA 5% GMS

Size
2906 SISO events

Image Type
35% IR 13% VIS 52% both

TC Intensity
39% CI 1 - 3 52% CI 3.5-5.5 9% CI 6 - 8

Aircraft fix 12 hrs before SISO
63% of cases

Fig. 4 Characteristics of the Simultaneous Independent Satellite Observations (SISO) data set.

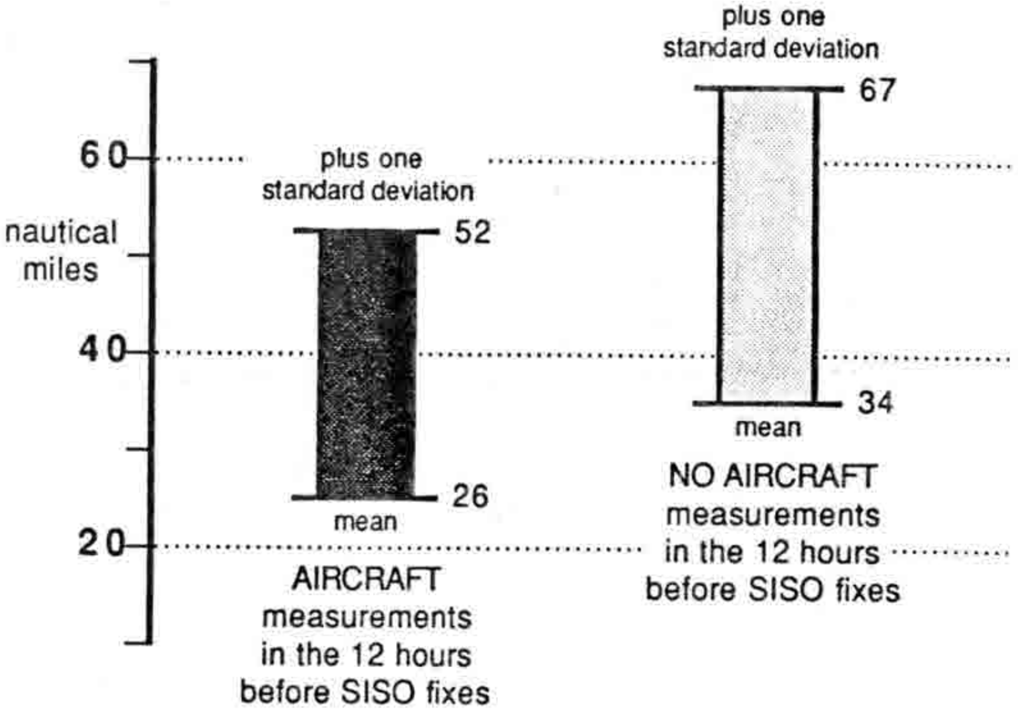

Fig. 5 Comparison of mean and one standard deviation of fix position difference from Simultaneous Independent Satellite Observations (SISO). Diagram on the left shows fix difference when reconnaissance aircraft were present within 12 hours prior to the satellite fix times; diagram on the right gives information when no aircraft were available within 12 hours of fix time.

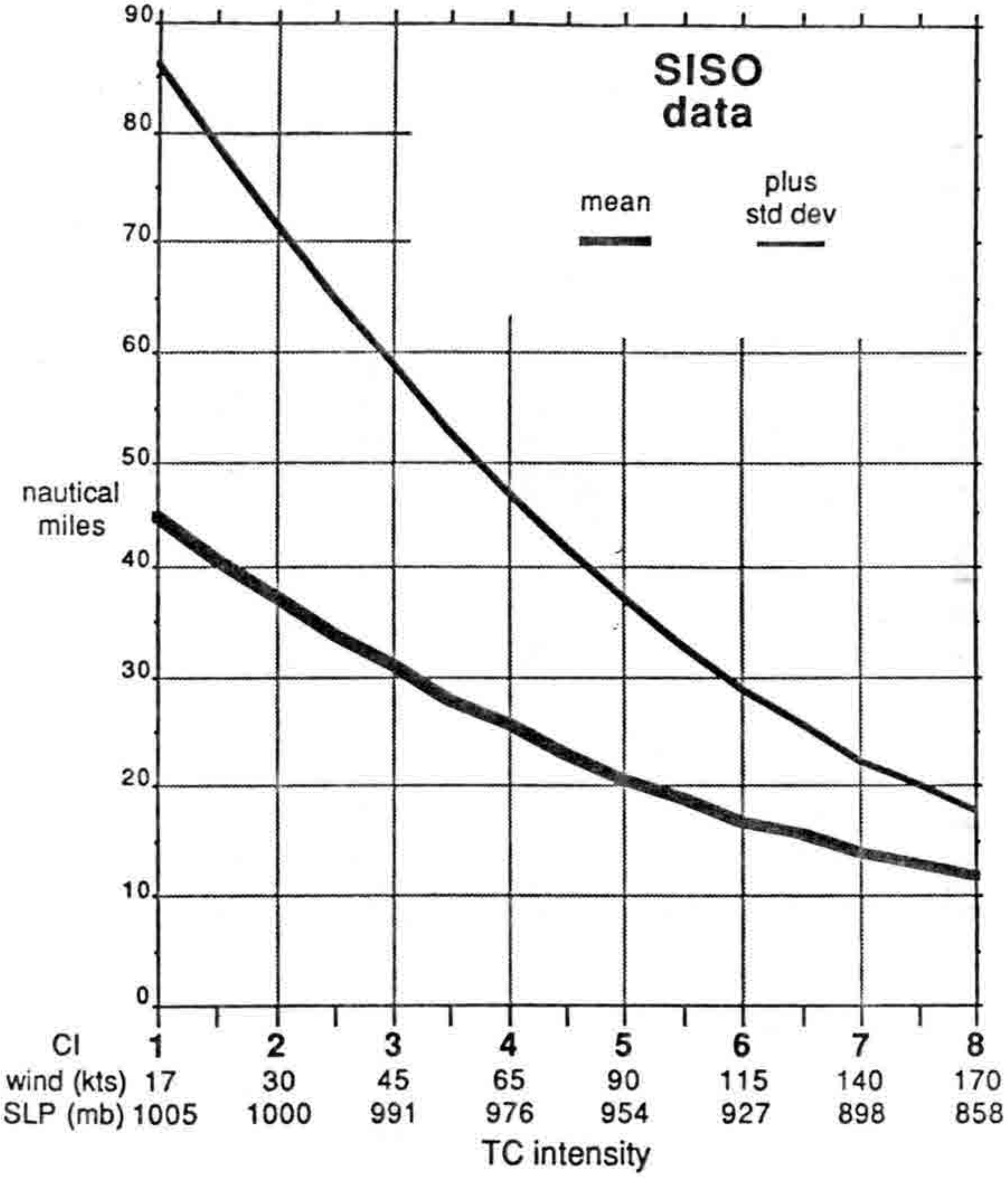

Fig. 6 Mean plus one standard deviation of simultaneous Independent Satellite Observation (SISO) derived differences of TC positioning as a function of cyclone intensity.

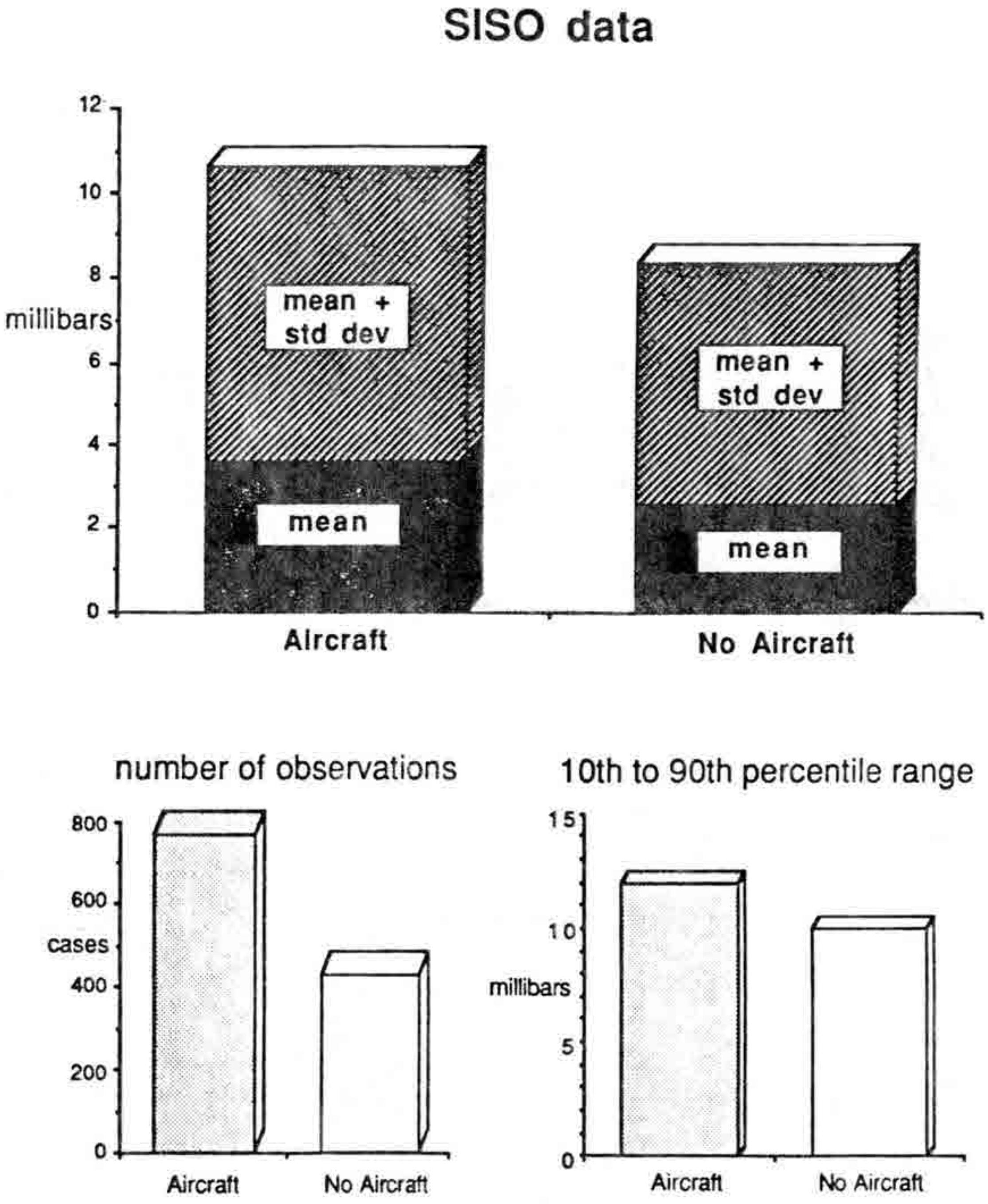

Fig. 7 Comparison of Simultaneous Independent Satellite Observation (SISO) TC intensity differences between cases when aircraft measurements were and were not available in the 12 hours prior to the SISO measurement. Mean and one standard deviation values shown (top) along with the number of cases studied (lower left) and the range in values from the 10th to 90th percentiles (lower right).

Nowcasting

Talk given by Greg Forbes
At the CIRA Satellite Research Workshop
Pingree Park, CO
21 September 1988

I'm not going to do any well focused presentation this evening. Instead, I think I'm just going to bring up a few thoughts I've had recently. One of these concerns the average operational forecaster, out in a National Weather Service Forecast Office somewhere. By in large, access to satellite imagery in any useful way at this level has been pretty primitive. Satellite data, for them, is still generally limited to hard copy pictures. They're not dealing with a lot of interactive graphics and looping capabilities, and so on. I think a real problem is that a lot of what we're seeing on the research level hasn't been put into the hands of the operational forecasters.

We're moving into a totally new era of operational meteorology with AWIPS and NEXRAD approaching, where there's going to be some fundamental changes in the way that weather forecasting is viewed, the way weather forecasts are made, and in the way, perhaps, the products need to be disseminated. So, I think the nowcasting questions to be addressed here should include: "just how are we even going to do nowcasting, or are we going to be able to do nowcasting? Who's the user? Are nowcasts going to be disseminated directly to the public and, if so, in what form?" I suspect it has to be in some type of video format. "Are we going to transmit just a current radar picture, or satellite image, and let the users fend for themselves, or are we going to produce forecasts, beginning from some initial state, defined by satellite or radar imagery? Just where are we going as our technological capabilities improve?" Those are some things I think we ought to be thinking about as we develop new technological capabilities.

What I'd like to do is go back and pick up the theme that we were on after Roger Pielke's talk; where it was suggested that there may indeed be different ways to approach weather forecasting on different time and space scales. I would put forth a premise that when you're talking about time periods less than six hours, you don't have time to initialize models with the larger-scale smooth data, then let the model run to generate mesoscale features, as Roger was suggesting. My way of approaching the short-range forecast would be to work with what you know is already in existence. You would then combine a diagnosis, (that is, recognize what it is you're seeing in satellite imagery or radar imagery), with some rules for forecasting the movement and evolution of those features.

To follow along on that premise, I think we ought to be doing some research that, in the future, can put forecast products directly into the hands of the operational forecasters. This, for example (Fig. 1), is an outflow boundary over Arkansas. We need to ask, "what is going to be the subsequent evolution of that weather system and is there going to be new convection developing along it?." These are very difficult questions, yet this is a phenomena that I think we know a lot about. There are just as many phenomenon, that we know very little about. For example, we can see that there's some organized cloud pattern here in Northern Illinois, but there is really very little in the way of nowcasting knowledge available to forecast how that area will evolve.

What I'm advocating follows from some things that were discussed in England last year at the workshop on Interpretation of Satellite and Radar Imagery. I'm suggesting that some research be done to develop mesoscale forecast rules. These rules might be called conceptual models, but my definition of conceptual model perhaps is a bit more general than others. Not only must you recognize the phenomenon in some way, but then you must have available some idea of how the given phenomenon will evolve. Thus, we need to have done studies for a variety of mesoscale features like we've done in terms of evolution of a thunderstorm. We must be able to describe, in ways a forecaster can use, and incorporate into his thought processes, all of the critical physical processes. An example might be the squall line. We should look at a variety of meteorological fields (such as moisture convergence, etc.) that could be used to make some qualitative, if not purely quantitative, estimate as to whether the system is likely to intensify or weaken. We need to quantify rules for using prognostic and diagnostic fields to predict formation of these types of phenomenon, and yield guidance for forecasting movement and evolution. This figure lists some of these phenomena, and identifies the current knowledge-base for each.

Audience: Greg, where is that figure from?

Answer: Figure 2 is from a summary of the workshop that is in the July 1988 Bulletin of the AMS (Bader, M. J., K. A. Browning, G. S. Forbes, V. J. Oliver, T. W. Schlatter, 1988: Towards improved subjective interpretation of satellite and radar imagery in weather forecasting: Results of a workshop. *Bull. Amer. Meteor. Soc.*, **69**, 764-769.) If you look at that article you'll find a group of meteorologists from the U.S. and a number of operational forecasters from Europe summarized, using an ABC rating scale, how well we thought operational forecasters understood the various aspects of meteorological phenomena on various scales. The consensus of that workshop was that a fair amount needs to be learned. I think we have individuals in the research community who know quite a bit, but a lot of that knowledge needs to be transferred to the operational forecasters. Out of the workshop came a proposal for, and plans to develop, a manual that would include various chapters on the current state-of-the-art knowledge of various aspects of what I've defined here as a conceptual model. For each of the chapters and subsections within, there has been a European and a North American author selected. We're hoping to get a first guess of the current state of knowledge, and what would be useful to develop for forecasting, including those guidance products that you might use along with the imagery. The next workshop will be at the end of July 1989 in Reading, England.

Audience: Are the authors of that manual researchers or operational people?

Answer: It's a mixture. There are a couple of operational forecasters that were eager to participate. They've had a fair amount of research background, some of them with the Satellite Applications Lab. But the problem with operational forecasters is that they don't have the opportunity most of the time to be doing detailed case studies. They've got to worry about making forecasts. We'd like to have all of this manual prepared by operational forecasters, but that doesn't really turn out to be practical.

I'll get a little bit off generalities now, and go more to the specifics. I'd like to show you an example of some of the work that I've been doing with wind profilers that we have operating in Pennsylvania. In this example (Fig. 3), we see a very large comma cloud system, which has some very well defined features. Notice the jet stream marked by an edge-type cliff of cirrus cloud, and the cut-off low, with a pretty well defined vorticity circulation. We can see what's often called a "second comma," the enhanced cloudiness in the PVA area. As this system came across Pennsylvania, there were two very well defined periods of snowfall on order of 1-2" per hour, at two very distinct two-hour periods, with a break in between. These snow periods were from mesoscale features, effectively precipitation bands embedded within a synoptic scale cyclone.

Here is a blow-up of visual imagery at midday (Fig. 4). We find a very sharp cloud edge with this traditional conveyer belt. Note also a few other well-defined edges, layers of cloud coming back and wrapping around the comma. One of the heavy bands of precipitation was just on the east side, under one of these layers. Another of the heavy bands of precipitation was beneath the tail of the second comma. The cyclone was moving and by the next morning it had drifted across Pennsylvania and was out over the Atlantic. It had moved across Pennsylvania in pretty much a steady state. It made a fairly interesting case study in which to use wind profilers. Because of the scale of the interesting features, it was a situation in which we were obligated to think of time/space conversion. This approach works best when you have somewhat of a steady state system.

This is a time-height section during the snowstorm, (Fig. 5). Time increases toward the left,and height increases upward. The data are from one of the profilers in central Pennsylvania. The 10-11 km altitude where the jet stream had been very nearly uniform for a period of probably 12 hours prior to that and was constant in altitude and speed. Suddenly, as that cloud edge approached, the mean jet altitude decreased and high wind speeds plunged down to lower altitudes. If you look in the region near 4-6 km, you can find wind speed changes of 60 m/s in a three hour period, going from the axis of the jet in toward the calmer areas. In Figure 5, one snow period is on the east side of the jet; a second one is where the lighter winds were approaching near the trough axis.

What I have been trying to do for this case, and what we plan to do with other cases, is apply time-space conversion techniques to get companion data sets to go along with features that are seen in satellite imagery. From a profiler standpoint, we don't have sufficient spatial coverage to do mesoscale simultaneous analyses. Time-space conversion requires a system velocity, but the synoptic and mesoscale systems may move differently. To separate the synoptic scale and mesoscale components in the time series, you have to go to appropriate time averaging, to substitute for spatial scale separation. That's what I have done. I have used both rawinsonde and the profiler data to come up with an average over a long period of time. I've made the period long enough such that it would be representative of a length scale that's a synoptic scale, the size of the cyclone itself.

Figure 6 shows a 36 hour mean. I'm going to work with the level where we had the very strongest changes in wind speed, at 4.6 km. I've computed the 36 hour mean, hour-by-hour mean from the profilers, and from nearby rawinsonde stations that collect data at 12-hour intervals.

We can see a disturbance traveling across the profiler during the time series (Fig. 5). I'm going to subtract the individual velocity observations for the mean to come up with a perturbation velocity field; then I'll do a time/space conversion based on the propagation speed and velocity of the cut-off low. We had two profilers operating; one at Crown, PA and one near McAlevy's Fort, PA. Figure 7 shows the analysis corresponding to the visible satellite picture for 1930 UTC (Fig. 4). The observations at the profiler sites earlier have been displaced forward a distance corresponding to $C\Delta t$, and the observations subsequent to 1930 have been moved back. Similarly, I've used the preceding 12 UTC and succeeding 00 UTC rawinsonde observations, to move these observations the appropriate distances, and subtracted out that mean.

Figure 7 illustrates what you are left with. It's the time series version of the perturbation quantities that were associated with that travelling weather system. Low and behold, two southerly or southeasterly mesoscale jets show up very nicely; one associated with the first period of heavy snow, and another one moving toward central Pennsylvania. It will cross the region in a couple of hours. At this point it is centered in Virginia and extends into southwestern PA. Without too much difficulty you can see the general vorticity pattern associated with that comma cloud. If you then add back in the mean wind speed, you come up with what a total wind analysis might have looked like at the 4.6 km level at 1930 UTC (Fig. 8).

Adding the prevailing mean flow yields a main axis of 55 m/s flow across southeastern VA, but also shows one skinny band of 50 m/s speeds coming up right along the edge of the conveyor belt cloud edge into central Pennsylvania. In this case, the weather system was moving fast enough such that the circulation is not as evident in the total wind field. We find faster winds on the south flank, and very light winds on the north flank, but only the relative flow shows the circulation.

Figure 9 shows a similar analysis for the 9.1 km level. The total field shows one jet sort of splitting near Washington, DC. The jet across central PA is probably a new one forming. The snowbursts were right underneath the jet across PA just a little ahead of the cloud edge. Just to the west, at this time, the second snow region hadn't reached central PA. It was situated around Pittsburgh in the PVA area just ahead of the comma cloud center. So this example shows that there are mesoscale jets and convergence zones which the normal rawinsonde network doesn't resolve. Aside from one or two odd winds, the time-space converted winds blended well.

Another type of weather system diagnosis problem identified using profilers is that the numerical models show simple wind-shifts as wave troughs, not only at the low levels, but all the way up through the atmosphere. They're fronts in which all of the wind-shift takes place within an hour or two. The actual vorticities are often about twice as high as those shown on NMC analyses. We use the time/space conversion techniques to improve what you get with rawinsondes. As an example, here is here a cut-off low drifting across Colorado (Fig. 10). This was from Colorado profiler data. The v-component of the wind does not look very wavy, does not look sinusoid, but as the cut-off goes across the region we find that v/r equals a constant. This means that the core has constant vorticity, about twice the magnitude of the maximum vorticity in the NMC analysis. The synoptic analysis, on the other hand, would underestimate the shift, because the rawinsondes are spaced farther apart than the wavelength of the disturbance. It took just 2 hours for this core to drift across.

There are a great many signatures that show up in the wind field that could be related to features in satellite imagery, especially when you consider perturbation winds. We find that virtually every precipitation event that comes across Pennsylvania shows up as a time period of enhanced southerlies, when you compute the perturbation winds with respect to a long term average as in Fig. 11. After 1100 UTC, the perturbation southerlies are no longer extending to the surface. There is still some drizzle. Later, the perturbation southerlies become much shallower, and precipitation stops reaching the ground.

Audience: It sounds to me like you're just giving us short glimpses of the fantastic opportunities that will exist when the wind profiler network is out; that we can expect to find a good strong correlation between what we see in the satellite imagery and what we can diagnose in the wind profiler data.

Answer: Yes, that's all I'm trying to do -- indicate that there are a lot of things that can be done. Upon occasion, you can find the cases where you have companion data to go along with various satellite features. If you can get a few of these cases, you can begin to generalize in a way that says if you see a certain pattern in satellite imagery, it's probably going to mean a certain wind pattern underneath it. Such studies need to be done with satellite imagery combined with NEXRAD and special data collected in field projects. I think we could really make some gains if we used satellite imagery together with these quantified conceptual models. This approach could really improve the prospects for what I believe will be the way we'll have to do the 3-hour forecasts in the future, if we're going to do any more than just simply advect meso-systems. If we're worried about evolution, I think we'll have to go beyond just detection of existing disturbances.

One final point. Someone here asked about cloud physics earlier. The wind profilers that we have ordered are 404 MHz. Except when the rain was intense, our own 404 MHz profiler should get two very definite and distinct spectral peaks that actually indicated both the air and precipitation vertical velocities individually and simultaneously. To see these, we collected the spectral data and not just the output from some algorithm that outputs a single mean wind. We've been hard at work using this data from MIST, or COHMEX, in 1986. We are at the point now where we can deconvolute the spectral information to get microphysical information; including concentration and range of drop sizes, for light to moderate rain cases. So for some of the satellite special applications, it may very well be that you'll be able to make some use of that type of information. The microwave radiometers on satellites and from ground-based profilers that will be measuring at liquid water contents, could be used in conjunction with the profiler data.

Audience: What about clouds? Are you getting any indication of clouds?

Answer: Well, we can't resolve cloud drop sizes, with such small vertical velocity. Their return blends in with the air velocity. If you had a weak air vertical velocity with, say, just one meter per second droplet fall speed, the two speeds would be close enough to where the two spectra curves might overlap. I wouldn't try to separate those. For diameters yielding terminal velocities of two or three meter per second and up, it seems that you should be able to do pretty well.

Vince Oliver: As you probably know, we did three years of nowcasting experiments on Chesapeake Bay. One of our conclusions was that somebody should do the basic research on how are we going to get the data to the public. We can't do it by any voice or text. The public doesn't know location geography well enough for that, not even within 30 km. You have got to figure ways of getting the nowcast to them as an image. Have you thought about that?

Answer: I think that the outlet is already there. If you could demonstrate that you have the capabilities to provide this kind of a forecast in a visual form, it could go out over cable TV. I think the market is there. The weather channel was, early on, thought to have no chance of succeeding, but it has. The number of local cable companies continues to grow. I think the demand would be there, such that if you could provide both a current satellite and radar imagery blown up to an area of a county or two on it, and then some animation and extrapolation to the future, I think that there would be a big market. Most of the cable companies have got a couple of open channels that are used only for announcements and so on. I think that they would probably be delighted to work a useful and interesting nowcast into a portion of those free slots. So, I suspect that's the way to go. There's a lot of logistical problems there in the initial start-up, but it's a start.

Audience: Just what you've described, they have it in England.

Answer: There are a few places around that ...

Audience: Denver Weather Office has a PROFS workstation and they're using it operationally. For the last two convective seasons, they have had quite a bit of success issuing nowcasts relative to major highways. While people may not know what 30 km north is, they certainly know where the intersection of Highway 36 and I 25 are, for example. So, that is one way to approach the problem in voice-only communication.

Answer: Yea, if you've got something that is fairly small and fairly isolated, you can pinpoint those events verbally over NOAA weather radio and traditional radio. I was thinking about a more generic nowcast, where you're putting out a more general forecast. As an example, say, for farmers that are going to be doing haying operations, that would be interested in seeing the precipitation bands coming in from the west before they decide to go out and start cutting the hay. If you're talking more generic situations, you may have to give the public visual displays so that every locality could see what's headed their way.

Audience: Greg, at the beginning of your talk, if I understood you correctly, you were proposing what I would call sort of a recipe or a cookbook approach for forecasters; one having a lot of training elements to it with a notion of utilizing various forms of data available, I believe you mentioned NEXRAD and satellite data, conventional data, Doppler, profilers and so forth. I wonder if you've given any thought to the notion of using some sort of expert system approach to this problem to try to reduce and integrate these data sets and to maybe automate it, too, for the forecaster, maybe create something that is much simpler, but attempts to take into account all these factors. That might be the way to go here.

Answer: There is certainly room for an expert system approach, especially if you look at certain specific applications. If you have a specific site to protect from lightning strikes, or something like that, I think you could tailor that kind of system to work fairly well for general public forecasting. When you have ten or twenty different phenomena that might, in the course of the season, affect the site, it would probably be a little bit more difficult to fully automate it, but we're trying to take one step. If it shows some success and promise then I expect that others probably will take up the ball and maybe try that kind of thing.

Audience: Let me comment on that just a second. Out at CIRA, we have developed an expert system that uses a little personal computer to forecast local thunderstorm activity and severity, as a specific application. Another specific application we have working forecasts downslope winds; another is for snowstorms in the Fort Collins area. These systems are very good for checking out rules, and they can evolve. A person should be able to have certain information input into the system and know at least which things will be his major worry later in the day. I think instead of having to look for everything, I think the expert system can eliminate a few things on this mesoscale time-frame. Then the forecaster could go in and do his own met watch, or short-term forecast, based off his knowledge. What I really like, that I saw you put up there, is the chart out of the AMS Bulletin on the development of conceptual models, and the idea that it was a conceptual model. That it was based on physical principles, where you actually went out and did measurements to make sure what you are telling people is based on physical reality, not some undefined phenomena. I think that's extremely important. That points to the need, again, for validation, making sure you can validate what you're doing. I think that's a thing that we might want to address a little more heavily throughout this meeting.

Audience: I wanted to ask a question of nowcasters. I'll ask Greg, and then maybe John might want to add something. Revised terminal area forecasts are important to me. I'm a pilot and I pay attention to them. So, revised terminal area forecasts are, to me, nowcasts that are being made every day; they're being documented, communicated. The questions I have are: does anybody score them? Are they using satellite data? What's the spectrum of use of satellite data, I mean from none to a lot or whatever? What's happening in that area?

Answer: One comment, more to address the normal mode of terminal forecast updates than to answer the question. I think the main reason the terminal forecast is usually updated is that, the surface observation has come in, and the requirement, specifying below-minimum visibility or below-minimum ceiling has been met. I think that is the normal reason for changing the terminal forecast. I suspect there will be some changes based on radar and satellite as well as new numerical model output that's become available in the near future.

Audience: Does PROFS ... ?

Audience: Yea, aviation forecasting seems to always be the stepsister of meteorology. I know for a fact that they use satellite imagery, at least so far as they can use it now. But, mainly they're interested in cloud bases, not in satellite strength. They're interested in surface winds, and things like that, tracking the cloud system, and developing rules and techniques for diagnosis and analyzing system evolution. You very rarely see a terminal forecast amendment issued prior to the event. That's the problem. It does become, like Greg said, revised observation instead of a forecast. I think that's pretty much the fault of our observing system and in the way data is locally processed within a weather station. As we get these advanced capabilities with satellite imagery, combined with Doppler radar, I think we'll see some major changes. We're already seeing changes like that in Denver, where they are forecasting in an anticipatory way, things like turbulence in and around the airport, onset of low clouds and fog and thunderstorms. They often put out a statement 1 to 2 hours ahead of time, before storms are even developed. This is out of Denver with the PROFS Weather Station which provides a source of mesoscale data and a way to really see the systems and how they are evolving. So, I'm optimistic that once we can get all that data in a displayable form and rapidly out to a forecaster, we're going to see some improvements.

Several brief discussions followed:

1.) Very short-term, airport forecasting problems and hopes for future systems.

2.) Transmitting CRT forecast products to planes and boats.

3.) Radar is the best precip update tool for airports, shuttle launches, etc., (according to Austin?)

4.) Verification of very short-term forecasts.

    a) Models poor
    b) Climatology very bad
    c) Radar good first few minutes, but very bad in only a short time
    d) Need to improve and test iteratively
    e) Best way might be to better understand the physics and improve the tools

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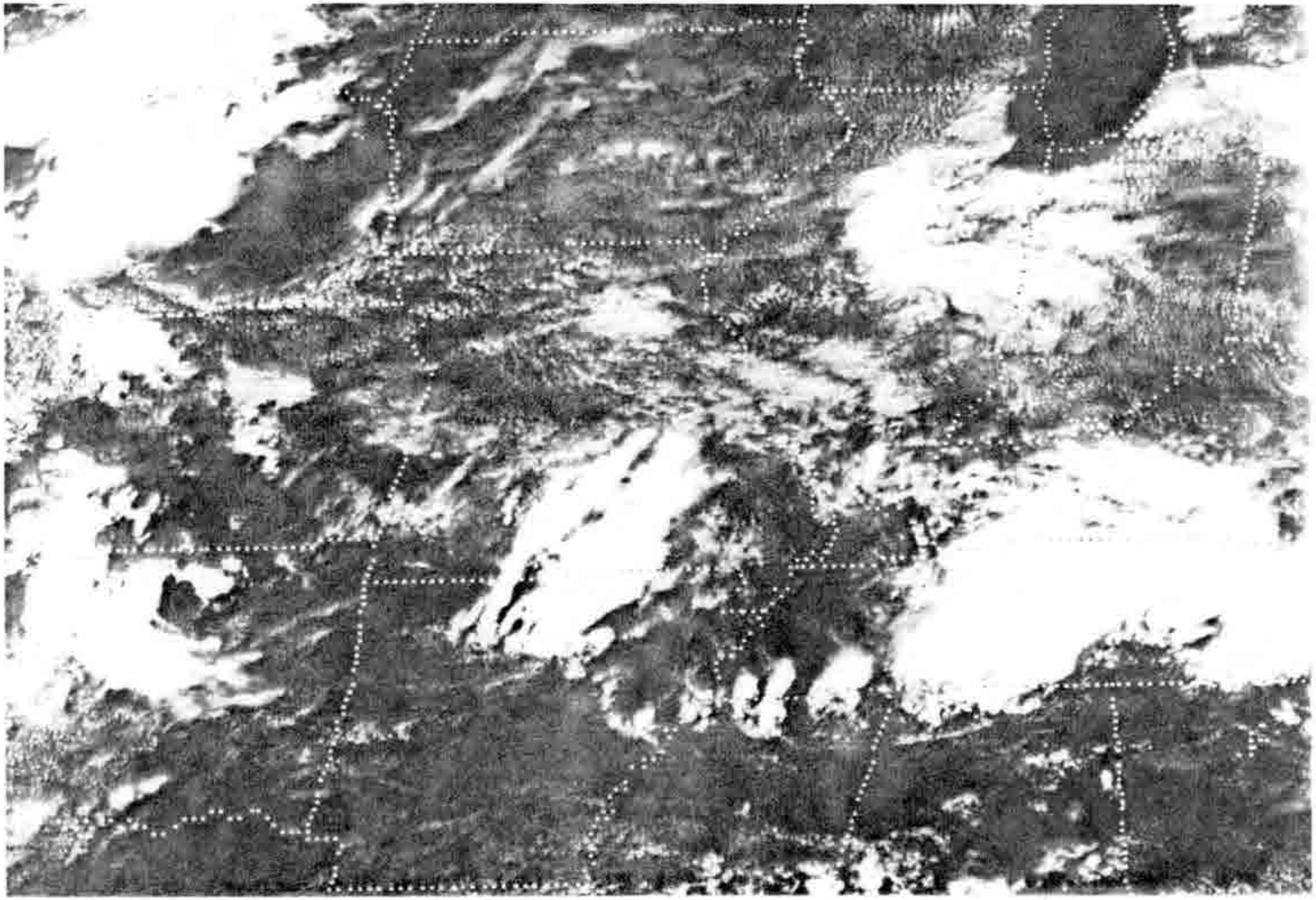

Fig. 1 Visible satellite image from 2100 UTC 16 July 1981, showing numerous mesoscale boundaries.

	Distinguishing features on satellite–radar imagery	Life-cycle description	Understanding of processes	Knowledge of associated fields	Guidance for predicting formation, motion, and evolution	
Conveyor belts	A*	B†	A	B	B	B
Ana cold fronts	A	B	A	B	B	A
Kata cold fronts	A	B	A	B	B	A
Warm fronts	B	B	A	B	B	B
Occluded fronts	B	B	B	B	A	B
Cyclones	A	A	A	A	A	A
Instant occlusions	A	A	A	B	B	B
Upper-level jet streaks	B	B	A	B	A	A
Individual convective storms	A	A	A	A	B	C‡
Squall lines	B	B	C	B	C	C
Large mesoscale convective systems (MCS)	B	A	C	B	C	C
Cold-air vortices (baroclinic and convective types)	B	B	B	B	B	B
Mesoscale rainbands	B	B	B	B	C	B
Shoreline phenomena	B	B	A	B	B	C

* A: Useful information available; few problems

† B: Some useful information; remaining problems

‡ C: Little useful information; many problems

Fig. 2 The status of conceptual models of atmospheric phenomena.

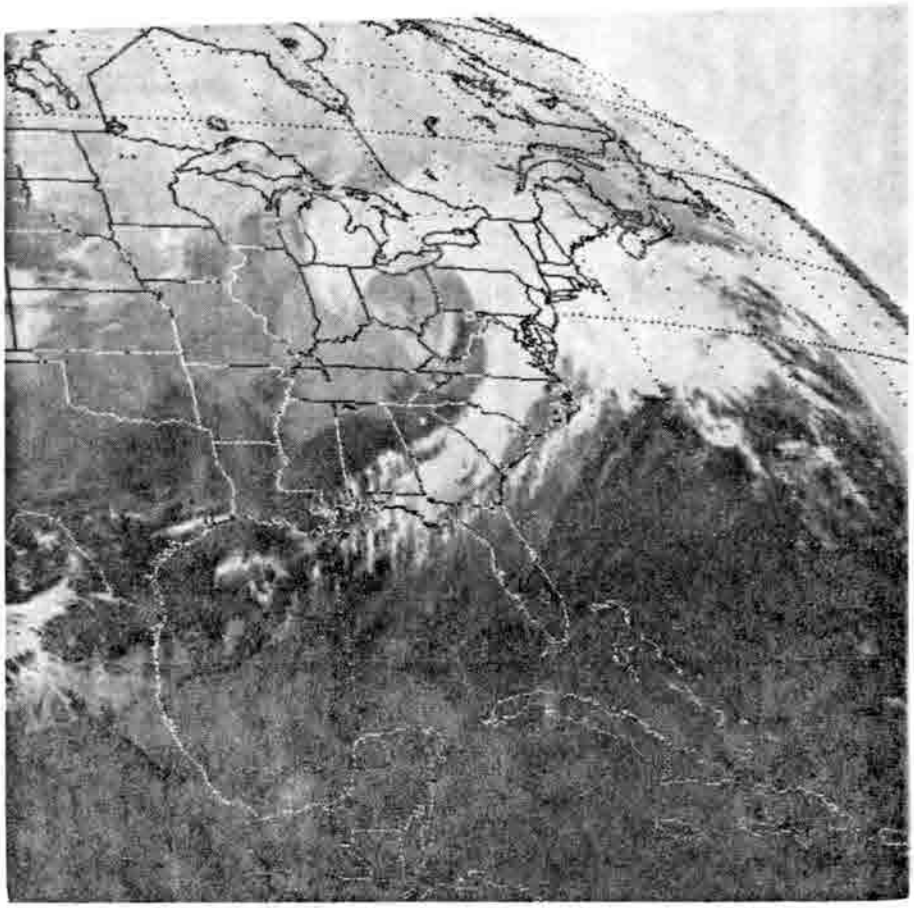

Fig. 3 Unenhanced infrared satellite image of the comma cloud of 19 January 1987, 1901 UTC.

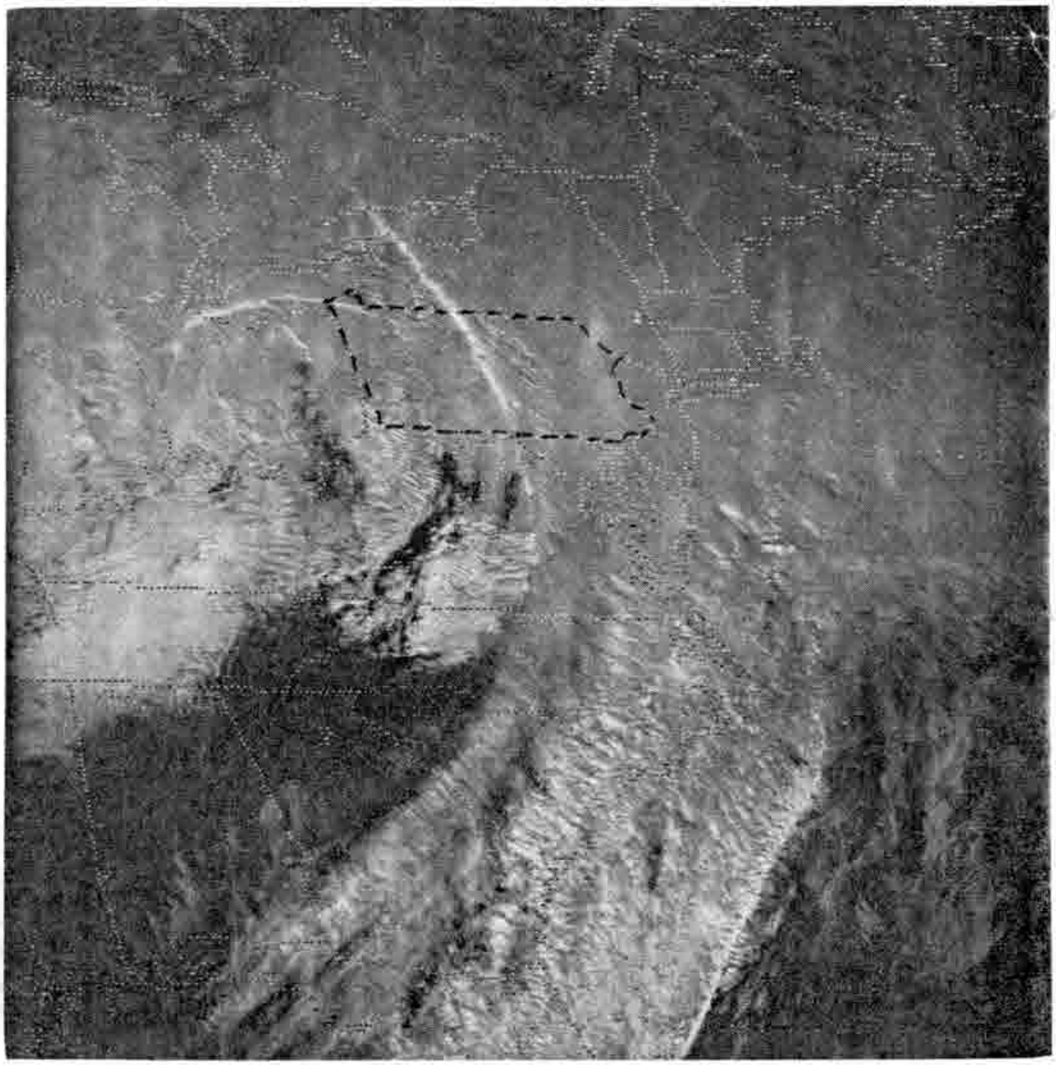

Fig. 4 Visible satellite image of the comma cloud of 19 January 1987, 1931 UTC.

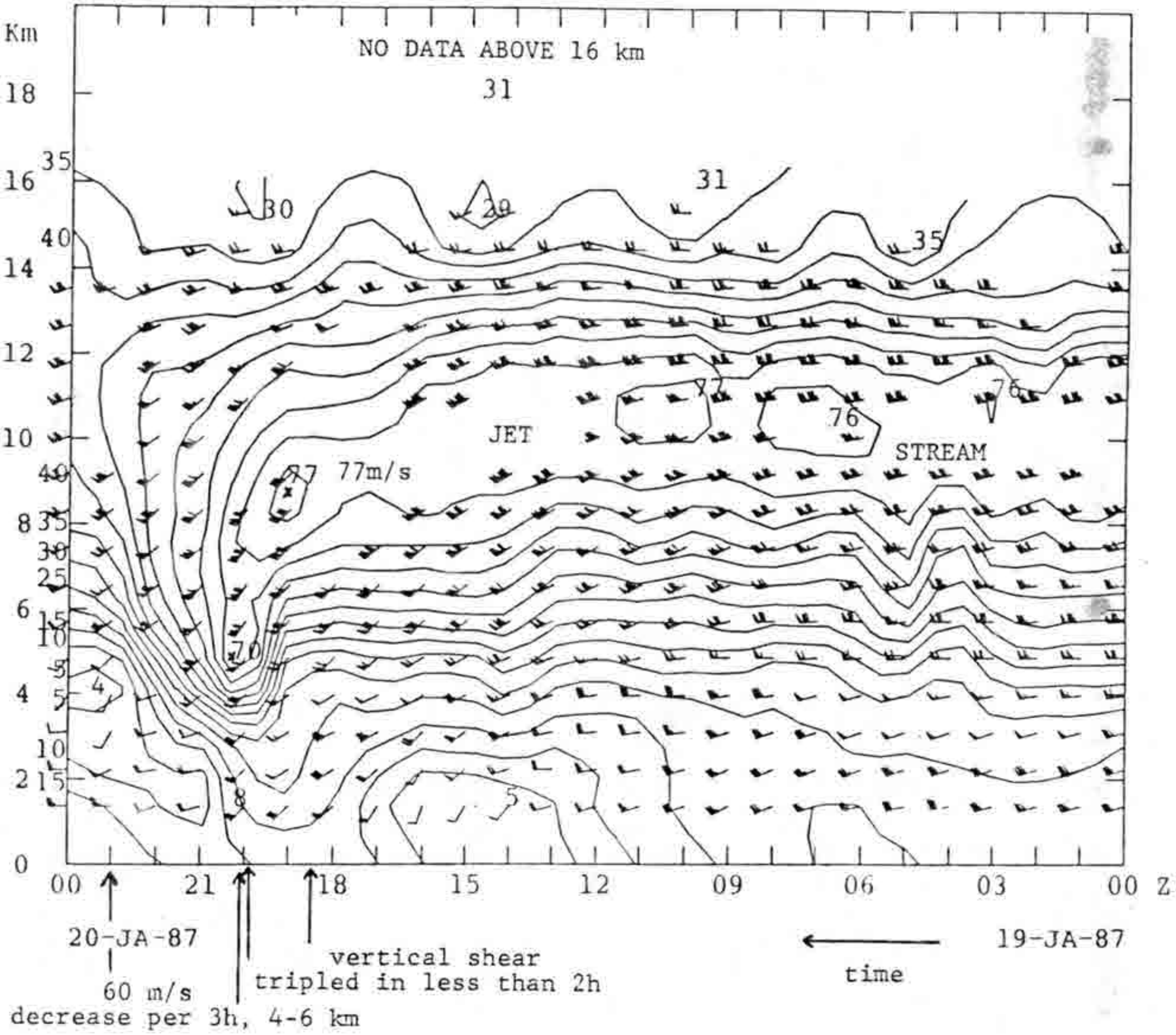

Fig. 5 Time-height section of winds from the McAlevy's Fort, PA wind profiler on 19-20 January 1987. Isopleths are wind speed in m/s. Wind barbs show wind direction and speed; a barb represents 5 m/s; a pennant represents 25 m/s.

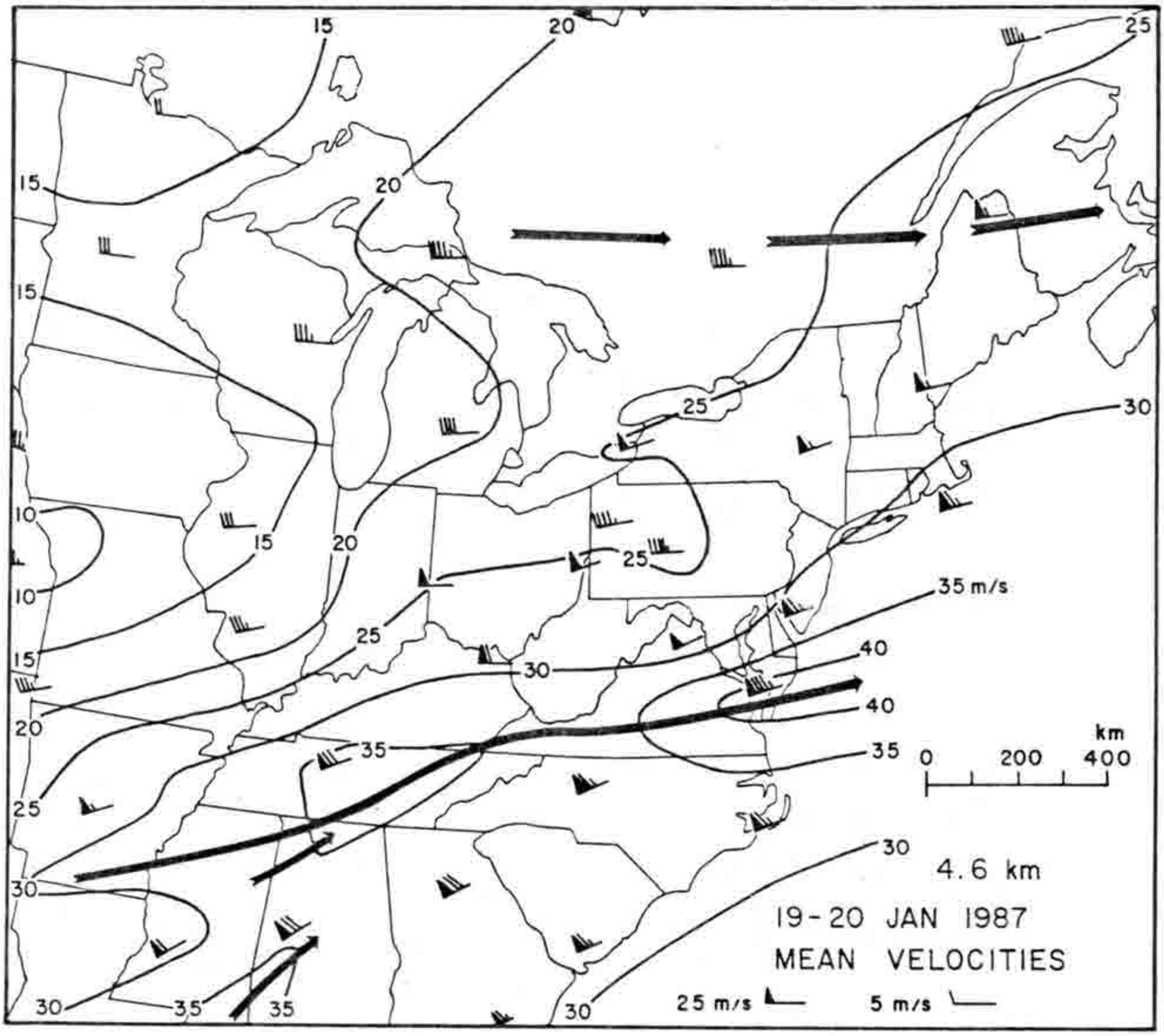

Fig. 6 36-hour mean velocity at 4.6 km on 19-20 January 1987. All winds are from rawinsondes except for the two sites in Pennsylvania showing speeds of about 22.5 m/s, which are from VHF (about 50 MHz) Doppler radar wind profilers.

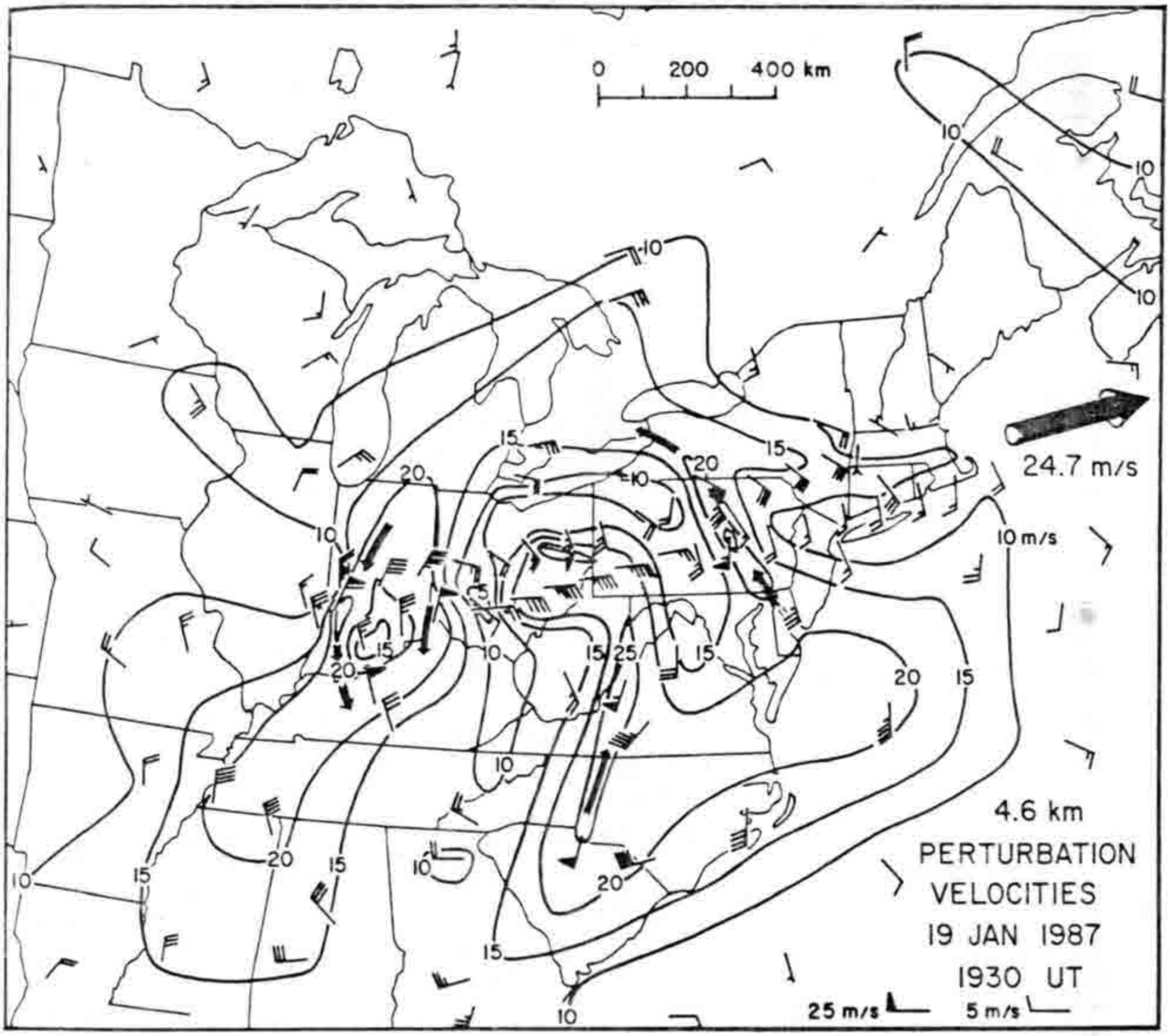

Fig. 7 Time-space converted mapping of the perturbation velocities at 4.6 km, positioned relative to the comma cloud at 1930 UTC. Five belts of wind maxima are evident: in east-central Pennsylvania; across central South and North Carolina, western Virginia, and into eastern West virginia; in eastern Ohio; in southwestern Indiana; and near the Ohio-Indiana-Kentucky border. Each contains a perturbation wind speed of about 25 m/s or greater.

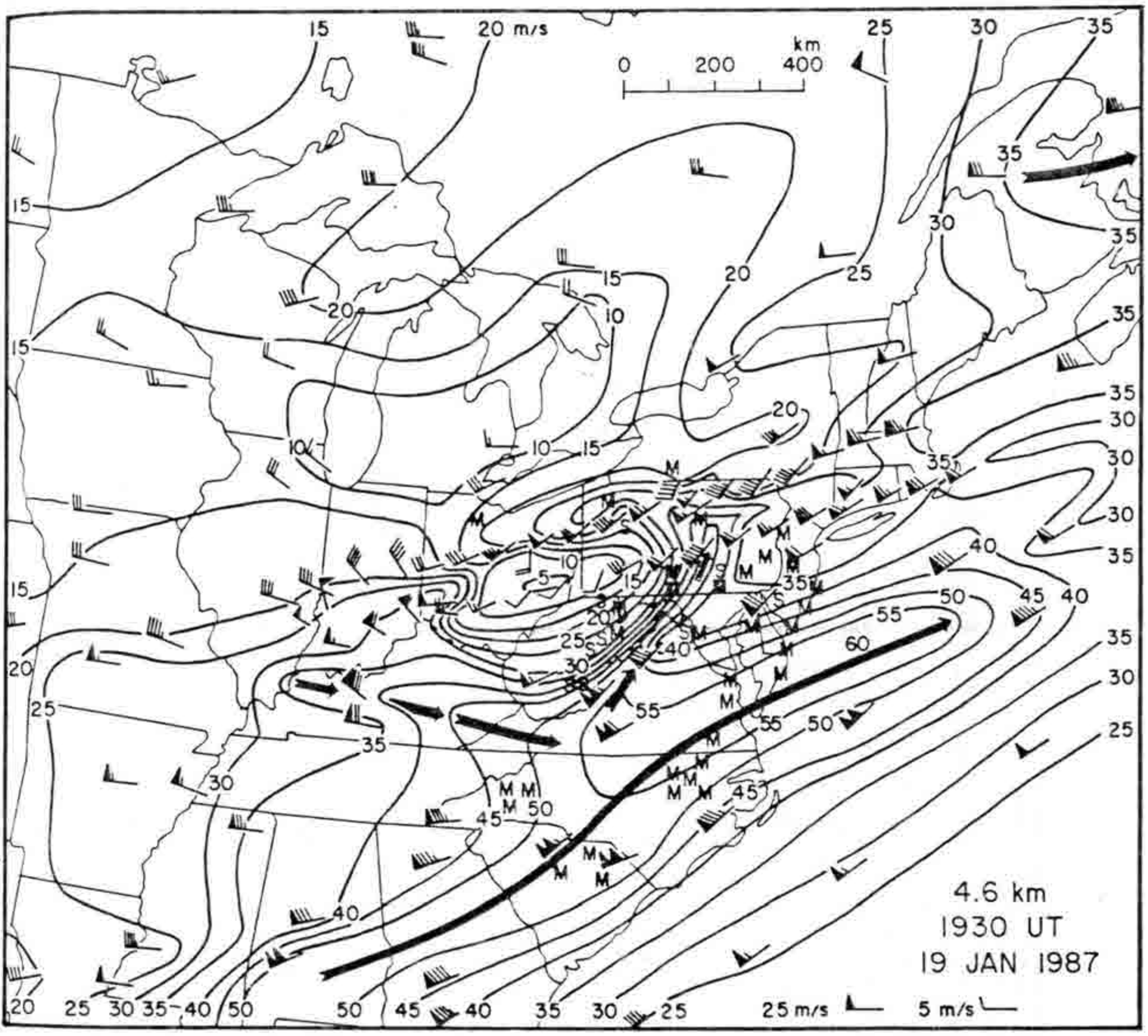

Fig. 8 Total time-space converted wind velocity field at 4.6 km pertaining to 1930 UTC on 19 January 1987.

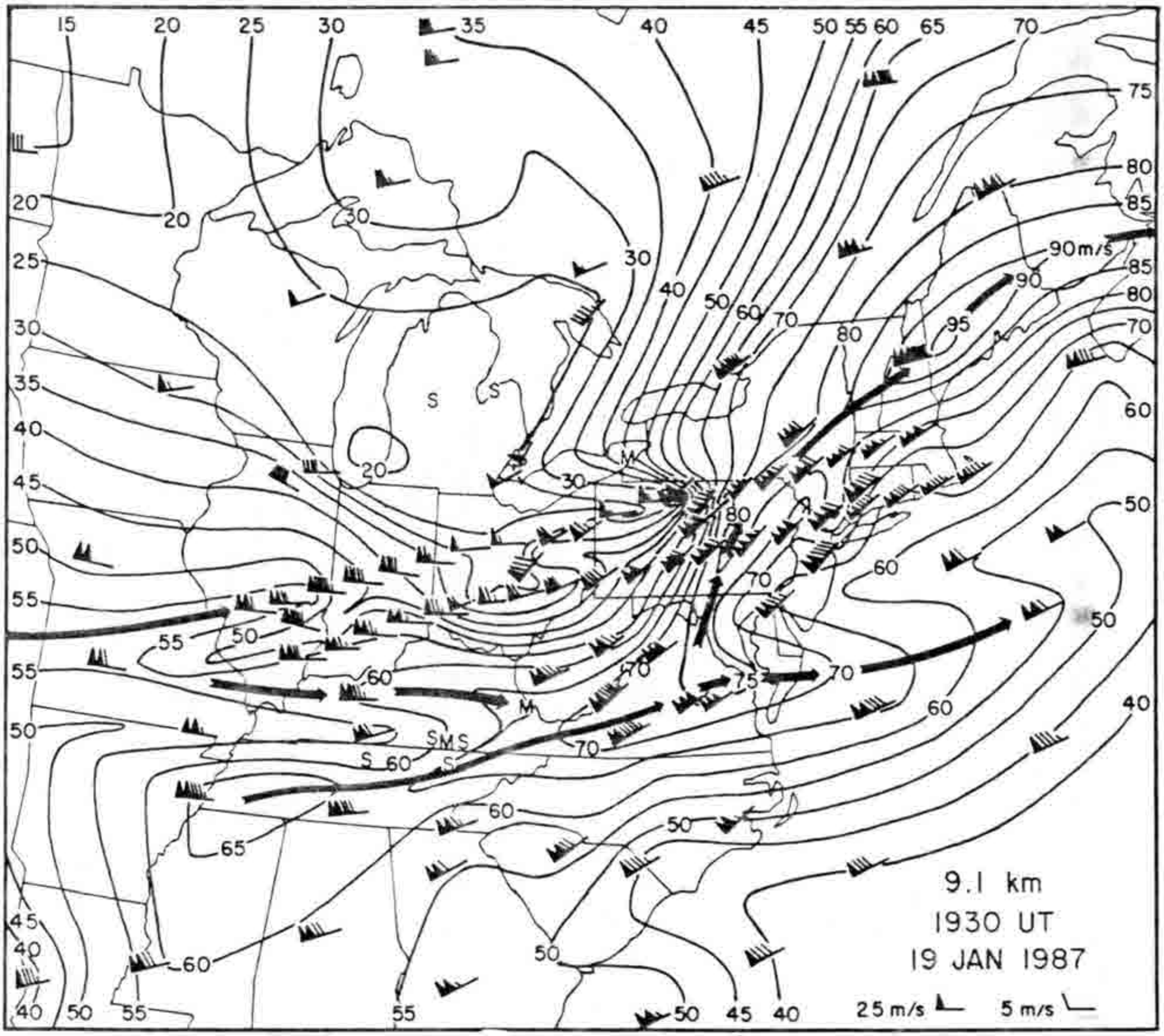

Fig. 9 Total time-space converted wind velocity field at 9.1 km pertaining to 1930 UTC on 19 January 1987.

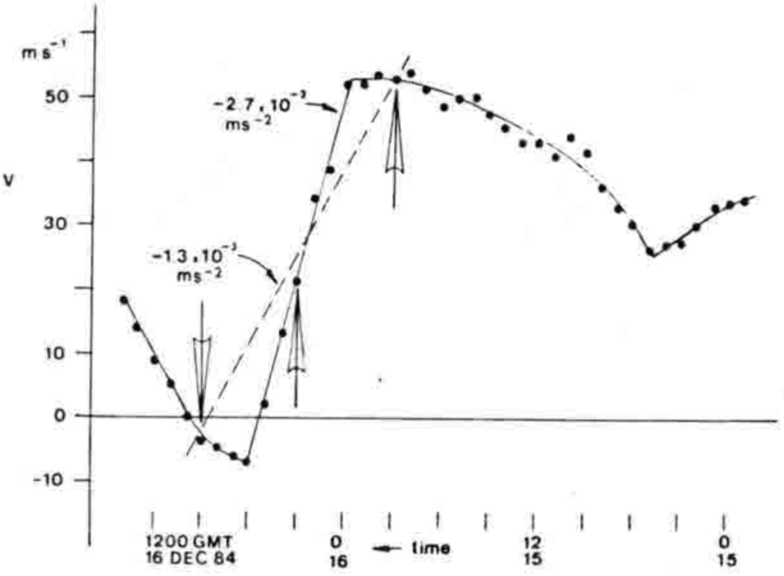

Fig. 10 Time series of the v (north-south) component of the wind at 9 km MSL above Fleming, Colorado on 15-16 December 1984. Dots indicate individual hourly observations and the solid line represents a smoothed portrayal of the evolution, showing a tendency of -2.7 x 10^{-3} ms^{-2}. The dashed line represents the tendency that would be deduced from the worst possible location of radiosondes (arrows) relative to the 300 mb trough -- see text for details.

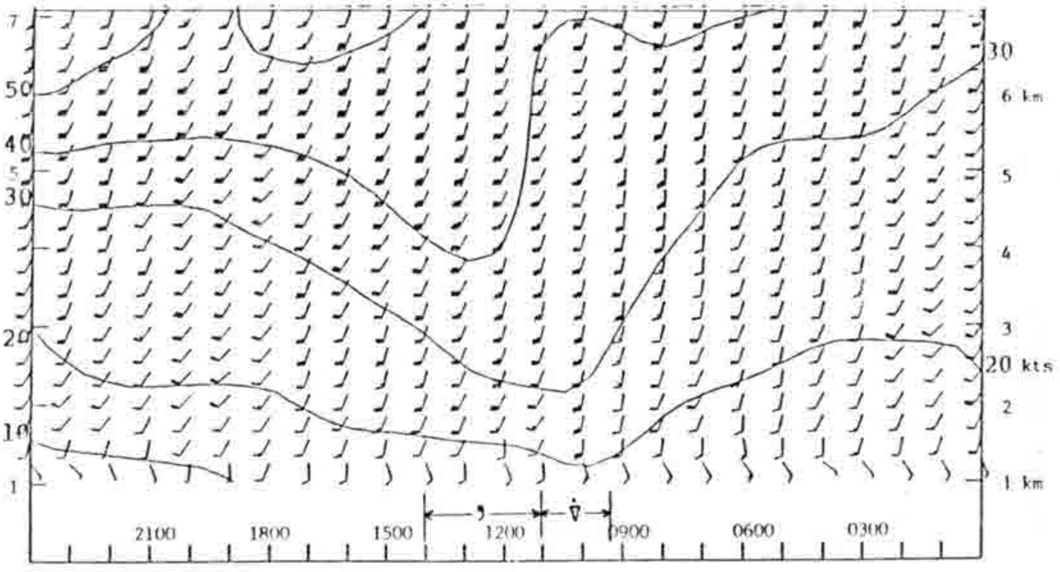

Fig. 11a Subtle upper-air disturbance associated with precipitation event on 20 May 1986.

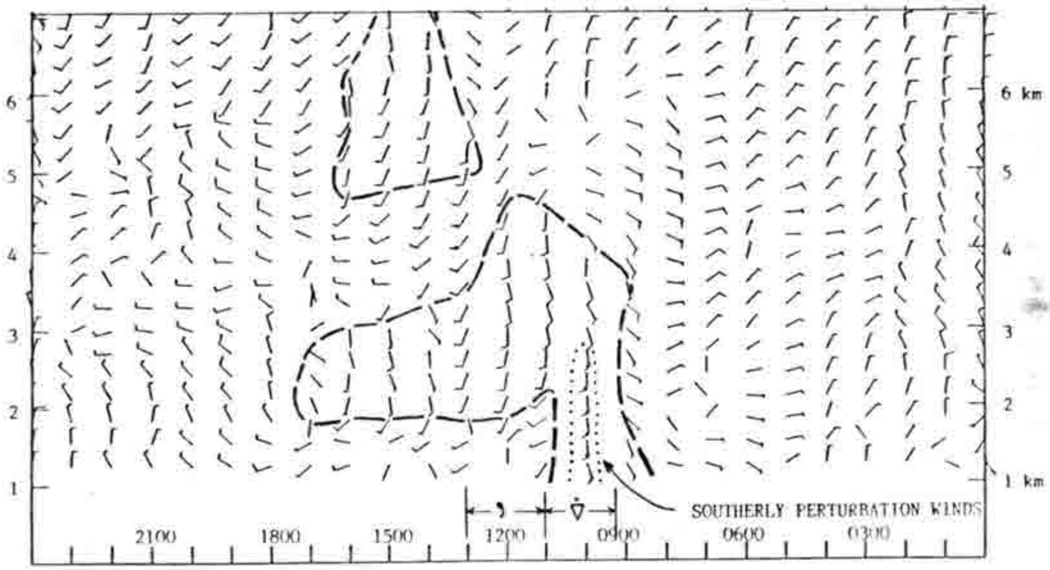

Fig. 11b Perturbation winds, obtained by subtracting the 24-hour mean wind from each hourly wind, indicate that the 20 May 1986 precipitation event was associated with 8 kt southerly perturbation winds below 4 km. Wind barbs are doubled (relative to normal plots).

Large Scale

Talk given by Thomas H. Vonder Haar
At the CIRA Satellite Research Workshop
Pingree Park, CO
22 September 1988

You are guests of Colorado State University. The Vice President for Research, Jud Harper, provides funds for CIRA to have such a workshop every year. As such you avoid some problems. Those of you with the federal government might be worried about accepting gratuities from potential contractors. These funds are not federal funds, they are State of Colorado or endowment funds. Insofar as we cover some of the expenses or all the expenses here, we don't need to be concerned about that. If you have any concerns we can always provide a letter for those of you who are federal employees. You are invited here as a scientist, not to represent your institution. We're happy to have you tell us what's going on at your institution, but this is just a group of scientists and some graduate students exchanging information. That's part of the University's mission, as all major research universities, to sponsor such open scientific discussions.

I want to talk today about climate. You've seen the outline. All I can do is introduce some topics, as I'm not prepared to talk about all of them. I've brought material for the smaller group sessions. I want to introduce Dr. Garrett Campbell, my colleague who is involved in a lot of this work. He'll be here while I can't stay as long as I'd like. While I mention a few of these things, like aerosols, ozone; Phil Durkee and Nancy Cobb or Chandrakant Bhumralkar, I would be happy if you would chime in.

The World Climate Program is about 10 years old. The part that we work with in international arena is the World Climate Research Program. That's jointly sponsored by the World Meteorological Organization and the International Council of Scientific Unions, which is a large group of scientists from all countries. The Climate Research Program has some very specific projects underway. We won't talk about all of them. You've heard about the ocean related projects, the World Ocean Circulation Experiments (WOCE), and the Tropical Ocean Global Atmosphere (TOGA) experiments. We'll get into some of them. There are other parts of the World Climate Program that I don't know very much about. Then in the U.S. we have a National Climate Program mandated by Congress. It's almost 10 years old. There's the National Climate Program Office. NOAA is the lead agency, participation from a number of groups. So, there is a plan and program for climate research in the United States. I'm extracting things from both the national and the international plans. We'll talk a little bit about the studies of variability of clouds and other aerosols; we'll touch briefly on greenhouse gases and biogeochemical cycles; we'll talk about some measurements from our satellites that give us records of the energy exchange between earth and space, and poleward energy transports which are under study. The whole climate program right now focuses largely on the interannual variability of short term climate. There is quite a bit of work on paleo-climate and other things. In a recent issue of *Science* magazine you'll see some results by Prof. Kutzbach and others on simulations of longer term climates, but most of the research in the World Climate Program is focused on the shorter term, involving ocean-atmosphere interactions and interannual variability. So, it blends in with long-range forecasting. But the idea is to understand the physical basis for climate change. Why is one winter different from the next? Why is one growing season different than the next? Obviously, there is enough to keep many scientists busy for their lifetime. We'll go on and talk a little about these topics. We'll see how the new satellite data from the GOES and the NOAA might help us approach some of these topics. DERF, the Dynamic Extended Range Forecasting; that's the way we talk about long-range forecasting today and climate modelling. Then the rising star of the climate spectrum, back to basics again, let's understand the hydrological cycle. Vern Suomi believes that the hydrological cycle is a missing link in our understanding. It's an energy short circuit. Vern told me that he couldn't come to the meeting, but he talked to Bill Smith about what he would say if he were here. Vern said he's studied a lot of planets, besides Earth. He said Earth gets more energy than the other planets because of our albedo and where we are relative to the Sun. We collect more energy than the other planets, but the kinetic energy in our atmosphere is less than many of the other planets. It is because the hydrological cycle is providing an energy short circuit that carries energy and captures energy. Vern said that in the next 5 or 10 years we ought to focus very hard on the hydrological cycle. The operational NOAA satellites will probably play a bigger role than we imagine in this better understanding of the hydrological cycle.

First, the clouds. The reason we study clouds is because in the shorter term we know that there is a feedback. We don't know how strong it is. We don't know if it's always positive. But the circulations cause clouds, and clouds influence the radiation, radiation changes the circulation, changes the clouds. There is a loop that is not well understood, and as we try to understand it using climate models the first thing you have to do is predict the clouds, generate clouds with a climate models, realistic ones. That's not easy when you're not carrying liquid water as an explicit variable, which most of them don't, although some of them are starting to do. There are empirical studies in relations of the clouds to certain aspects of climate. There is a need to know how many clouds are on this planet and how they vary. We don't know that. We know the albedo of this planet and its variability in terms of how many watts/m^2 are reflected back to space. We know that better than we know how many clouds are on the planet. The average between 0% and 100% is 50%, and that's what we see in our textbooks. Right, 50%? Then, we go out as an observer, you see a lot of days with no clouds and a lot of days with a 100% clouds and the average is 50%. Does that average really mean anything physically to us? No.

The International Climatology Project is an ongoing project of the World Climate Research Program, started in 1983. The idea was to use the in-place satellite network, including the two GOES. You might say the legacy of FGGE, to collect and file data in a systematic way, to use the NOAA satellite to fly under all the other satellites as an inter-calibration satellite (kind of a moving standard), and to obtain information about the variability of clouds, would help us do a number of things, including check climate models. Obviously the GOES-Next satellites will make a contribution to this ongoing program. The output grid, is roughly 250 x 250 km, every three hours, 365 days of the year for 7 years. It goes until 1990 and therefore would not get into the GOES-Next period. But we expect the cloud climatology results will be exciting enough, they're starting to come out now and they will continue, in which case it will be into the GOES-Next era.

Jim Purdom: Tom, how can we really talk about a climatology program for clouds when we only have 7 years of data? Climate seems to be a long-term thing. These 7 years of data, may be only one part of the sine curve somewhere and we don't really know what true data set you have. It seems incredible.

Tom: Well, it's just that nations make decisions for finite periods of time, and the scientists do expect that it will continue. But like any other program it's done in increments. Like any experiment or program, it has some data gaps, all the more reason to continue it.

I'll give you some very brief examples of some results. Here is a busy chart for 7:00 a.m. This was prepared by Dr. Campbell (Fig. 1). Most of you will recognize a swatch of data taken along the west coast of the U.S. What we're showing here is the diurnal variation of total cloud amount. The clock diagrams show the amplitude and phase of diurnal variation in terms of local time. A vector like this one shows that the maximum of the diurnal variation is around 1500 local time somewhere around Oklahoma. The amplitude is 15%. We're picking up, the diurnal variation of clouds, not only over the places where we know something about, but out over the stratocumulus regions and in the other portions of the world too. With the data every three hours we're learning about the diurnal variation of cloud amount. This is the vector which signifies the peak amplitude of the diurnal variation. The isolines are the amount of cloud. In regions where you have very large amounts of clouds you find this system detects cloud at all hours of the day with smaller diurnal variations. This particular system we've talked about before, where you have an imager that provides visible and infrared data in the day and infrared the rest of the time. You have an option to use the infrared estimate of cloud or to give a consistent 24 hour picture. The ISCCP output archive gives you a big data set to work with for research. We'll describe that in a smaller group. It provides you with a wealth of information. ISCCP doesn't just put out the cloud amount of this box. It gives you 128 parameters including the statistics of the cloud. We're only pointing out a very small piece of it, and that's the qualifying statement.

This is available on a global basis, except for a few regions like over India. We have NOAA satellite data but not as much INSAT data as we'd like. Now, given output parameters like this, you the scientist, are free to slice them up any way you want. In this particular case Bill Rossow at Goddard Institute for Space Studies looked at all the clouds that have come out of July 1983 that have cloud top pressures less than 400 mb and cloud optical depth less than 8. What he's done is make a decision, that this is cirrus. He chose anything with pressures lower than 400 mb. We use the visible light data and a radiative transfer algorithm to take the reflected radiance, make an assumption about the absorption in the cloud, to obtain the optical depth. Someone else might say I want to just look at the clouds above 300 mb instead of 400 mb, then this picture is going to change. So, what he's picking up are thinner clouds. ISCCP, being a satellite oriented program, always sees the first layer of clouds. As you start looking for low clouds you have to take that into consideration. It turns out cirrus is one of the more difficult clouds to detect. One of the things we are looking forward to having are improved cirrus detection systems with the new satellites. The 3.7 μm channel on the present AVHRR and VAS is a powerful channel. But because it's not on all the satellites around the world, we have to use the 11 μm and the 0.5 μm channels because they are on everything. As new satellites come along I see no reason why we won't use more. For example, Driedonks if you're planning future satellite experiments in Europe and you're really interested in cirrus, a special channel to help get cirrus is certainly a candidate.

Bill Smith: If you look at the CO_2 slicing method you see a lot more extensive cirrus than you see with the 11 μm IR and visible. There is a lot of semi-transparent cirrus that places the altitude of the cloud much lower than 400 mb and probably doesn't get into Rossow's climatology. The 3.7 μm and an 11 μm would tell you whether cirrus exists or not, but it won't tell you the altitude. It is quite critical that the CO_2 channel information be used in future cloud climatology to define the altitude of the cirrus.

Tom: One of the other things that is going on in the cloud business, is regional experiments. Along with this global climatology there are special regional experiments in Europe, the U.S. and Japan designed to measure a volume of atmosphere with a lot of special detectors to understand the ways which clouds are formed, so models can be told how to parameterize them. Also, to estimate the radiative effects and to develop new ways of detecting them. Bill Smith and his group participated in one of the experiments all over the United States in October 1986. Bill would you talk about this a little bit, just give an example of what's going on in some of the regional experiments that I mentioned, that are the underpinnings for this global project.

Tom: This big global experiment is not flying blind, it's got these special intensive periods underneath it that help us understand the satellite data, and give us the kind of ground truth experiments that Jim mentioned that we might want to concoct for other purposes. We talked yesterday about maybe flying more into certain parts of hurricanes. The FIRE Experiment, has in the U.S. about 40 scientists involved. FIRE, which is the First ISCCP Regional Experiment, so it's a second order acronym, looks at small volumes of the atmosphere. It gets information about vertical motion and large scale horizontal motions, so we can understand formation and dissipation in clouds. That helps the climate models. When they produce clouds or radiation from those models, we compare that with what is measured on a large scale by experiments like the cloud climatology project or Earth Radiation Budget. At the same time these experiments, like one that began in Europe called ICE, the International Cirrus Experiment, give us ground truth. So, we have a pretty good closed scientific plan.

I would like to mention aerosols. Looking at some INSAT (Fig. 2), we noticed lower tropospheric aerosol blowing off of Iran over the sea. We talked yesterday about GOES-Next and its increased capabilities to see more things, to detect more things. INSAT in a sense is a little brother to GOES-Next. It is three-axis stabilized and has similar radiometers with improved radiometric sensitivity. You really do see things that we probably wouldn't see from GOES present. One of the things that we're going to see more is lower tropospheric aerosol. Now, Phil, you've looked at this from a number of perspectives. Do you want to say a couple of words about that?

Phil Durkee: We've been doing some work on using the polar orbiters for aerosol detection. Our first result that we've developed this summer from our aerosol retrieval, April 1982, days after the El Chichon eruption, in the southern Mexico. You can see the plume. The end of March was the time of the eruption. We can also see actual Gobi Desert dust. We also look at two channels on the AVHRR; we can look at the red visible and we can look at the infrared. We can see information about the size of the particles, essentially the slope of the size distribution. We think this is Gobi Desert dust because it follows the trough pattern. We can also see Sahara dust and plumes coming out of North Africa, and the Gobi Desert region.

We're certainly able to measure this with AVHRR and then with GOES-Next. Some of these low albedo depths, which are important for climate if they're persistent. If they're persistent they can become very important for climate. The improved radiometric sensitivity is really going to be helpful, both for cirrus and for aerosols, but not cloud aerosols. There is a product on that list that Bill mentioned to us, that AVHRR list. That is one of the things that will come out on the NOAA-K, L, M series if not already on a test basis.

Tom: Global biogeochemical cycles, you all have been hearing and reading a lot about that. The key parts that I think are important for us right now, aside from the hydrological cycle which we'll talk about a little bit later, are the land/surface processes that involve some of these cycles and some of the ocean biological measurements that we can take from space if we have color imagers. Most of you don't need to be reminded that CO_2 is taken up by the oceans and there's a respiration-decomposition from the ocean. Part of the carbon cycle, a big part of it, is the ocean, not only the biomass but also the ocean mass. There are a lot of gases that are known to change the greenhouse. Here you see a summary chart by Ramanathan, a number of years ago (these numbers have all changed). The point is that stratospheric aerosols, clouds, as well as CO_2, methane, and other greenhouse gases like ozone, will change the surface temperature according to the scenarios that have run presently in these imperfect climate models. Climate models are always improving, so you should expect numbers to change as time goes on, as the models get better. In terms of spectral radiance, you see that the chloro-fluoro-methanes and other gases like SO_2, methane, nitrous oxide do have discreet spectral signatures. So it is possible to develop techniques from satellites to measure greenhouse gases directly. Well, there is another approach. We know CO_2 is increasing. The so-called CO_2 fingerprint search; like Sherlock Holmes looking for a fingerprint. You know the murderer has been in the house but you don't have any evidence. So, the search for the CO_2 fingerprint is to detect what increasing CO_2 has done. Some people are looking for that fingerprint in the surface temperature records, but other ways to look for it are in the radiance to space, coming in certain spectral regions. Either as a result of changing temperatures and/or gas concentrations. So, I want to point out that in any spectral measurements that we can make, even in the lower resolution ones made by NIMBUS back in the late 60's, we do resolve the ozone and some of the gases, although it's a complicated problem. There have been papers published trying to pull out some of these special signatures. We are capable of putting interferometers and spectrometers in space and we also see it as a way to get better information about greenhouse gases.

I would like to mention something that one of my students put together many years ago, I brushed through the file and pulled it out the other day. It is a composite, a latitude spectral diagram, a composite for a month back in 1969. This gives us a picture as a function of latitude of the very gross spectral emission from our planet. If you look in regions where you know there are features like ozone, in the polar regions for example, we see an increased emission due to presence of ozone in the atmosphere. In other places of course it's a reduction in the outgoing radiation. Just as ozone would have this, so would some of the other species. I pulled this out because back in 1969, this is before the ozone depletion in the polar regions. I really would like to have measurements now to see whether these numbers have changed after we noticed ozone depletion. So, in a sense we might have something from the old satellite data that will help us understand, not just the CO_2 fingerprint, but the ozone fingerprint as well.

One of the things that we are measuring from our present NOAA 9-10 satellites is the outgoing longwave radiation from space measured with a number of experiments. We have long time records. This happens to be from the NIMBUS satellite which is being continued now with the NOAA satellites and a special satellite for that purpose (Fig. 3). Garrett put together this summary, looking at the outgoing radiation over regions near the equator off the west coast of South America. There seems to be a recurring variation here in this region in outgoing radiation to space, which is one part of the climate heat budget equation. During the ENSO the reduction in outgoing radiation was remarkable and certainly far above the signal-to-noise of the system. This portion of our planet lost more energy to space. We've all heard about that because the longwave radiation is used by the climate analysis people as an index of convective activity and high clouds and many other things. What happened to the planet as a whole during this time period? Garrett looked at the same data from NIMBUS, but he looked at the entire globe, all the latitudes and all the longitudes. There is an annual cycle of energy that goes out to space, mostly due to the continentality effect. The whole planet loses more energy to space in the northern hemisphere summer, but in the ENSO period you see almost twice the amount of radiation going out to space. So the ENSO event which is a regional feature, puts a pulse on the whole system. If you were on Mars or Jupiter watching Earth you would see that spike. We don't know whether that's important or not, but it turns out because the type of clouds that are important are cirrus type clouds and stratus type clouds, which accompany some of these big perturbations, that it's a nonreciprocal effect, that the solar part of the equation doesn't compensate the infrared and so there is a finite pulse of watts/m^2 put on the planet.

Let me just talk a little bit about the climate modeling area and give you a few topics that we need in order to cover this thing a little bit, some big names that are coming along, some of them obviously related to the El Nino Southern Oscillation. When we have our next ENSO, the question we have to ask ourselves with this new capability of satellite data is: what are we going to do with the GOES-Next or the NOAA satellites? How are we going to use the new data to study the ENSO better? Recently the people at GFDL, Kirk Brian and others, were running one of the very best climate models today, which has an ocean-coupled atmosphere system, that's the Manabe-Brian Group, found that their climate model occasionally jumps to another state. In other words if started from the same initial conditions the model will occasionally come to equilibrium at a state familiar to us today, and sometimes at another state. There's a couple of papers out, one of them by Bjerknes (1964), who discovered that in the North Atlantic, in the historical records of sea surface temperature, there is an occasional occurrence of a circulation pattern that involves a stronger than normal atmospheric circulation along the gulf stream boundary, which in turn is related to a change in the magnitude of cold water coming from the north. It turns out that there may be something in addition to El Nio. El Nino is one of the biggest regional signals. There may be now a North Atlantic anomaly which has to be important. It should certainly be of interest to Europe. If the climate can jump to another state and come to equilibrium there (the period there is something like 15 years in the second state) that would be a very exciting thing to study. So these kind of things go together.

In use of satellite data to verify models, the models have to be putting out precipitation and clouds. We expect that all the climate models in the next few years will have the capability (as the computing power comes along) to carry, liquid water as a variable. Therefore you have clouds in them at all times. We'll be helping verify climate models with cloud data. Precipitation (QPF) is a very important goal for the forecasters. They're definitely going to keep trying to improve that. As a result the research that climate people want to do on global energy, water transport, particularly latent heat and precipitation variability, should fit nicely with the developing climate models. As we talk about precip, for all the other reasons of flash floods and things of that kind, don't forget that it's a very key variable in terms of climate. As we talk about water vapor for forecasting severe weather and things of that kind, don't forget that may be the missing link in understanding some of the climate variability that we get on an interannual basis. The Climate research fits neatly with a lot of the other research, but it isn't going to happen if we don't explicitly make it happen. As people develop techniques we should keep an eye towards what we can contribute to the climate side of the house as we work on some of the other problems. That's the message I'd like to leave.

Jim Purdom: One of the things I'd like for us to be sure to address when we meet in our groups is really a serious issue (although you might not think it is), how do we prepare for the occurrence of special events using the satellite data, so that, for example, if an ENSO is coming up, what do we want to do with the new satellites as it's occurring or as its forecast; what should we do? What special observations should we take? How might we combine these with other programs? We might well think about such things as hurricanes. If you have an intense hurricane let's not wait until we learn it's the most intense in the Western Hemisphere and has already destroyed Cozumel and most of Jamaica before we decide to take some stereo on it, after it's out over the other part of the Gulf of Mexico and starting to decrease. I'd like for us in these working groups to somehow come out with a statement. Let's be prepared better to study these types of special events and to make sure we have the data sets available to do it.

I understand that NOAA is about to drop its obligation to the scientific community and the world by not saving GOES-Next data, but at three hour intervals. I can't honestly believe we'd let that occur. People in NOAA that are responsible for that ought to be lined up against a wall and shot. (Laugh, laugh) We ought to think of the scientists; people are throwing away data. We're not sure how to use it, but we have some pretty good ideas. To me it's absolutely incredible. I think we need to address that issue. I know Fran Holt knows about that and Bill Shenk knows some of it. I don't want to just see special events data, we have to save all the data. One of the things I can see coming out from our discussions is the need for validation experiments and special things to note what the data is really showing. In fact I have the secretaries making a copy of Greg Forbes slides so that we can pass them through the group. He talked about the five or six ways to look at nowcasting and the development of physical models. That's really a guide for a lot of what you might want to talk about, that will go with the saving of the special data, but we need to think about saving the routine data for other studies that might be done. We don't know all the answers yet; we might come up with a few more after we have the data a little while.

I know this is early in the morning so it's probably good to take a break right now for about 10 or 15 minutes. We are pushing late into the time of the working groups. It's important that we continue the discussion on these specific items when we go to the working groups. Let's come back about 8:15 promptly, and then we'll have the next three talks probably about 10:00.

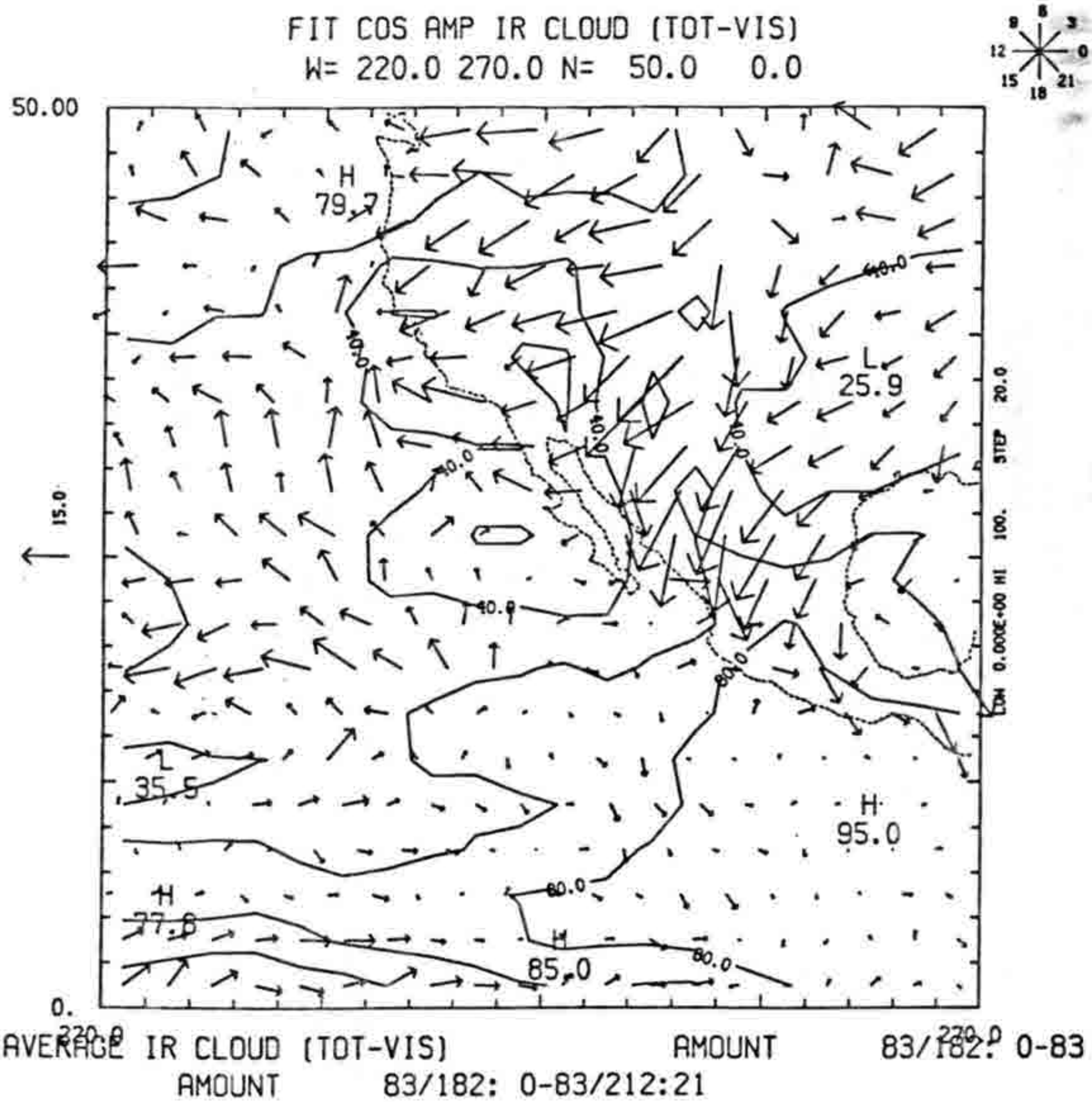

Fig. 1 Diurnal variation of total cloud amount minus visible only cloud amount, July 1983. The arrows show amplitude and phase of the diurnal cycle. This limited region over western North America shows the near noon cloud maxima over the mountains. Then east of the Rocky Mountains the maxima shifts to later in the day. This is well confirmed by the local climatology.

Fig. 2 INSAT visible image of blowing dust as seen moving southward across the Persian Gulf.

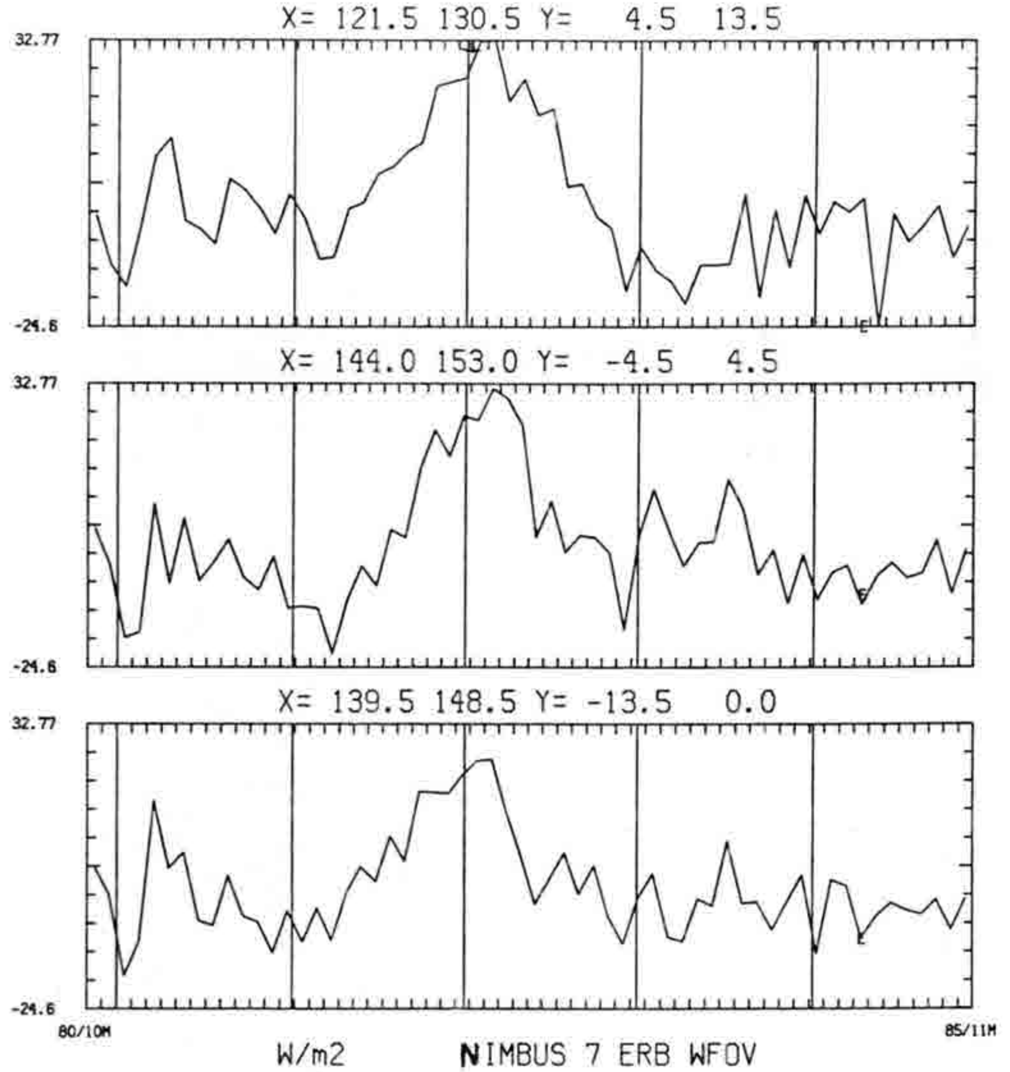

Fig. 3 Time series of Nimbus 7 wide field of view longwave radiation anomalies from the monthly means show the effect of El Nino in 1982.

The Polar Low
An Overview

by
Talk given by Erik A. Rasmussen
University of Copenhagen
At the CIRA Satellite Research Workshop
Pingree Park, CO
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In a paper on one of the first successful numerical model simulations of tropical cyclone developments Ooyama (1969) pointed out that: "Results also demonstrate that the supply of heat and moisture directly from a warm ocean is a crucial requirement for growth and maintenance of a tropical cyclone, <u>a fact that characterizes the tropical cyclone as truly a creature of the tropical oceans</u>." A similar statement can be made for polar lows, i.e., "the supply of heat and moisture directly from a (relatively warm ocean is a crucial requirement for growth and maintenance of some polar lows, a fact that characterizes these polar lows truly as creatures of the arctic oceans."

The question whether polar lows have any similarities to tropical cyclones has been rather controversial. There is no doubt, however, that certain features <u>are</u> common for tropical cyclones and (some) polar lows.

Fig. 1 shows a polar low. Satellite images such as Fig. 1 show convincingly, that polar lows exist as phenomena in their own right. Most of our knowledge of polar lows is of a relatively recent origin, and only 10 to 15 years ago most meteorologists were unaware of these phenomena. Going through the meteorological literature almost nothing can be found about polar lows prior to Harrold and Browning's 1969 paper on "The polar low as a baroclinic disturbance." What <u>can</u> be found shows that before 1969 the polar low was considered as a mainly non-frontal, convectively driven system.

Harrold and Browning's paper marks the starting point of a debate about the nature and structure of the polar low, a debate which is far from finished. In their paper they conclude that the polar low is basically a baroclinic disturbance of short wavelength. This point of view was challenged by Rasmussen (1977, 1979) and Okland (1977) who both supported the older point of view that the polar low was driven by deep convection and that baroclinic instability played a minor if any role at all. Rasmussen and Okland both made use of the CISK (Conditional Instability of the Second Kind) - concept, and in the following years much of the debate was concentrated on whether polar lows were of baroclinic nature, or of the CISK-type. One of the reasons why the CISK mechanism was only reluctantly accepted as a possible explanation for the formation of polar lows, was probably the widely accepted point of view that tropical cyclones only develop over regions with sea surface temperatures around or higher than 27 °C. How, then, could the same basic mechanism possibly work over the Norwegian and even the Barents Sea? However, as demonstrated by Rasmussen and Okland, even in these hostile environments far north, sufficient energy in the form of latent <u>and</u> sensible heat was available (over the sea) to drive disturbances such as polar lows. An important point stressed by both authors is that sensible heat transport from the relatively warm sea surface plays a much more important role for polar low developments than it does for tropical cyclones.

Reed (1979) identified the polar low with the so called "comma cloud." Comma clouds develop typically in a baroclinic region with a pronounced upper level flow, and poleward of, but relatively near to the polar front. Numerous examples of comma clouds have been described in the literature by Reed and others, and observations and theoretical arguments alike show that comma clouds are basically baroclinic disturbances. They are of relatively large horizontal scale, ~ 1000 km., and it seems possible to explain their basic dynamics from quasi-geostrophic theory. Comma clouds form mostly over the sea and often in regions of low static stability which ensures a strong response for a given dynamical forcing. Convection may add to the baroclinic development and possibly, as pointed out by Reed (1979), enhance some developments through a CISK-mechanism. More recently comma clouds have also been studied by means of numerical models and they probably are some of the best understood cold air mass phenomena.

The question whether comma clouds are "real" or "true" polar lows has caused much debate, and the answer depends of course on the definition of a polar low. An unambiguous definition, however, has never been generally accepted. Reed (1988) suggests a "broad sense definition" according to which the term polar low "denote any type of small synoptic or subsynoptic cyclone, of an essential non-frontal nature, that forms in a cold air mass poleward of major jets streams or frontal zones and whose main cloud mass is largely of convective origin." Other definitions as, for example, by van Delden (1985) "that a polar low is a warm core (like the tropical cyclone) vortex consisting of deep Cb-clouds," limits the number of polar lows much more and is difficult to use in practice. For practical purposes even Reed's definition seems too limited. Small scale cyclones with many features similar to large extratropical cyclones quite often develop, for example, in the North Sea region and northwest of the British Isles. These "baroclinic polar lows" may start as "convective polar lows" which fulfill Reed's definition, but later on they change into small baroclinic waves showing the typical cloud structures of a baroclinic wave An example of a baroclinic polar low is shown in Fig. 2.

Between the "ideal" types of convective polar lows (Fig. 1) and baroclinic polar lows (Fig. 2) we find numerous hybrid types where both convection and baroclinic instability are important. This can be inferred from satellite images and has also been shown theoretically and by numerical model studies.

It is not surprising that polar lows appear in so many forms. Some of them form very close to the ice edge, i.e., close to a semi-permanent secondary shallow baroclinic zone, and some, such as most comma clouds, much farther to the south close to the main baroclinic zone (the polar front). These factors as well as many others mean that a variety of forcing mechanism are effective which again leads to a whole spectrum of polar lows. A useful, practical definition should reflect this, and Reed's definition might be improved if changed to: "a polar low is a small scale synoptic or subsynoptic cyclone that forms in the cold air mass poleward of the main baroclinic zone and/or major secondary fronts. It will often be of convective nature but baroclinic effects may be important."

The proposed definition includes besides "real" polar lows as the one shown on Fig. 1, comma clouds, baroclinic polar lows and hybrid types. Excluded are the small but otherwise normal wave cyclones that quite often form on secondary frontal zones north of the major frontal zone.

Since 1977 the formation and development of polar lows of convective nature many times have been associated with CISK on numerous occasions. This implies that these polar lows in some ways should have a structure similar to tropical cyclones. Some common features indeed have been found. For example, deep convection is essential for both types of phenomena, and warm cores and/or "eyes" have been observed in polar lows. A more detailed comparison has not been attempted so far, probably because, even now, relatively little is known about the detailed structure of polar lows.

Physical factors important for the development of tropical cyclones include the release of latent heat in cumulus convection, the oceanic sources of moisture, and the conservation of angular momentum in maintaining the hurricane. Physical parameters related to these processes include:

* sea surface temperature
* degree of convective instability
* low level absolute vorticity, and
* vertical shear of the horizontal wind.

All the parameters mentioned above are also important in a polar low context.

The following discussion will be based mainly upon the December 1982 polar low episode discussed in detail by Rasmussen (1985).

During the late afternoon on 13 December 1982 a vortex-like polar low (in the following referred to as "polar low A," "polar vortex A" or simply "vortex A") formed in a preexisting cyclonic disturbance a little west of the Norwegian weathership AMI (71.5 N, 19 E). The fully developed vortex at midnight between 13 and 14 December 1982 is shown in Fig. 3. The vortex subsequently drifted in an easterly direction and its center passed almost directly over AMI. Figure 4a shows the surface pressure at AMI, and the estimated surface pressure at the center of polar low A from 12 to 14 December. Fig. 4b shows the surface mean wind velocity and pressure at AMI during the passage of the vortex. Finally Fig. 4c shows some important thermodynamic parameters.

During the passage of polar low A around midnight between 13 and 14 December, the value of q_w increased significantly at AMI as seen from Figure 4c. This increase was due partly to an increase in the temperature and partly to an increase in the dew point. The relatively small decrease in the surface pressure, p_s, had little effect. Ooyama (1969) mentions that for tropical cyclones a sharp decrease in p_s may raise q_e (and q_w) significantly, and in this way "boost" the surface air shortly before it ascends into the warm core. On the other hand Ooyama's numerical experiments show, that "this effect does not seem to be of crucial importance for development of a tropical cyclone."

The increase in the surface value of q_w is extremely important for the potential for deep convection in the vortex since the possibility for deep unstable moist convection in this region, just as in the tropics, depends critically on high values of q_w in the boundary layer. Boundary layer air with a $q_w = -2$ C which is representative for the conditions just prior to and just after the passage of vortex A (see Fig. 4c), will not be able to penetrate very deep into the troposphere. Depending on the degree of entrainment, surface air with $q_w > -2$ C can penetrate into the upper troposphere. Assuming arbitrarily that deep convection may proceed when q_w reaches a value between -1 C and 0 C we see from Figure 4b and 4c that this region is restricted to a relatively small region around the center of the vortex. Fig. 4c and Fig. 5 demonstrate the importance of "air of good quality," i.e., air with a high θ_w in the boundary layer, in order for deep, penetrating convection to occur.

As mentioned in Rasmussen (1985) the high temperatures in the disturbed region must be the result of an enhanced sensible heat flux from the warm surface. Correspondingly the increase in the dew point temperatures reflects the result of increased moisture fluxes. The potential wet bulb temperature, θ_w, which represents the temperature as well as the humidity increases because of the increased surface fluxes due to the increasing wind velocity. This in turn effects the intensity of the convection and the release of latent heat, and through this the intensity of the system. An intensification of the system means stronger surface winds, and a positive feedback system has been established. Several researchers (including the author) have proposed that (some) polar lows are the result of Conditional Instability of the Second Kind (CISK). In addition, other mechanisms such as baroclinic instability and the positive feed-back mechanism mentioned above might contribute to the developments. Recently, however, Emanuel and Rotunno (1989) have argued against CISK as a mechanism for polar low development (as well as for tropical cyclones) pointing out that in the real world very little Convectional Available Potential Energy (CAPE) will be available as envisaged in the CISK-theory. Instead they have argued that a new type of instability called Air Sea Interaction Instability (ASII) alone explains the growth of a hurricane-like vortex, without a reservoir of available potential energy. According to ASII enhanced surface winds associated with an earlier formed disturbance lead to enhanced surface fluxes of sensible and latent heat which are then redistributed aloft by convection. This leads to the formation of a warm core, i.e., an intensification of the system, and the positive feedback loop is closed.

It is generally accepted that the supply of latent heat and moisture from the warm ocean is crucial for the growth and maintenance of tropical cyclones. While there is some doubt about the role of the oceans as a source of sensible heat for tropical cyclones, these fluxes are very important for polar lows. The importance of an in situ sensible heat flux was pointed out already by Rasmussen (1977, 1979) and Okland (1977), and is also verified by the present case, where latent and sensible heat fluxes were both of the orders 500 Wm^{-2}.

The Arctic Instability Low

Polar low A discussed in the preceding sections formed as the result of a spin-up process of a previously formed low. Later, on 15 and 16 December 1982, another vortex C* of very small horizontal scale, <100 km, formed in the same region (Fig. 5). although of slightly smaller horizontal extent C* showed many similarities to polar low A.

This is illustrated by comparing the surface wind and pressure data shown in respectively Figs. 4b and 6. Although the fully developed disturbances had many similarities, their initial mode of formation were different. As the result of the rapid formation of vortex A, a sharp internal air mass boundary was formed separating a shallow, cold northerly surge of fresh arctic air from modified warmer air to the east (see Fig. 5). This internal air mass boundary or front could be identified on the satellite images from around midnight between 13 and 14 December, 1988 and until 16 December. At least two small scale vortices formed along this shallow front, and the center of one of them, C*, crossed Bear Island. The Bear Island surface wind measurements show a not very strong, but very symmetric disturbance. The inner core region with a tangential wind distribution corresponding to a solid rotation has a vorticity around 10^{-3} s^{-1}, i.e., only a little less than that associated with vortex A. The diameter of this region with solid rotation is of the order 50 km according to the surface observations, while the diameter of the cloud free region is only about half of that.

The region around Bear Island has a rather unique climatology and geography. This may explain why vortices like C* have not so far, or at least very seldom, been observed elsewhere.

The term polar low is generally used in a generic sense to include several types of phenomena. To distinguish between polar lows in general with a scale of a few hundred kilometers and disturbances like C* which form in highly unstable airmasses and has a much smaller scale (≥ 100 km), it is suggested to denote the latter "arctic instability lows," a term which sometimes but not very often has been used as a synonym to polar lows.

A Subtropical Polar Low

Cyclones similar to hurricanes are known sometimes to form over the Mediterranean Sea. These small scale cyclones may have a striking similarity with polar lows, with regard to horizontal scale, intensity, structure and mode of formation.

One of the mechanisms responsible for the development of polar lows is convection. On the other hand, more-or-less shallow baroclinic zones are almost invariably present in the regions where polar lows form, and it is difficult to assess the relative importance of convection versus baroclinic instability.

In a case studied by Rasmussen and Zick (1987) baroclinic instability did not seem to play any role at all for which reason this development, even if it took place over the Mediterranean, might be considered as an "ideal" development of a polar low of convective nature.

The analysis of the Mediterranean disturbance was carried out mainly by means of satellite data in form of cloud track wind data. All data available indicate, that the Mediterranean vortex which could be followed for six days was the result of a rapid spin-up cause by deep convection, of preexisting vorticity associated with a synoptic scale low of modest intensity. This way of formation is very similar to that of vortex A discussed in the preceding. Fig. 7 shows the vortex a few hours after its formation and Fig. 8 the vortex the next morning on 28 September a little southeast of Sardinia. The "active part" of the disturbance is associated with the tight cloud spiral with the eye. The cloud bands with the spiral structure around the central vortex are remains from the original synoptic scale cyclone (and quickly disappear). By comparing the central vortex in Fig. 8 with the polar low shown on Fig. 1, we note at once the striking similarity with regard to horizontal scale and general appearance. The vortex at this time is cyclonic through its whole depth.

On the following day, 29 September 1983, when the vortex is situated to the west of Sardinia and Corsica, it has developed a dense cirrus shield (Fig. 9). At the same time it has changed to a warm core system with anticyclonic, divergent flow aloft. The temperature of the cloud tops of the most active cunimbs in the vortex at this time is -54°C which corresponds to a height around 250 mb.

One day later the vortex makes landfall and the center of the vortex passes almost directly over Ajaccio at the west coast of Corsica. Even if the center does not pass directly over Ajaccio, the barograph curve (Fig. 10) shows a remarkable pressure drop of nearly 10 mb.

Concluding Remarks

Although polar lows develop in regions very different from the genesis area of tropical cyclones over the warm tropical seas, there is increasing evidence, that there are significant similarities between (some polar lows and tropical cyclones.

The similarities include a tendency for an axi-symmetric structure, sometimes around a vertical eye of small horizontal diameter. Polar lows never reach the intensities of tropical cyclones, but their surface wind field and pressure distribution are qualitatively alike. The remarks above are only valid for a certain group of polar lows associated with deep, vigorous convection. Polar lows form in regions where many forcing mechanisms operate simultaneously with the result that a whole spectrum of polar lows, ranging from the convective type discussed in the preceding to types resembling small scale baroclinic waves, can be observed.

Polar lows normally are associated with horizontal scales of a few hundred kilometers. In the present paper it has been demonstrated, that polar lows may form on an even smaller horizontal scale, <100 km, not very different from the scale of large thunderstorms. To distinguish between the "normal" polar lows of a scale of ~400-500 km and these very small scale systems it is suggested to use the term "arctic instability lows" for the latter. This term has sometimes, but not very often been used as a synonym for polar lows, and is well suited for the particular systems developing in a highly unstable air mass modified by strong air sea interaction processes.

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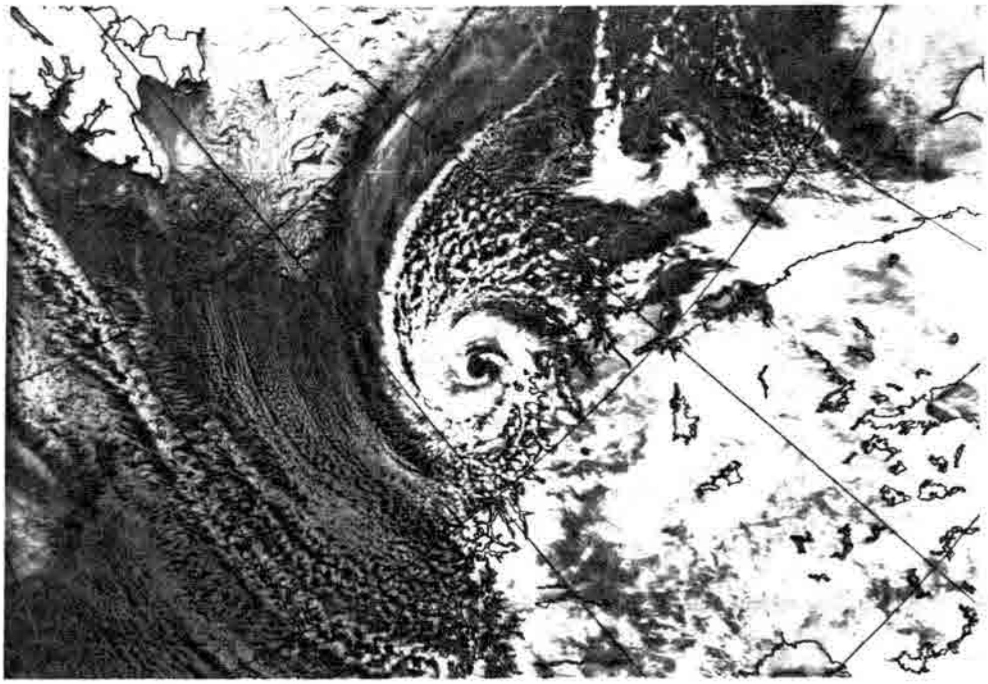

Fig. 1. NOAA-9 infrared satellite image 0418 UTC 27 February 1987 showing an exceptional symmetric, "ideal" polar low deep in the cold air mass just north of North Cape.

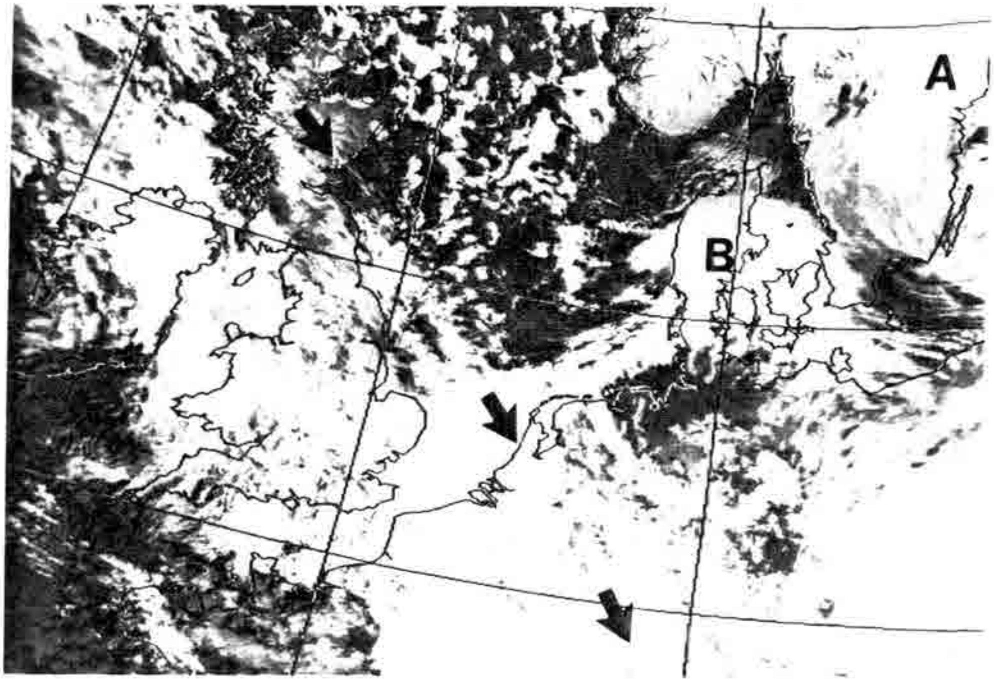

Fig. 2. NOAA-7 infrared satellite image 1404 UTC 7 December 1981 showing baroclinic polar low (B) over Denmark. The thick arrows indicate the jet stream associated with the main baroclinic zone, situated further south.

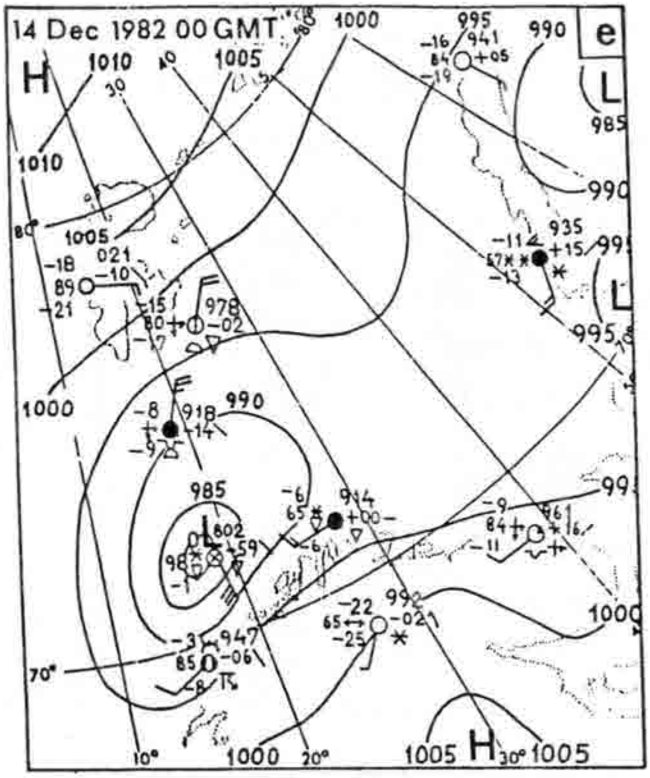

Fig. 3. Surface map 0000 UTC 14 December 1982.

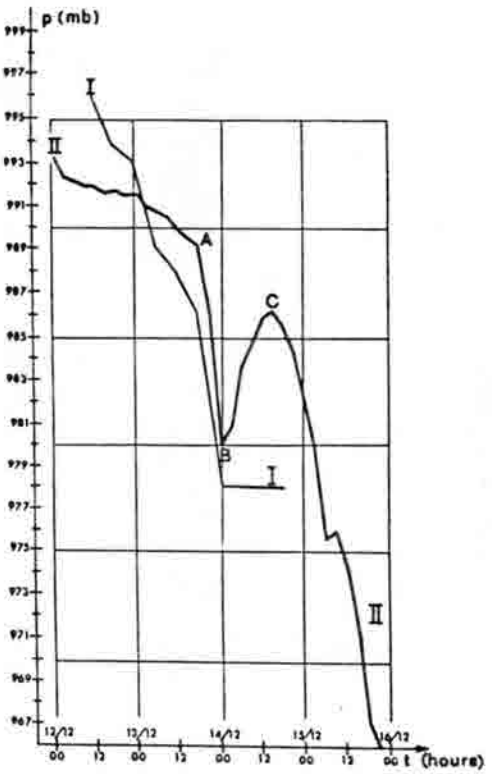

Fig. 4a. (a) Surface pressure as function of time at center of low/vortex A (curve I), and at weathership AMI (71.5° N, 19° E), (curve II).

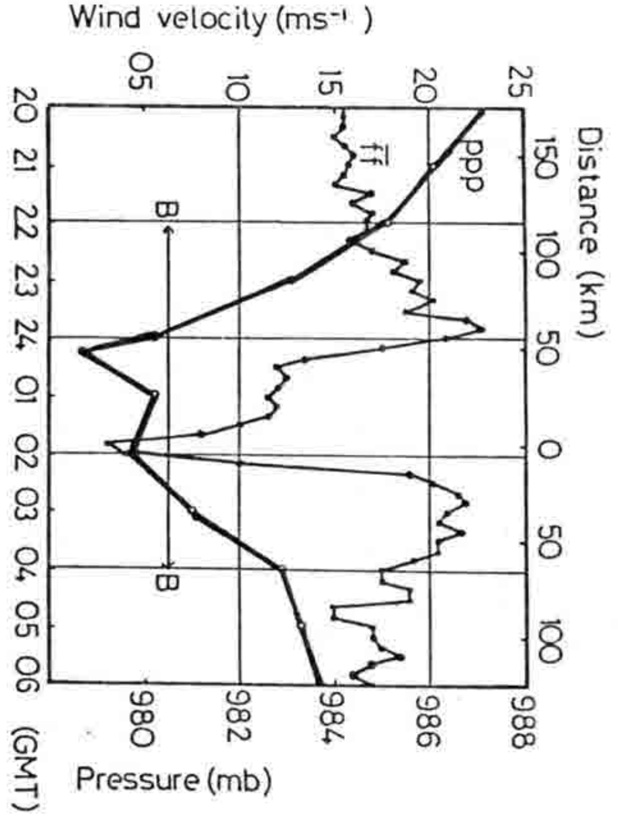

Fig. 4b. (b) Mean wind velocity ff in ms^{-1} (10 minutes mean) and surface pressure (ppp) from weathership AMI from 2000 UTC 13 December 1982 to 0600 UTC 14 December 1982. A length scale is shown on top of the figure. The distance B-B indicated on the figure is a measure of the horizontal scale of the system.

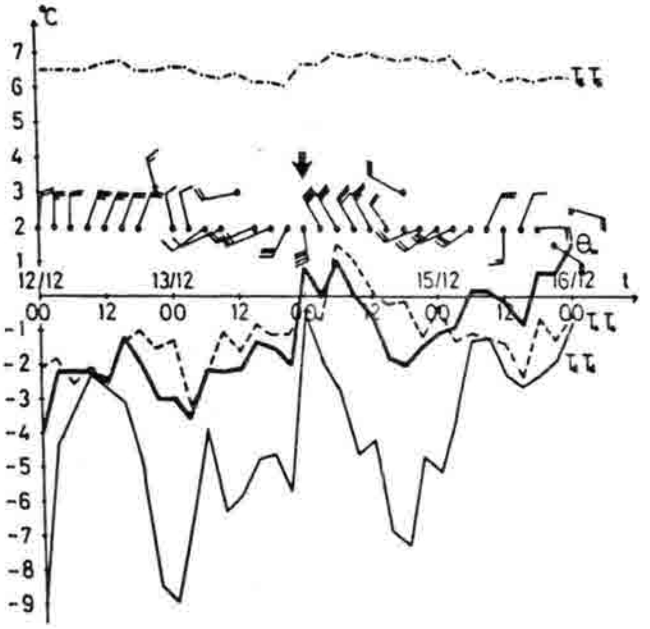

Fig. 4c. (c) Surface winds in knots (long barb indicates ten knots) and north "upwards." surface air temperature (T_aT_a, thin dashed line), dew point temperature (T_dT_d, thin solid line), surface wet bulb potential temperature (θ_w, thick solid line), and sea surface temperature (T_sT_s, dashdotted line), all from weathership AMI. The arrow shows the time at which the center of vortex A passes AMI.

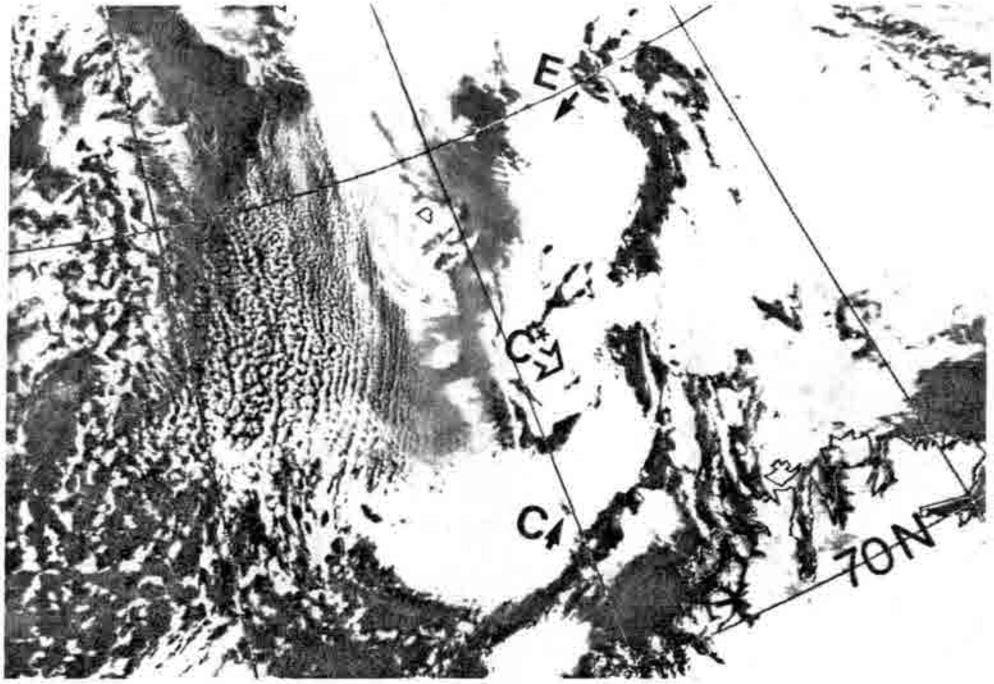

Fig. 5. NOAA-7 infrared satellite image 0406 UTC 15 December 1982. Arrows marked C, C* and E show a decaying vortex C, the newly formed vortex C* and a cumulonimbus cluster which later on develops into a vortex E. The shallow cold air mass west of 20° E is clearly indicated by the system of cloud streets and the region of a shallow overcast.

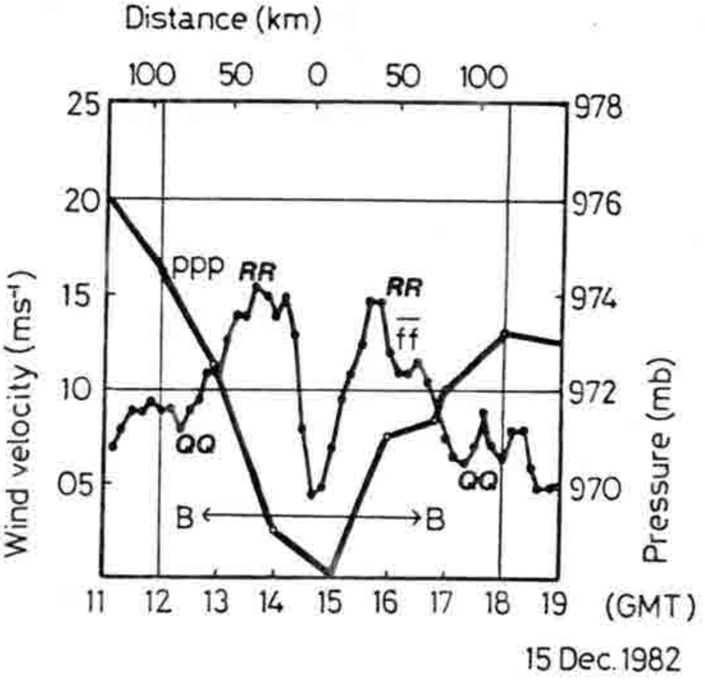

Fig. 6. Mean surface wind velocity ff in ms^{-1} (10 minutes mean) and surface pressure (ppp) during passage of vortex C* at Bear Island on 15 December 1982. A length scale is shown on top of the figure. The distance B-B marked on the figure is a measure of the horizontal scale of the system.

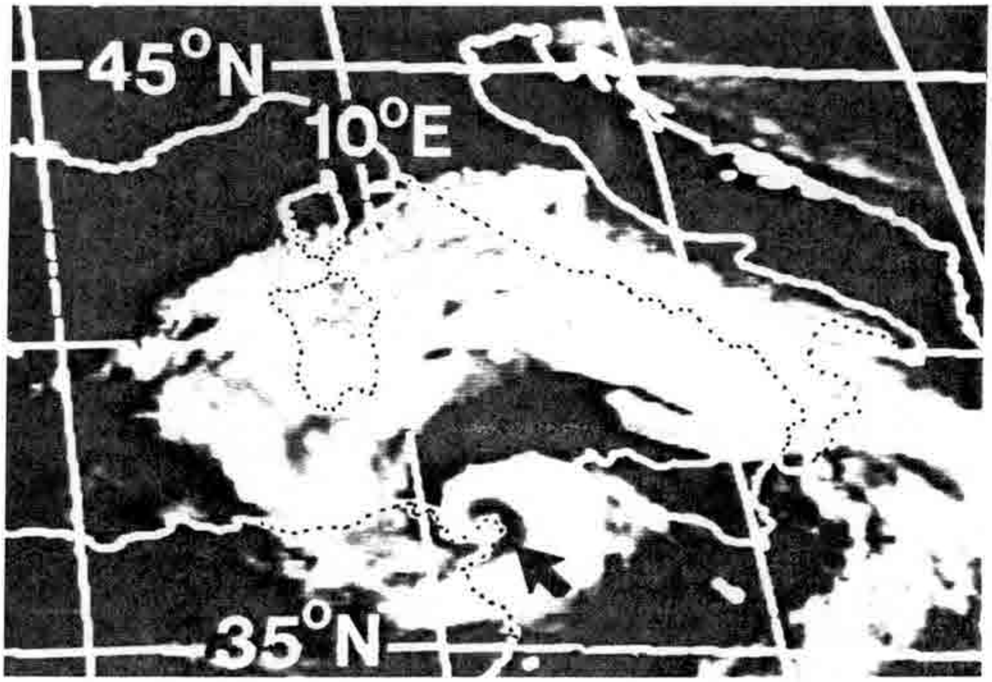

Fig. 7. METEOSAT infrared satellite image, 1800 UTC 27 September 1983, showing the initial formation of a subsynoptic vortex (at the arrow) over the sea close to Carthage in Tunis, North Africa. Received by ESA-ESOC, Darmstadt.

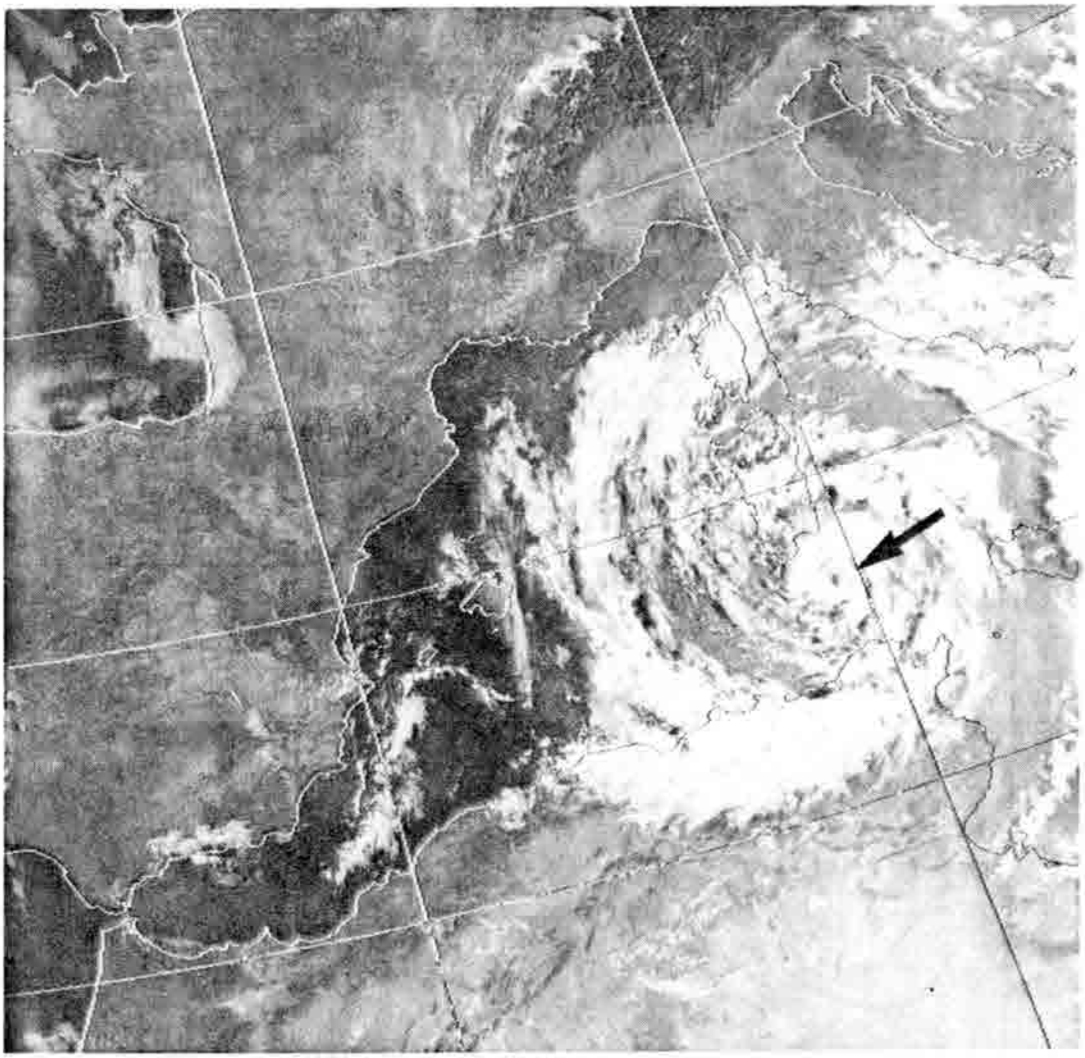

Fig. 8. NOAA-7, visible channel satellite image, 0807 UTC 28 September 1983, showing dissolving large scale cloud spiral with a subsynoptic vortex in the center (shown by arrow).

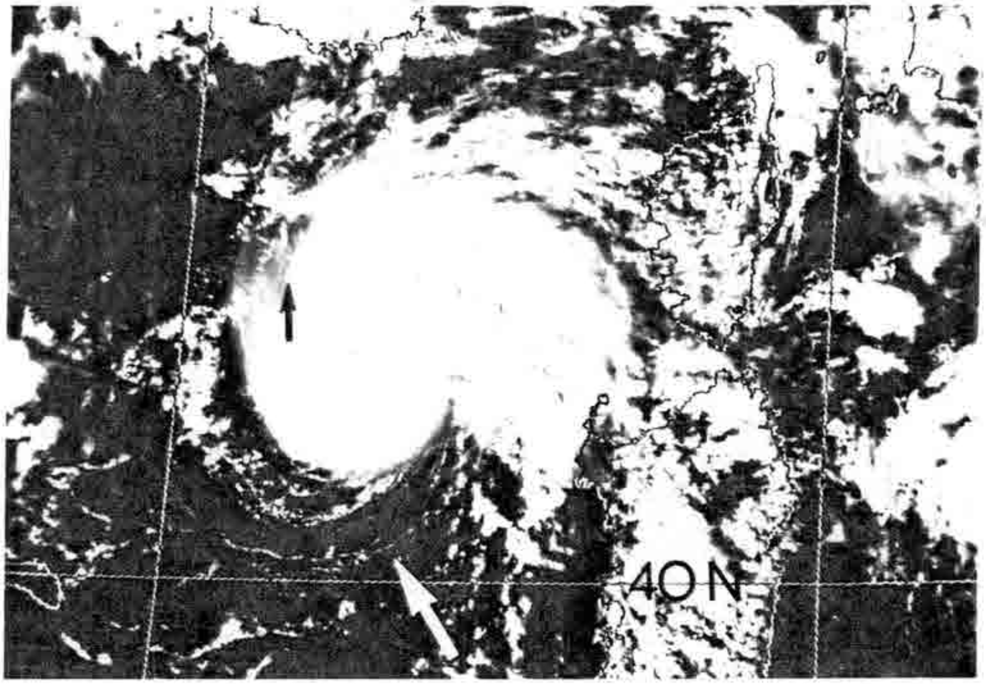

Fig. 9. NOAA-7, visible channel satellite image 14.18 UTC 29 September 1983, showing the vortex at the time when it is best developed. The white arrow shows rows of cumulus clouds converging cyclonically into the center at low levels. The black arrow indicates direction of anticyclonic outflow at upper levels. Corsica and Sardinia are seen to the east of the vortex.

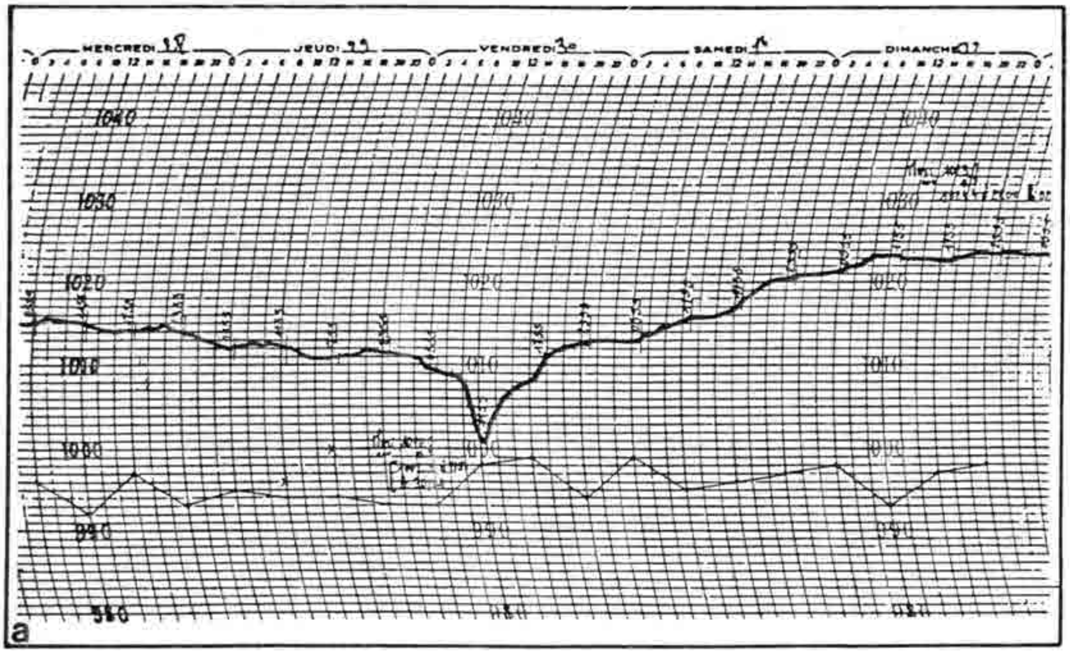

Fig. 10. Barograph record from Ajaccio from 28 September 1983 to 2 October 1983 showing passage of subsynoptic vortex at 30 September 1983.

The Interpretation of Radar and Satellite Imagery for the Production of Rainfall Estimates and as Input to Mesoscale Numerical Models

Talk given by Geoff Austin
At the CIRA Satellite Research Workshop
Pingree Park, CO
22 September 1988

Introduction

A review of some of the methods that have been used or have been proposed for the estimation of precipitation from satellites from the point of view of the use of modelling activities is presented in this paper. The presentation will be divided into two parts, the first concerning the estimation of rainfall patterns and the second the role of these in mesoscale modelling. The position taken here is to interpret a model in the broadest sense. In other words conceptual, statistical, as well as dynamical and thermodynamical numerical schemes will all be considered as models. This is perhaps a cloud physicist's definition of a model rather than that which would be used by a person from a background in numerical weather prediction. Some attempt will also be made to point out the dangers of using models as well as their advantages.

THE ESTIMATION OF RAINFALL

The basic problem in all aspects of remote sensing for rainfall estimation is that if we know exactly the distribution of rainfall and cloud density in space then it is possible, although possibly tedious, to calculate the appearance of the remote sensed image. The converse, however, is not generally true. This means that the observation of the cloud by whatever technique does not result in a unique rainfall rate. Thus all rainfall retrieval techniques are, in a real sense, dependent on indirect inference. The resulting accuracy is then not only a function of the accuracy of the observation but usually to a much greater extent on the validity of the conceptual model. To illustrate the use of models and the pitfalls involved a few techniques will be reviewed. The list is not intended to be comprehensive since more detailed surveys exist elsewhere.

Radar Techniques

Ground based radar estimation of rainfall is clearly the oldest remote sensing technique. The conceptual advantage is that the microwave radiation is reflected by the precipitation sized particles in the cloud and is totally unaffected by the cloud envelope. However, it is clear that the returned power depends on the radar cross section of the scattering particles which is usually described by the parameter Z, the radar reflectivity which is usually defined as;

$$Z = \sum_{\text{unit volume}} D^6$$

where D is the drop diameter. The rainfall on the other hand depends on the volume of the drop multiplied by its fall speed. The latter scales as $D^{1/2}$ approximately in still air. The rainfall rate, R, then becomes;

$$R \propto D^{3.5}$$

The difficulty is clearly that even for perfect radar data that a model is needed to relate the measured Z to the required R.

The model required in this case is that, for the volume of air under investigation:

1) There is a known updraft speed (which may be zero),
2) The frequency distribution of drop diameters is known,
3) The scatterers are all liquid and either all spherical or of a known cross section.

In general none of these conditions are exactly true so that there will be errors even for a perfect measuring instrument. The early measurement experiments of drop size spectra by Marshall and Palmer (1948) are well known and the assumption of negligible vertical wind velocity even in thunderstorms is routinely made. The history of this problem is interesting, however, in that many researchers, when faced with significant radar/raingauge comparison errors, have assumed that the error is predominantly due to the second of these assumptions. Many schemes involving the use of two wavelengths or polarizations have been proposed and some tested. The results are in fact rather poor and recent consensus at the AMS Battan Memorial Radar Conference (see also Austin, 1986) was that the majority of the observed gauge/radar discrepancies are due to;

1) differing sampling volumes for the two instruments,
2) miscalibrated radar systems,
3) radar beam overshooting the cloud tops,
4) microphysical modification of the raindrops in the lower atmosphere (virga for example),
5) geometrical problems (horizontal motion of raindrops between radar beam height and the ground for example),
6) too course space/time sampling.

The removal from radar data of artifacts such as ground clutter, interference and the effects of "bright band" (i.e. enhanced radar returns from the level of melting snow) has been a subject of research for many years. The use of many elevation angles to allow the construction of Constant Altitude Plan Position Indicator (CAPPI) displays has the effect of considerably reducing range effects and the amount of ground clutter (Marshall and Ballantyne, 1975). Further reduction of ground effects may be achieved by simple pattern-matching techniques, and some improvement can be achieved using a simple mask. The presence of "bright band" can be diminished by objective analysis of the vertical profile of precipitation (Collier, et al, 1980). The effects of the motion of the image between time samples is significant at even 5 minute resolutions. It can be eliminated by interpolating between images as has been done at McGill for the last 10 years. The resulting rainfall maps generally give good results when compared with gauges (~+/- 30% for 1-hour accumulations, Bellon and Austin, 1984).

Many of these problems are in fact treatable with more careful work, and effort in these areas is more likely to yield a steady improvement in results than a revision of the model described earlier.

Satellite Radar Systems

Spaceborne radars differ from conventional radar primarily by power and antenna size restrictions. The requirement for non-attenuating wavelengths (larger than 5 cm) would require prohibitively large antennas. The proposed satellite radar systems will be using wavelengths in the 1 - 3 cm band where attenuation of the beam even by relatively modest rainfall rates occur. Thus the various techniques proposed primarily depend on the measurement of this total attenuation as the beam passes down through the cloud, is reflected from the surface and then passes back through the cloud.

It is possible to have a situation where the attenuation/km is directly proportional to the rainfall rate with an appropriate choice of wavelength. In this case the measured attenuation A dB is given by

$$A = 2 \int_{0}^{H} kR\,dh$$

where k is a coefficient which depends relatively weakly on dropsize spectra, dh is the height increment and H the height of the raining column.

If we know the vertical distribution of rainfall rate we are now in a position to calculate the attenuation which would be observed. An appropriate 3-D cloud model can give such data and allows the necessary estimates to be made. Indeed running many trials with such a cloud model will yield a variety of surface rainfall rates which can be associated with estimates of total attenuation. While this produces an impressive curve which allows a one-to-one correspondence attenuation and rainfall rate, the procedure is not without its own built in assumptions.

Clearly the changing of the model parameterization in the cloud physics for example could result in different rainfall rates and even different heights so that minor changes could yield major changes in the A-R relationship. In reality, of course, there are at least several quite distinct rainfall producing meteorological mechanisms all of which have different height and rainfall characteristics. For example stratiform rain, cumulus clouds, and frontal rain bands all have significantly differing major physical processes. Even more discouraging is that real cumulus clouds when observed in vertical cross section by radar have rapidly changing vertical profiles of rainfall rate including the important near surface values. The result is the existence of very variable A-R relationships from cloud-to-cloud, time-to-time, and climate-to-climate. The question for climatological rainfall retrievals is whether the mean of all these processes is stable and can be adequately represented by a statistical model. The extent to which this statistical model has to be adjusted from place-to-place and season-to-season is an important area of future research as it is for many of the techniques described here.

Passive Microwave Techniques

In the microwave part of the spectrum the power emitted by a body is a linear function of its temperature. This allows the observations to be displayed directly as temperature values. The emissivity of the ocean is low (~ 0.5) and therefore presents a cold background against which clouds can be seen. In addition to this emission large drops and ice particles scatter microwaves away from the direction of the satellite thus lowering the observed radiative temperature. This scattering can be sufficiently strong that observations can sometimes be treated as the low temperature of outer space seen by scattering form particles in the cloud tops. The lower frequency part of the spectrum (<20 GHz) shows mainly emission effects while the higher frequency part (>35 GHz) shows mainly the scattering effects. The measurement of both the emission and scattering properties of the clouds in question allows the development of more subtle rainfall estimating schemes.

It is evident, however, that the observed radiative temperature of a cloud depends on rainfall rate, nature of the underlying surface, rain drop size distribution, cloud drop size distribution, rain layer thickness, the amount of ice above the rain layer, inhomogeneity in the field of view, water vapor concentration, wind speed and temperature at the sea surface not necessarily in decreasing order of importance. (TRMM Science Steering Group Report).

It is clear that a conceptual model or at least plausible values for the above parameters are required in order to develop a rainfall estimation algorithm from first principles. It is not surprising that there have been several different approaches to the development of such models (eg., Weinman and Guetter, 1977; Wilheit et al, 1982; Wu and Weinman, 1984; Szejwach et al, 1986; Kummerow, 1987; Olson, 1987). These models generally allow the prediction of radiation temperature patterns from known cloud parameters and only allow the estimation of rainfall if many of the significant parameters are assumed known. The alternative procedure is to establish statistical relationships between brightness temperature maps and observed rainfall rates at the surface. The concerns about the stability of such algorithms expressed in previous sections still apply and the usual problems associated with the use of sparse gauge networks or weather radar data apply.

VIS-IR Techniques

The enormous advantage of the geostationary IR and daytime visible data is that it has high spatial and temporal resolution as well as global coverage. The obvious problem is that the observations are not directly sensitive to the rainfall rate at all since only the shape of the cloud envelope and perhaps some estimate of its thickness is obtained.

A casual inspection of simultaneous radar and satellite data reveals that the area of rainfall is considerably less than that of cold cloud tops. Indeed, particularly in mid latitudes, extensive areas of cold cloud tops are associated with high but non-raining stratiform clouds. A variety of conceptual and experimental schemes have been proposed to deal with the problem which are described in detail elsewhere in this volume, the basic requirements are to:

1) eliminate non-raining clouds
2) to reduce the area of raining clouds
3) assign rainfall rates to the selected areas.

There are two general approaches to this problem:

1) The classification of the cloud type by statistical algorithms and then the assignment of probability or amount of rainfall. (Liljas, 1982; and Gerant and Weinman, 1986).

2) The estimation of cloud thickness from the observed albedo and then the combining of this estimate with the cloud height derived from the IR data. (Bellon et al, 1980).

A theoretical difficulty with the latter procedure has been that if typical cloud liquid water contents are used to calculate relationships between cloud thickness and albedo it is found that clouds thicker than a kilometer or so should have albedos near one. Observations, however, indicate that clouds in the thickness range of 0-6 km have reasonably different albedos (Figure 1). This so-called 'albedo paradox' (Welch, 1983) has recently been resolved by including into the calculation the effects of the observed extreme variability of the cloud liquid water (Lovejoy et al, 1989). It is thus possible to generate maps of rainfall rate, cloud height and thickness from the satellite imagery. Naturally, since some of these parameters are inferred indirectly, the accuracy is less than normally expected from radar data but is much better than the low resolution analyzed fields from mesoscale models, (Bellon and Austin, 1986). The accuracy of such procedures during the night is significantly reduced because the non-availability of the visible channel makes the elimination of cirrus clouds more difficult. Any practical deterministic dynamical cloud model is unlikely to be able to simulate cloud structure down to very small scales.

The assignment of mean rainfall rates for areas delineated as raining can obviously be achieved statistically given sufficient by good quality ground truth. The determination of the appropriate mean rainfall rate given that it is raining is straightforward from climatological data, but is likely to vary considerably with location, time of day, and season, for example. Attempts to determine this parameter have been based on observations of the rate of anvil expansion and one dimensional models which are primarily thermodynamic in nature. While these models are conceptually appealing, their accuracy is not high and in any case require knowledge of many parameters not generally known or easily measurable by remote sensing.

Sampling Problems

One area in which statistical modelling has achieved great benefits in our understanding, is in the question of the adverse effects of low spatial and temporal resolution. In this sort of activity long sequences of ground based radar data, or a statistical models of the rain field, are analyzed to see what results would have been achieved using various lower space time sampling strategies (eg., Kedem et al, 1988; and Seed and Austin, 1988).

Care has to be taken with the statistical treatment since any assumption of representedness or independence tends to reduce the sampling error and give unduly optimistic results. The sort of result obtained is that even a perfect rain measuring instrument on an orbiting platform overpassing a given location twice a day can yield mean errors as large as 200% for daily and 30% for monthly rain estimates over a 400 x 400 km area (Seed and Austin, 1988).

Conclusions

It has been argued that any interpretation of remotely by sensed data requires at least a conceptual model which describes why the particular characteristic of the observed image should be related to rainfall. In addition insight into the processes involved can be obtained by constructing a physical (numerical) model of the cloud rainfall process and the required radiative transfer to form the image. A grave problem with this type of activity is that present models are unable to reproduce either the subgrid scale variability or the case-to-case variability actually observed in the atmosphere. In addition they require a prior knowledge of assumptions about a large number of meteorological parameters. The third type of model is statistical and is generated by comparing the observed image parameters with the best available "ground truth". This latter has been assumed to be trivial by too many researchers but in fact is probably the most difficult of the three modelling activities to do correctly. In addition it is the only one of the techniques which yields directly some reasonable estimate of the probable accuracy of the estimates.

Mesoscale numerical modelling is still at a stage where it has little to contribute to rainfall estimates since its accuracy of prediction is significantly worse than even rather crude estimates from satellite and in any case requires accurate initial data to yield even plausible rainfall amounts (Roch, 1986).

FORECASTS MODELS

The use of radar and satellite data to aid short range operational forecast requirements has been widespread for many years. Most operational forecasters employ subjective schemes which depend on observing image motion and development and then, combining this information with their knowledge of local conditions and effects, produce the short range forecast. While this procedure is easily implemented it does not usually generate statistical scores of skill and may show considerable variability in the quality of the results depending on the amount of training, experience and ability of the forecaster as well as the complexity of the particular meteorological system. The applicability of these subjective schemes to the specific problem of the prediction of quantitative rainfall amounts is even less clear. The steps used in the generation of short term forecasts can be outlined as follows:

1) The production of quality controlled fields of cloudiness, rainfall and severe weather from the raw radar and satellite imagery.

2) The intercomparisons of the past history of these fields with other available meteorological data and models.

3) The preparation and dissemination of the short term forecasts based largely on extrapolation.

In order to improve the quality of the results and to further understand the limitations imposed by the ultimate predictability of small scale meteorological features, two further steps are required:

4) The scoring of the accuracy of the resulting forecasts by comparison with real images obtained at the forecast time or by some other "ground truth".

5) The recording of the data used and the forecast products so that the techniques may be optimized on a large representative set of data.

All of these operations can be performed by the forecaster directly or by computer or by a combination of man and computer. The desired degree of human intervention depends on many parameters, including the availability and cost of appropriately-trained individuals, the quality of the input data and the complexity of the meteorological systems. At McGill University we have concentrated our work on fully automated systems which have the considerable practical advantage that the latter two steps described above may be combined into a self learning optimizing system allowing the complete system to be installed in different geographical regions with rapid improvement of its skills even in the absence of experienced local forecasters. That is, the construction of an artificially intelligent automatic nowcaster.

Combining with Mesoscale Analysis

Browning (1979) presented a general strategy for a total system of weather forecasting. On the shortest time scales and smallest space scales the system relied primarily on radar and satellite observation of the weather. On the largest scales numerical models initialized from surface and upper air observations were to take a primary role. On the mesoscales the fine mesh numerical models provided the majority of the input to the final forecast. A simplified version of Browning's (1980) scheme is shown in Figure 2. The initialization of mesoscale NWP from the satellite imagery and more particularly the improvement of extrapolation schemes using the output of mesoscale NWPs are of major concern here.

Mesoscale numerical models have also been implemented and tested for forecasting in the 6 to 24-hour domain (Golding et al (1985) for example). Much less consideration has been given to the essential cross-linkages between the various techniques shown in Figure 2.

The initialization of mesoscale models is usually accomplished by nesting them inside synoptic scale numerical models. This is done since the analysis produced by the larger model effectively interpolates the sparse synoptic data in space and extrapolates them in time. While this procedure has operational advantages and may well generate plausible initial fields of wind, pressure and temperature, the observed high resolution fields of cloudiness, humidity and rainfall show very much greater variability. This extreme discontinuity of pattern does not yield meaningful interpolated fields. If these distributions of water constituted a minor component of the energy fluxes, it is possible that the errors involved in the use of the smoothed fields of water substance would yield acceptable results. However, it is clear that in many circumstances, over both large and small scales, most of the energy in the mesoscale system is contained in the latent heat changes so that cloudiness and rainfall become major determinants of subsequent rainfall but also the dynamics. The work of Roch (1987) contains examples of modification to the initialization of a mesoscale model by the introduction of the effects of satellite-derived cloudiness and rainfall distributions by the modification of the initial vertical motion and resulting convergence fields. In the absence of these cloud and rain data the model underestimated the rainfall over most of Quebec by about a factor of 20 (i.e. a 2000% error!). After modification the peak rainfalls were within a factor 2 of the observed values over part of the Province but completely missed a secondary area of rain showing maxima as large as 40 mm. The subsequent dynamic evolution of the low pressure area was significantly modified in pressure and location. Obviously a numerical model with inadequate initialization of water substance and cloud physics is not likely to be useful in the short term prediction of rainfall.

The relative accuracy with which rainfall estimates may be made from satellite data has recently been examined in Bellon and Austin (1986) where errors of the order 80% for point rainfall accumulations over one hour and reasonably high statistical scores are obtained (CSI = 50% for a scale of 8 km for hourly accumulations). The errors introduced in simple extrapolation forecasts were investigated by both Browning et al (1982) and Bellon and Austin (1984). The latter reported errors in the radar forecast rainfall of about 50-60% for forecasts in the 1/2 to 3-hour time domains on a scale of 4 km. While these errors are quite large and may well be more than optimum for operational requirements the extrapolation forecasts are, in fact, much better than those obtained from existing mesoscale numerical weather prediction schemes, where our limited experience suggests errors of an order of magnitude or more in rainfall amount frequently occur even when verified at low spatial resolution. If the image extrapolation schemes are scored with greatly reduced resolution comparable to the numerical models then the numerical skill values rapidly increase since errors due to trajectory misprediction largely disappear.

If quantitative rainfall predictions made by mesoscale NWP and image analysis schemes are compared, this must be done on the same spatial resolution. We do not believe this has been done for any statistically significant data set, but initial impressions yield very low quantitative score values for the mesoscale NWP schemes. It would be interesting to determine the cross-over point in forecast skill illustrated in Figure 3.

Extrapolation

It is clear that the skill of extrapolation forecasts of rainfall patterns derived from radar/satellite/gauges falls off considerably a few hours after the initial time. There is an attractive possibility that the introduction of ancillary data from surface networks and/or mesoscale models can improve the quality of the prediction. However, the work by Tsonis and Austin (1981) showed that elaborate extrapolation procedures using observed historical growth or decay in rainfall area and/or intensity showed negligible improvement of skill over simpler schemes in spite of many pious expectations to the contrary amongst the meteorological community.

If we classify raining meteorological systems into two broad classes (convective and stratiform) then only in the second case do large scale numerical models have any chance of predicting the subsequent evolution of the precipitation, since the process is being forced from the large scale. In the air mass thunderstorm case, for example, it is not clear that the subsequent development of existing showers depends at all on the slow changes in the large scale fields. Although there is some evidence that intensity and area changes may depend a little on thermodynamic variables, there remains the interesting question of the ultimate predictability of these small scale systems. This ultimate predictability might be limited by the requirement of extremely fine scale input data down to 100 m or less. Alternatively, the sensitivity of the extremely non-linear processes to small fluctuations in the input characteristics of the airflow may be such that practical prediction with a numerical model at small scale is not possible. Lorentz and others have suggested that no amount of improvement in the quality of the input data nor improvement of the power of the computers will extend the useful range of NWP forecasts by more than a few days. In consideration of small scale rainfall systems, this fundamental limit may well apply at a few hours. It would therefore seem to be highly desirable to introduce a much more statistical approach to forecasting these events where the likely range of evolution is predicted. Exactly how to do this is not clear to the authors, but such a strategy may prove essential if any real progress is to be obtained in the long run.

If we turn our attention to what we believe are the more promising cases where the first order physics is forcing from the large scale, then the question arises as to how tendencies and trajectories predicted by the mesoscale model can be put into the image extrapolation scheme to extend its useful range. Numerically predicted trajectories of large scale systems and winds could be used to modify the small scale system trajectories given by the image extrapolation system, and large scale predicted fields such as moisture advection, surface wind convergence and static instability could be used to parameterize the growth and decay of systems that appear in satellite pictures.

The practical procedure for trying these schemes will clearly have to be statistical and could be based on the familiar Model Output Statistics (MOS) technique described in more detail by Glahn and Lowry (1972). The procedure can include the image extrapolated trajectories in the available predictors so that any improvement in a average scores will readily become apparent and in any case it will be no more than the image based system. The critical role of a large independent data set for evaluation is clear.

The objective of this statistical procedure would be to combine the results of the two techniques as illustrated in Figure 3 so that a single system can yield better forecasts than either of the two original systems by themselves.

Conclusions

a) In order to make useful extrapolation forecasts the input fields need to subject to considerable quality control.

b) Short term (up to a few hours) extrapolation forecasts of precipitation yield scores significantly better than models.

c) The mesoscale numerical models provide rather low scores in their quantitative predictions of precipitation. This is particularly apparent when allowance is made for the rather coarse spatial resolution of the models compared with the radar and satellite image extrapolation schemes.

d) The mesoscale NWP schemes need to be initialized with high resolution rainfall and cloudiness data.

e) There is some chance that predictors derived from mesoscale NWP systems can improve the accuracy of rainfall estimates on time scales longer than 3 hours.

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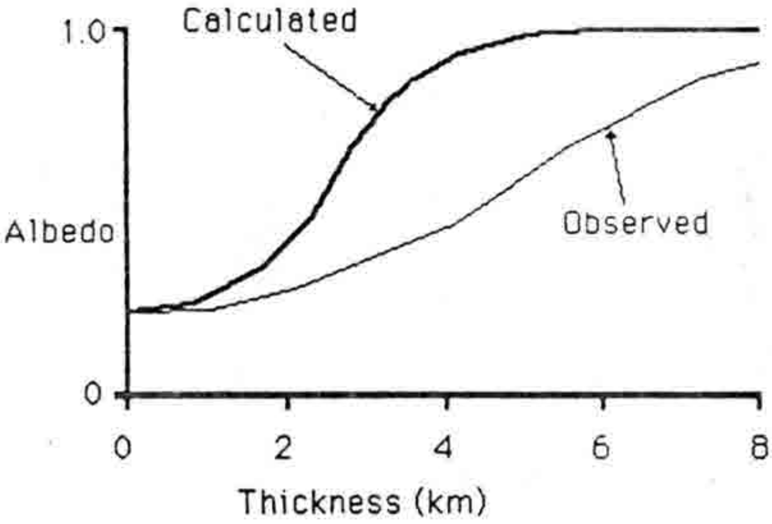

Fig. 1. Theoretical and experimental relationships between albedo and thickness of clouds.

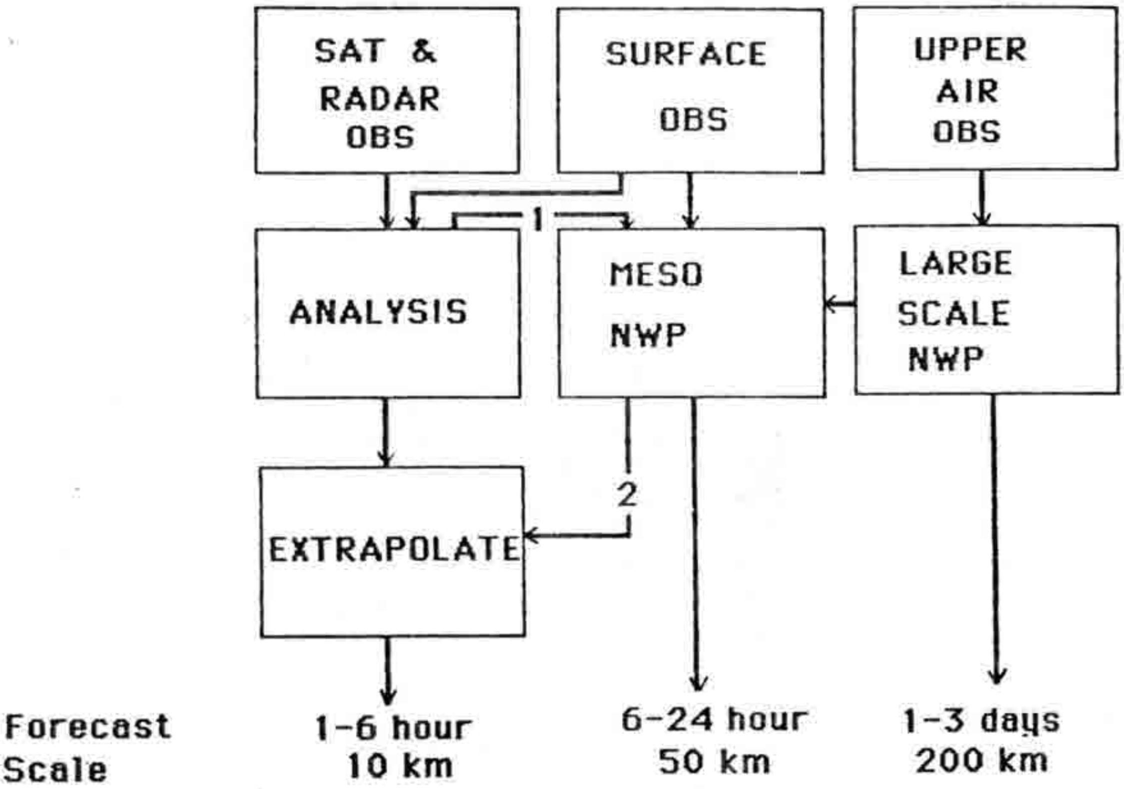

Fig. 2. Total forecasting system.

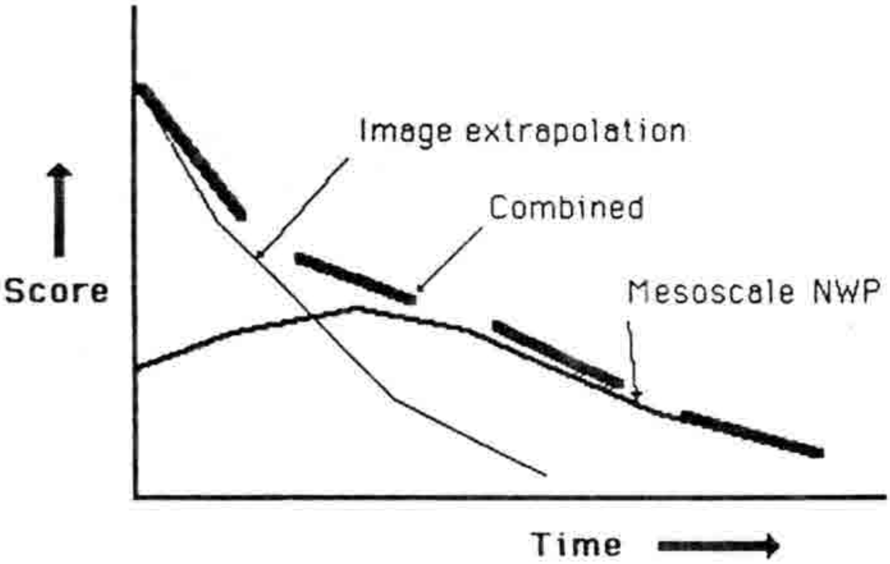

Fig. 3. Possible combined forecasting system results.

Severe Storms

Talk given by James F.W. Purdom
At the CIRA Satellite Research Workshop
Pingree Park, CO
22 September 1988

As I go along I hope to offer some ideas for the types of research we might be doing with the current and future generation of meteorological satellites to investigate convection and severe storms. This first photograph, Figure 1, shows the type of damage that was done by a tornado, on 28 March 1984. I plan to talk about severe weather, but first I want to address convection and mesoscale phenomena. What is the difference in how we perceive weather when our main observational tool is a satellite?

First, let's address the question of observational perspective. Figure 2 shows how convection often appears from the ground, as does Figure 3 if there is a developing thunderstorm, or Figure 4 if a thunderstorm happens to be directly overhead. What you are able to conclude concerning the local state of the atmosphere from the types of clouds you see is effected by the curvature of the earth, by haze and other restrictions to visibility, including clouds, and by obscurations such as trees and buildings. The earth based observer is limited when trying to assess the clouds and state of the sky that in turn reveal information about the atmosphere's mesoscale structure. As our observational platform changes from ground based, perhaps to an aircraft, as in Figure 5, we begin to detect considerable variability in the cloud field, which reflects mesoscale atmospheric variability. For example in Figure 5, the type of cloudiness varies considerably: from humble cumulus to thunderstorms, and from stratiform cloudiness to seemingly organized cloud free regions. If a rawinsonde were released at one particular location versus another, within the area shown in Figure 5, you should expect different information, especially concerning the structure of the boundary layer.

I will return to observational perspective in a moment, however, first I want to pursue this idea of mesoscale variability. Prior to the high resolution geostationary satellite, there was a lack of routine observational information on the evolution of deep convective clouds and their local environment. The mesoscale was a data sparse region, and meteorologists were forced to make inferences about mesoscale phenomena from macroscale observations. With satellite imagery, meteorological phenomena that are infrequently detected at fixed observing sites are routinely observed: for example, with the GOES satellite a "reporting site" exists every 1 km using information available from the visible data and every 8 km using infrared data. In addition, geostationary satellites allow very frequent interval views of cloud fields. This allows the use of animation to detect many of the mesoscale mechanism's responsible for triggering deep and intense convective storms on time scales compatible with their life cycle. I will speak of the sounding capabilities later, but with these thoughts in mind, let's return to the topic of viewing perspective.

Let's move our observational platform even higher, to that of a geostationary satellite. As I just mentioned, one of the advantages of GOES satellite imagery for mesoscale meteorological applications was the ability to view the cloud field over an area both frequently and with high resolution. Figure 6 is a 1 km resolution satellite image, over the central Florida area, taken by the GOES-East satellite at 4:30 PM, EST, on 18 September 1987. About one third of the way up from the bottom center of the image Lake Okeechobee appears as a dark and near circular cloud free region. Much of the of the brighter anvil cloudiness in the image is associated with thunderstorm activity. Since GOES-East is located over 75 West at the equator, and this images center is near 28

North and 80 West, the viewing perspective does not allow for the detection of much of the sides of these clouds. Now a natural question is, "which of the thunderstorms are active, and which are not?" Figure 7 provides part of the answer to that question. In that 1 km resolution GOES-West image, taken 15 minutes after the previous image from GOES-East, thunderstorms take on a much different appearance because of the different viewing perspective. With GOES-West located over 135 West at the equator, the viewing angle over Florida is very oblique. This view from GOES-West allows the detection of the sides of the base of the active thunderstorms, the side of the cloud and then the top of the anvil cirrus blowing away. The thunderstorm activity to the south of Lake Okeechobee, along the east coast, is associated with a bright convective tower while the activity that was slightly north of Lake Okeechobee along the east coast is inactive: there is no tower connected to the anvil cirrus. It is important to realize that there are differences in how a cloud field will appear depending on satellite viewing perspective. This can be an advantage, as illustrated here, when interpreting satellite imagery. Another advantage of having two satellites view the same cloud field from different perspectives is that by using geometry one can use stereo techniques to assign the height of cloud features commonly observed by the two satellites: not only can cloud top height be determined, but in some cases such features as cloud base and its height above the ground can be determined. For example, we should be able to measure the change in height of cloud base as a function of time across a scattered cumulus field, perhaps a summertime situation in which thunderstorm development is expected. That change in cloud base might be related to the deepening of the moist layer and its destabilization. No doubt, a combination of this information with a mesoscale boundary layer model and satellite low level moisture information would be appropriate. The point is, this information could be related to a thermodynamic profile. By tying that information in, perhaps, with satellite sounding data, we might be able to diagnose the low level moisture field and how it is structured over an area. That would be an interesting area of research that ties satellite imagery, soundings and mesoscale models together in a common goal.

Generally, organized convergence zones that trigger strong convection are detectable in satellite imagery prior to thunderstorms forming along them. Those convergence zones may be associated with dry lines, frontal zones, or an area of prefrontal convergence. The ability to locate areas of incipient squall line development was one of the earliest uses of geostationary satellite imagery in severe storm forecasting. Figure 8 is a typical example of squall line development as detected using geostationary satellite imagery. These four GOES-East visible images were taken on 14 June 1976. In this series of pictures, a very well defined convective line is detectable a few hours prior to thunderstorm development along it. I'd like to pose two questions: "Why are the thunderstorms developing along one part of a squall line sooner than another?" and "Why are the storms stronger along one part of the line than another?" While the southern part of the line is virtually stationary, the northern part of the line advances fairly rapidly toward the east. This implies, that along the northern part of the line, stronger low level convergence exists than to the south. Furthermore, with the jet stream axis to the north, one might expect positive vorticity advection, and broad scale ascent across the northern part of the line would be stronger than to the south. Noon time surface observations across the region showed warm and moist surface air across Kansas, with a tongue of warmer and more moist air extending into portions of Missouri and Iowa. Again, with the jet stream axis to the north, one might expect that gradual lifting as well as upper level cooling over that area would steepen the lapse rate and lead to a more unstable air mass in that region than to the south. How this instability combines with the vertical forcing along the convergence line to produce thunderstorm activity is very important. At the present time, it is impractical to routinely release rawinsondes at frequent intervals at mesoscale spacings to measure instability in an area of expected squall line development. However, frequently available information from satellite soundings may be able to provide information concerning the variation in instability over a region of potential squall line development. If the width and depth of the vertical forcing associated with a convergence line can be estimated with some degree of accuracy, then a forecaster might be able to predict the location and time for the onset of deep convective activity. These are among the type of questions that we need to address with satellite data. "What is the instability?" "What is the vertical forcing along these boundaries?" "How do they tie together to give us thunderstorm activity?" We need to develop experiments to do this. Currently planned activities, like the national STORM program, are certainly targets of opportunity for interaction. But we must get involved and assure that our unique questions are addressed as those experiments are developed.

There are a couple of things that I want to bring out at this time, to extend what we've just seen, and to bring out some different thoughts. Figure 9, taken at 2:30 pm CST on 11 March 1988 is another example of squall line development. A bright convective line extends from near Fort Worth (FTW) in north central Texas into southern Oklahoma. Extending into the rear of that bright convective line, from near the western edge of the image, is what appears to be wispy cloudiness, or perhaps blowing dust. In this case, however, the lighter appearing area is neither blowing dust nor cloudiness but rather smoke from range fires: the black area in west Texas locates the burned grasses associated with the fire area. To the east of the developing line skies are clear, becoming cloudy again west of Tyler (TYR), there is huge mesoscale variability in the cloud field across this image. Most of the cloudiness to the east of the line is cumuliform with a distinct ropy appearance except west and north of McAlister, Oklahoma (MLC) where the cumulus is more scattered. Such ropy appearing cumulus are an indication of cumulus clouds developing under a fairly strongly capped low-level inversion, as will be shown shortly. Figure 10, taken 3 hours after the previous one, shows that a squall line has developed across Texas and Oklahoma. However, 150 km to the east of that squall line the air is very stable, as indicated by the type of cloudiness there and as shown in the sounding for the Longview area (GGG), Figure 11. That sounding was released near the time of the image shown in Figure 10. Notice that while the lowest levels are nearly adiabatic, there is a very strong capping inversion that must be removed if thunderstorms are to form in that area. Does that mean that we have a lid across this area, like Toby Carlson has talked about? We should be able to follow the hot and dry air from the Mexican plateau, the stable lid, using satellite multispectral techniques. What is the structure of the lid and of the low level moisture field between Longview, where it is cloudy and the wide clear zone just ahead of the developing squall line? There are a number of ways that such stability may be overcome: broad-scale vertical motion, precipitating into the stable layer and destroying the inversion through evaporative cooling, differential advection at various levels, or strong vertical forcing along the surface convergence zone. Because of the proximity of the stable air to the squall line and the lack of mid-level clouds over the region, the first two mechanisms mentioned above most likely did not play a significant role in this case. Most likely a combination of the later two mechanisms came into play in the development of this squall line. Again, if it were possible to get a time sequence of local air mass characteristics from geostationary satellite soundings, both the question and its answer might become more clearly focused. In the near future we will be faced with the challenge of combining Doppler radar network data, GOES-I satellite data and continuous wind profiler data to explain this mesoscale variability. That is an important challenge.

It's interesting to note that a large amount of smoke can still be seen in the GOES-East image in Figure 10; also note how clearly the cirrus at the bottom of this image appears. This is six-bit visible data, as is currently available from GOES. Figure 12 is from the GOES-West satellite and is near the same time as the GOES-East image in Figure 10. Notice that you don't see either the smoke or cirrus nearly as well with the western GOES as with the eastern GOES at this time of day. At this time of the day there is a lot of forward scattering of the sun's visible light by the smoke and cirrus into the telescope of the GOES-East satellite, while this is not the case with GOES-West. This is one reason that I'm very excited about the 10-bit capability we're going to have with GOES-I through M series of satellites. We will have an east and west GOES, just like we have now. With such high bit resolution, we should be able to use both satellites to look at the change in reflectance throughout the day, at very high accuracy, image to image and also satellite to satellite. Hopefully we will be able to better address a number of problems dealing with smoke, pollution, haze, smog, and cirrus, to name but a few. For example, over the Platte River Valley in Colorado in the winter time there is a serious pollution problem. We hope to be able to use the 10-bit visible imagery to track the movement of pollution through that area. We might even be able to say something about the pollution by using the differences in reflection into the two satellite's telescopes. Just how we approach the use of 10-bit visible imagery is a research problem, and is something we have got to come to grips with. There are phenomena in the preconvective environment that we might want to try and investigate using this new type data. For example, in summer time, the southeast United States is very humid, very moist, with limited visibility due to haze because of moisture and aerosols, the latter due mainly to terpenes emitted by trees. I think we'll be able to use the difference in scattering between GOES-East and GOES-West, similar to the haze region Tom Vonder Haar showed you with the 10-bit INSAT data, to say something about the haze layer. Perhaps by using stereo, and identifying common features in the haze layer, we will be able to define the depth of the moist boundary layer across an entire mesoscale region. The point is, there are real targets of opportunity that we might not even have thought of using this new kind of data.

Back to convective phenomena and severe weather. Figure 13 is a GOES satellite image taken at 2000 UTC. Or was it? Actually this GOES-East image over the United States was taken between 3 and 6 minutes after the hour, because of the time that it takes to scan from the polar region down to and across the United States. Now consider the surface observations. In practice they are normally taken 10 to 15 minutes before the hour. For mesoscale applications, we should realize that there may be as much as a 20 minutes mismatch between the satellite image and surface observations that indicate they are for the same time. In the past, this has led to some misinterpretation of mesoscale features seen in the satellite image. When you put data together, as Geoff Austin was saying, you better realize what some of the very basic characteristics are.

Figure 14 is, what I consider, one of the most beautiful pictures of a thunderstorm that I've seen in quite a while. The storm is in the Dakotas, and has formed along a well organized convergence line that is easily seen in the satellite image. This image, if you haven't already guessed was taken by the GOES-West satellite. You can clearly see the western and southern base of the storm, up the side of the storm and across its anvil which has an overshooting top area with a nice downstream wake. Feeding into the storm from the southeast is a well defined flanking line. Notice how the towers along the flanking line get progressively larger from south to north where they eventually merge into the main cell. Behind the storm, on its northwestern side, is a region of low level stratiform cloudiness: I think, a reflection of the rain cooled air that the storm has left behind. What a view! We're looking right into the storms. When doing severe storm work, this is the satellite we ought to be using. The eastern GOES satellite view was masked: most of the storm was obscured by the anvil.

COMMENT FROM AUDIENCE: I think, Jim, as you said earlier, we have to use both satellites. In the near future, the new three dimensional display techniques will allow us to combine the two views and get a 3-D illusion. I think that's very important.

I agree. Three dimensional display, its promise to provide a common ground for integrating satellite with radar data, stereo, they are all very important. I just wish we had previously given close inspection of severe thunderstorm development using research rapid scan imagery from GOES-West. I can't wait to get hands on it now, I believe it may provide some crucial observations of tornadic storm development. I'm going to talk about tornadoes in a few minutes, and, hopefully show how things like being able to see distinctly into the back side of the storm will be very important. At any rate, I don't think we've taken advantage of satellite viewing perspective. For example, the polar orbiting satellites are in a much lower orbit, and much of the view is at slant angles; a lot of energy has been spent trying to correct for this, instead of use it as an advantage. I actually think it's a terrible disadvantage to have something you're interested in sitting right under the satellite subpoint, especially if it's convection.

I would be remiss if I didn't talk about some of the opportunities to use satellite data to study cloud top temperatures associated with severe storms. As Bill Shenk pointed out previously, we're going to have an improved system for measuring cloud top temperatures from space with GOES-Next. An important problem, yet to be solved is how do we put information from different sensors together. I'm speaking specifically to integrating satellite cloud top temperatures with Doppler radar data. The NEXRAD Doppler is going to provide routine volume scans at 5 minute intervals during severe weather outbreaks, as will GOES-I. However, with GOES -I, for research, we hope to obtain 30 second interval imagery, MSI, over selected severe storm regions; about the size of Oklahoma and Kansas. What we will see will certainly be interesting and provide a rich data set for research. However, we need companion observational data sets to go with this super rapid scan data. Especially important will be data at 30 second intervals from Doppler radar, however, the NEXRAD can't accommodate that rapid a repeat interval. We need to use some of the research radars to help in this area. Perhaps we will only be able to do limited RHIs at 30 second repeats, however, we need to begin to prepare for this now. There are a plethora of interesting questions to be addressed. Observations by Fujita and others have shown very rapid changes occur at cloud top. How do they relate to storm intensity and updraft structure? At CIRA, we have shown that the coldest part of cloud top, as shown with GOES IR data, does not coincide with the top of the visible overshooting top. What implications does this have?

I'd like to point out one thing, simple targets of opportunity that I feel we've missed. Let's talk about nocturnal low level jets for a minute. When you think about a low level jet forming over the Great Plains of the USA, that jet may well advect warm and moist air northward. Through various mechanisms that jet, with its warm moist air, may aid in the development of strong thunderstorms, perhaps a mesoscale convection complex. Stabilization of the boundary layer is one of the important processes that occurs allowing the low level jet to form. What happens when there is cloudiness present? Is the jet of the same intensity when nighttime skies are clear versus when they are cloudy? This needs to be studied. Obviously satellites are a valuable observational tool in such an endeavor. I don't think we've even gotten into something as basic as this in our satellite interpretation: watch the surface cool off, have the sounding near it, try to figure out where the boundary layer is stabilizing the quickest, see how that relates to low level jet formation under the proper conditions. In a similar mode of thought: What about a dry line forming during the day and helping trigger afternoon thunderstorms? What if there is thick cirrus over the area that inhibits daytime mixing? If that occurs then we're not going to get the vertical mixing and the downward transport of momentum and the development of a strong dry line. Perhaps. Have we studied this? So basic, so easy to do, yet we somehow passed up these targets of opportunity. Outflow boundaries and severe storms. We've been talking about arc cloud lines as seen in satellite imagery, thunderstorm outflow boundaries, and their influence on thunderstorm development, including severe and tornadic storms, for many years. We have made research aircraft flights to investigate these boundaries and documented that strong convergence and vorticity exists along them. Doppler radars are now picking up lots of information on outflow boundaries and storm structure. We need to combine our knowledge from these various observational systems: research aircraft, rapid scan satellite and Doppler radar. Figures 15a and 15b show a thunderstorm outflow boundary, produced by the large thunderstorm complex over the Gulf of Mexico, as it moves on shore near Boothville and Lake Charles, Louisiana. Notice how near Boothville the winds shift in response to the mesofront passing by, with no thunderstorms forming nearby. However, near Lake Charles, thunderstorms form as the arc cloud line moves onshore. Why the difference? If we assume that the vertical forcing in depth along the outflow is near the same, then the reason for thunderstorms in one region along it versus another must be due to mesoscale variations in the atmospheres ability to support deep convection in one region versus another. Notice that in the Lake Charles area there are preexisting cumulus while there are none in the Boothville area. The atmosphere, through formation of cumulus in one region versus another, is telling us something about how it is destabilizing differently in one region versus another. Figure 16 is a multiple exposure image for this case. Notice how the cloudiness along the arc cloud line decreases in time, and how the distance between the arc image to image gradually decreases. I've measured the decrease and found it to be on the order of 15% or so over the two hour period. However, the cloudiness decreases dramatically along the arc during that time. This is because the convergence in depth is decreasing dramatically. While the convergence due to horizontal velocity may only decrease by 15%, the depth of the leading edge of the cold air along the arc may decrease by slightly more than 20%. Thus the convergence in depth has decreased significantly. That is why arc cloud lines need to interact with some other convergence mechanism to be most effective in triggering new deep convection. Obviously, the earlier in the arc cloud line's life that it interacts, the better. We need to learn how to tie this information together. Obviously, how well the cumulus along an arc or convergence zone shows up in satellite imagery is a function of the moisture and instability of the atmosphere in which they form. In the High Plains, where there is not the abundant moisture that exists over the southeast United States, the cumulus will not show up as well. However, Doppler radar is showing promise in detecting these boundaries and convergence zones when they are close to the radar. In fact, one thing that I read in the report by the Denver Weather Forecast Office was the single most important thing they thought they were able to do with Doppler radar data was follow these boundaries and nowcast thunderstorm development. Where some good research could be done is in using satellite sounding data and cumulus development to determine the amount of vertical forcing needed to trigger deep convection.

Now, my pet theory on why tornadoes occur. Figure 17 is a GOES-West image of the Wichita Falls tornadic storm. Using our three dimensional perspective, it is apparent that the convective towers feeding into the overshooting top have formed along the boundary separating rain cooled air left behind by the storm, and the more unstable environment to its immediate south. As these super cell storms rain and move along, they leave behind a stable region in the lower levels. That stable region introduces a new boundary layer to the rear of the storm, relatively speaking. This is shown schematically in Figure 18. This new boundary layer, left behind by the storm would be stable, and thus locally inhibit vertical mixing that would have been prevalent in the "dry line" air behind the squall line. A sudden nocturnal inversion so to speak. In that area, the surface layer is decoupled. I think this would help accelerate the air into the back of the storm leading to a strengthening of the outflow in the region where ground chase teams have reported the "rear flank" downdraft that is associated with super cell tornadoes. I would sure like to investigate this. It has consequences beyond the tornadic storm. That is a storm redefining its local environment and this in turn influencing how the storm evolves.

We know from a number of case studies, as well as modeling studies, that vertical wind shear is an important parameter in severe storm development. This is something we ought to look at immediately with the GOES-I system. It's a good target of opportunity. We ought to be able to measure cloud wind shear across any region that we have cumulus developing and be able to tell something about the type of storms to expect, super cell, multi cell, or scattered storms over that area.

I think the new generation satellites we have coming up are going to give us information about the atmosphere that is absolutely incredible. What we've got to do is go in with well thought out objectives keeping in mind the other new data that will be available that will allow us to validate what we do. We've got to have good conceptual models, we've got to validate those models with measurements, then we can explain the atmosphere better, and that's what it's really all about.

Okay, let's entertain a few questions. (What follows is edited for grammatical purposes only, and unless otherwise noted, are comments from the general audience.)

A) Just a comment, I guess. As I listen to your talk, and look at all those images, I am kind of frustrated by the amount of information that is available in the images and the complexity of trying to figure out how to unify that information in some way. I think that we've been touching on it here; the idea of looking for some unified principles, and I guess conceptual models represent one way to do that. Maybe we need to identify a sum. What are the important mesoscale or storm scale issues where we need conceptual models? We need to get people working on the ones that at least the community feels are the most important, and develop those conceptual models, perhaps to the point of reducing the interpretation to solving a pattern recognition phenomena. I think we need to reduce the number that we're trying to work on. There appear to be enough for everybody to spend the next 100 years working. How can we unify this effort?

B) We've had satellite data in field stations for a long time. We find that the people in the field stations are doing some things routinely: trying to use cloud cover, trying to detect early squall line development. We know about outflow boundaries as detected using satellite imagery and the Doppler radar experience in Denver with boundaries. I think that those are types of things we know we ought to measure and quantify for accurate conceptual models.

C) Phenomena that we can reproduce using a mesoscale model are worthy of quantifying with field measurements and using to develop conceptual models. If we can't use a numerical model to reproduce what the basics of our conceptual model show, then we need to understand why and perhaps revisit the problem. Mesoscale models are powerful tools that ought to be used to tie the conceptual model, satellite observations and field measurements together. I think there's root work that needs to be done in this area.

D) How you exactly set priorities for the research to be done depends how good a salesman the person is trying to get his job done; basically that's what it comes down to. For example, I'd like to see satellite soundings used to extend the moist static energy studies and thermodynamic analyses of Betts, Telford and others. I would like to unify our work with the modeling in terms of the temporal and spatial variability of the moist static energy. How we're used to approaching a problem is an important consideration in how we address the research we want to do. I think that it would help us unify a lot of what we see with what the models can reproduce.

E) Part of the answer to your question is some research is done because a user has a requirement for specific information or products. Research for an immediate goal. That's one way, obviously, to set priorities. Another way research is done is because an interesting idea or thought on something relating to the atmosphere comes up: a good idea that ought to be investigated. Those are two basic ways that we set priorities. I feel that right now, in a way, we're almost driven too much toward the immediately goal type research by user communities. Make everything pay off right now, give me the answer right now. If you can't validate what you're doing right now to some problem in the next two years, we're going to cut you off. This is bad; it's poison. If the other type research is not done, it will be akin to "eating our seed corn." We can't afford to lose our basic research. That poison is running rampant through some of our funding agencies right now. It's just going to end up killing someone if we're not careful. One major problem we have is how to educate the decision makers that may not understand the fundamental meteorological problems and don't realize the importance of basic research. That's what I think.

F) I just want to tie onto a comment that you made relative to the user of the data. We better start thinking seriously about how the user is going to react to the information. The field forecaster, who's often at the end of the line, is inundated with information. We better start thinking about asking for assistance in how to apply that information. The type of thing that Greg Forbes talked about yesterday with the training manual that is being developed has to be done.

G) The influx of data that we're going to see in meteorology four years from now is going to be absolutely astounding. I'd love to see a psychological study done on meteorologists' research or operations with this influx of data. Just to see ratio of nervous breakdowns to intelligent observations would be interesting, because we're going to see things that we don't even know exist today. However, we need to be sure that we keep a good balance between immediate product oriented research and basic meteorological research.

H) Vince made a point. He said that there had to be both types of research. I think we've got to be careful as we develop a product for operations. We're not thinking, maybe, about the research that needs to be done, and while the two are certainly related they have important differences. I think that when we develop a product for operations we must try and figure out what the man who is going to use it is really trying to do. That's a different problem than the research required to get the answer. They relate to each other, but they each have their different requirements and approaches. If you let one dominate, if you worry about getting it to operations before you really know what it is you're giving them, then you might miss the answer. I'm a little worried that sometimes we get those confused.

Fig. 1 Photograph from aircraft of damage done by a tornado in North Carolina during the 28 March 1984 outbreak.

Fig. 2 Typical photograph of how clouds appear to a ground-based observer.

Fig. 3 Ground based view of a developing cumulonimbus.

Fig. 4 View from the ground when the cumulonimbus shown developing in Figure 3 moved overhead as a mature thunderstorm.

Fig. 5 Photograph from an aircraft showing the mesoscale variability in a cloud field.

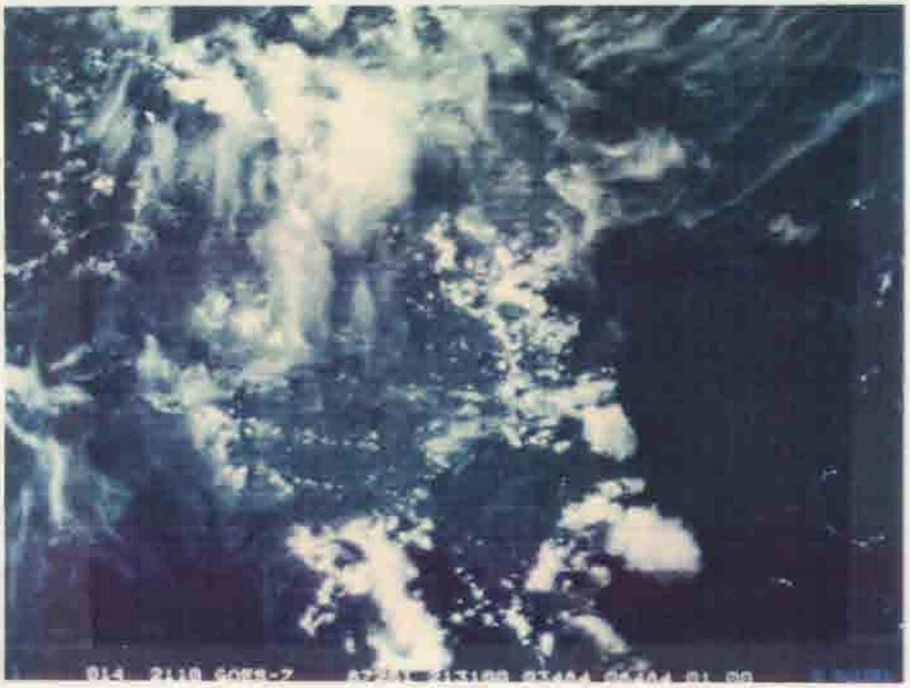

Fig. 6 1 km resolution GOES East image taken at 4:31 pm EST on September 18, 1987, of the central Florida area.

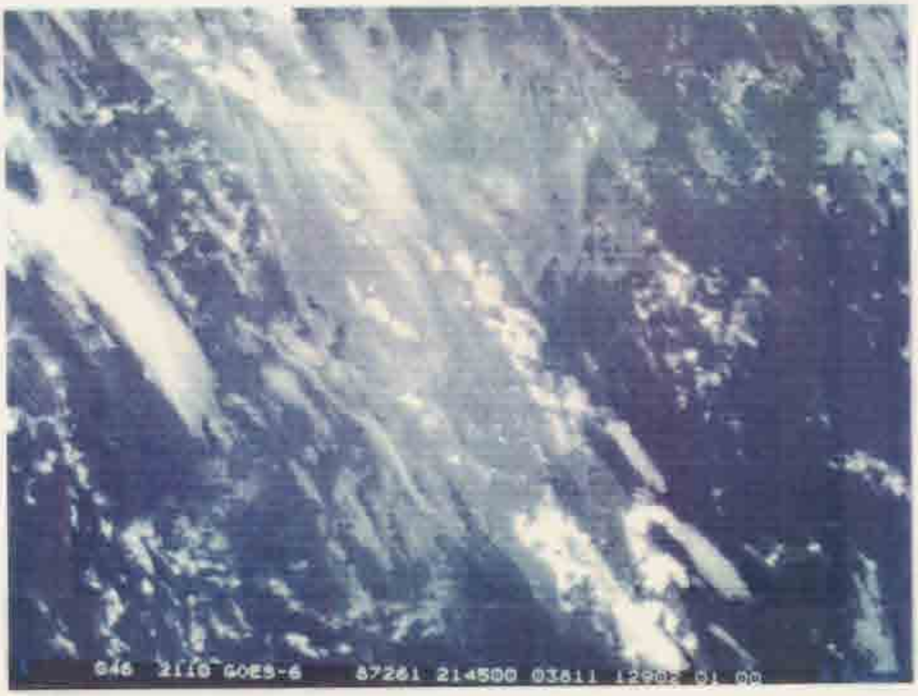

Fig. 7 1 km resolution GOES West image taken 15 minutes after Figure 6 and of the same area.

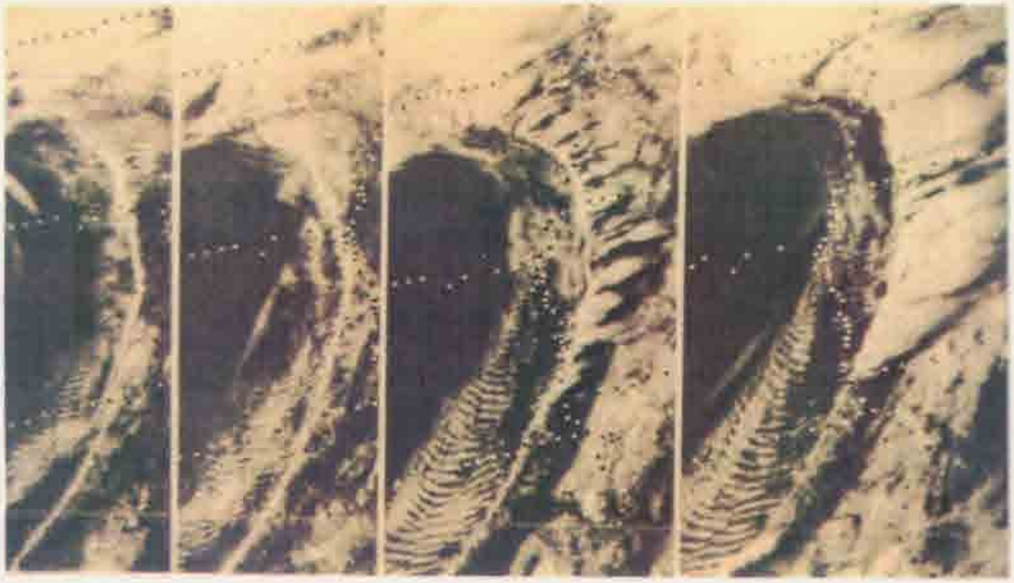

Fig. 8 GOES visible images at 1 pm, 2 pm, 4 pm and 5 pm CST showing squall line development on 14 June 1976.

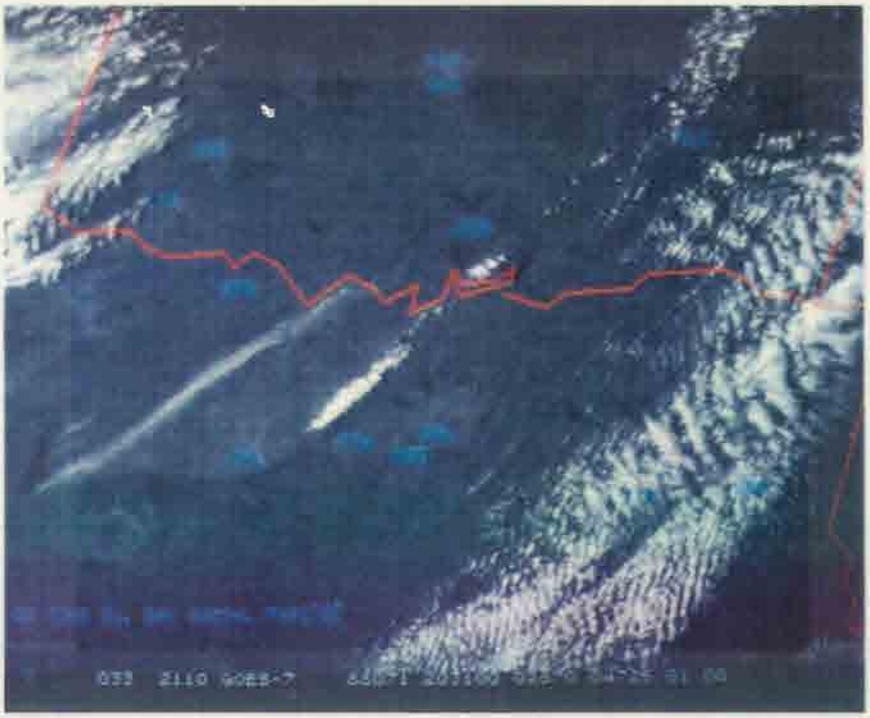

Fig. 9 GOES East 1 km resolution visible image at 2:30 pm CST on 11 March 1988 over portions of Texas and Oklahoma.

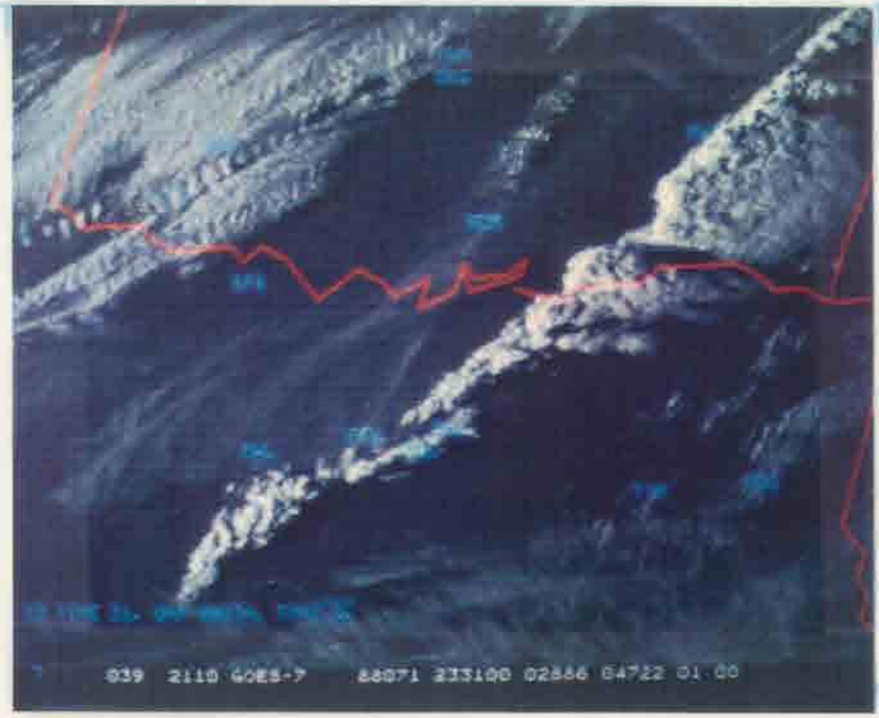

Fig. 10 As Figure 9, but 3 hours later.

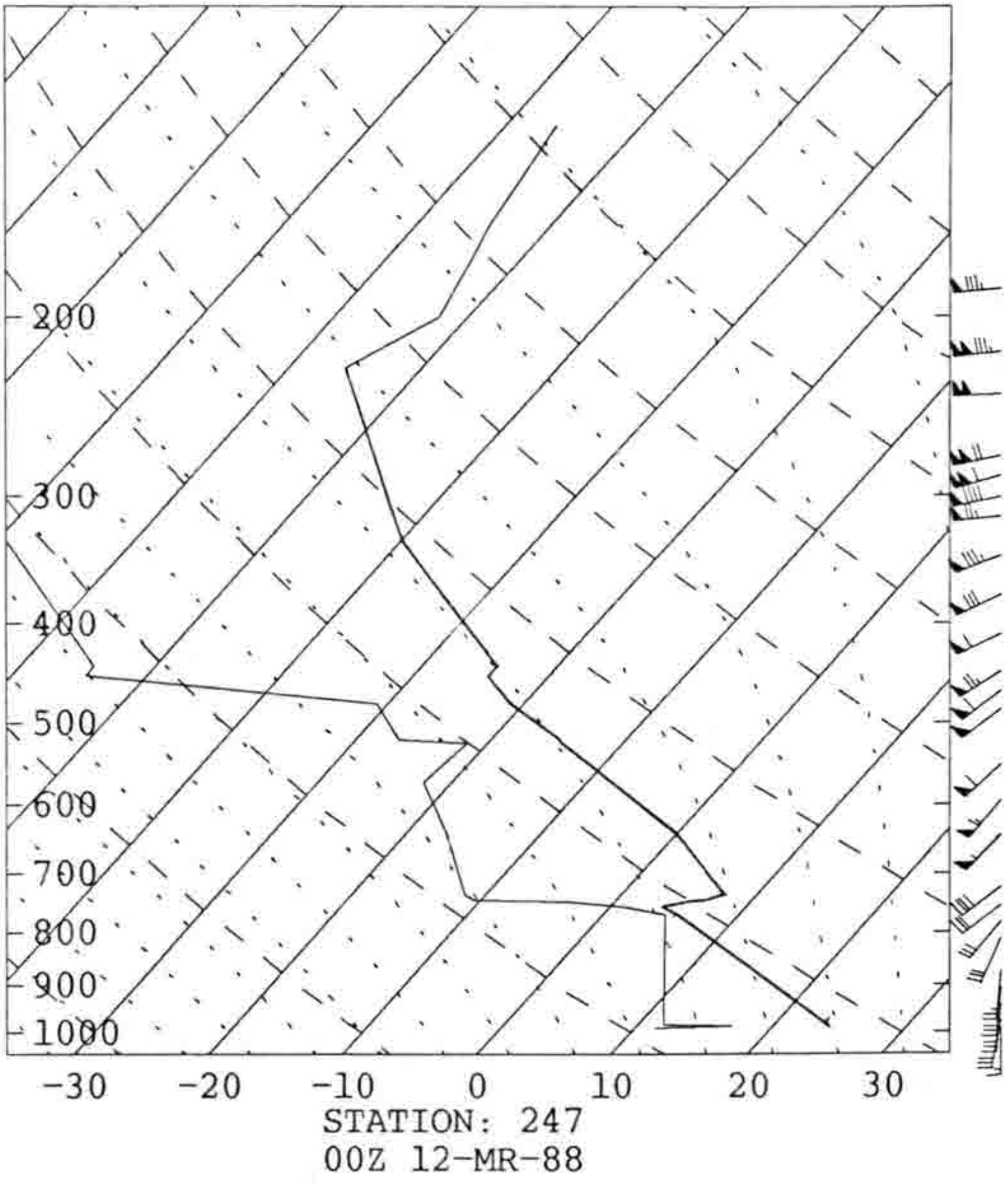

Fig. 11 Sounding for the Longview area (GGG), near the time of Figure 10.

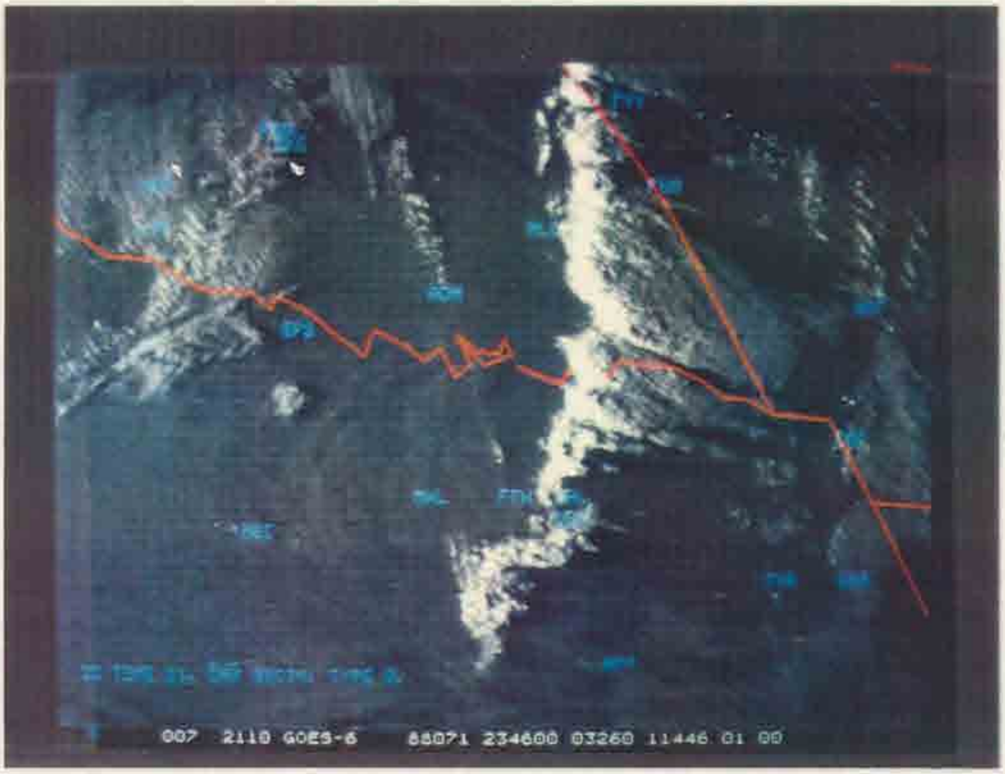

Fig. 12 GOES West 1 km resolution visible image corresponding to Figure 10.

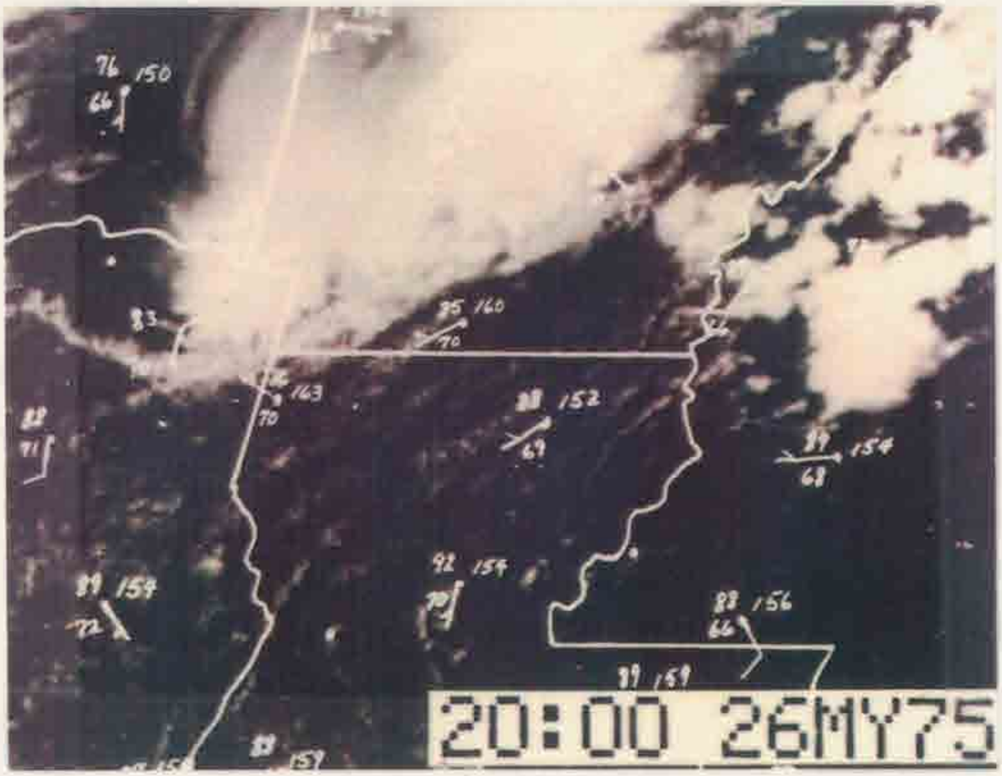

Fig. 13 GOES East 1 km resolution visible image at 2000 GMT showing thunderstorm produced outflow boundaries over northeast Texas and western Mississippi on 26 May 1975. Surface observations are for the 2000 GMT observation reporting time.

Fig. 14 GOES West 1 km resolution image at 5:45 pm CST on 6 July 1988.

Fig. 15a GOES 2 km resolution visible image at 1 pm CST on 30 July 1986 showing a large thunderstorm complex and arc cloud line over the Gulf of Mexico.

Fig. 15b As in Figure 15a, but at 3 pm CST with the arc cloud line moving on shore.

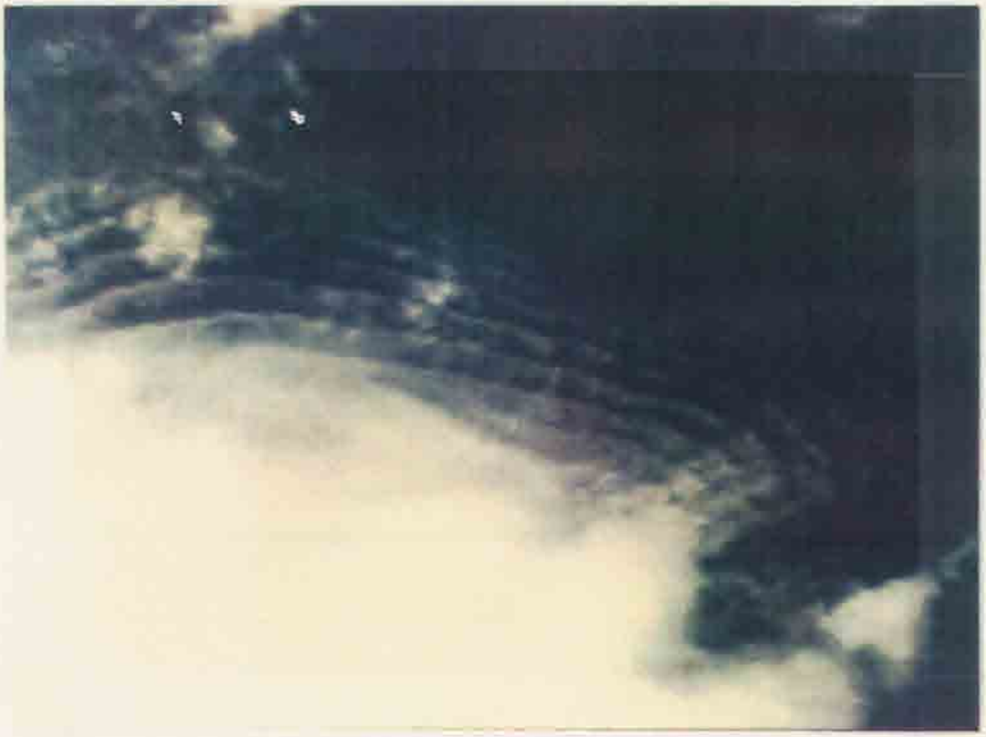

Fig. 16 Multiple exposure GOES imagery covering about 2 1/2 hours during the life of the arc cloud line in Figure 15a & b.

Fig. 17 GOES West 1 km resolution image of the Wichita Falls tornadic storm at 7:15 pm CST on 10 April 1979.

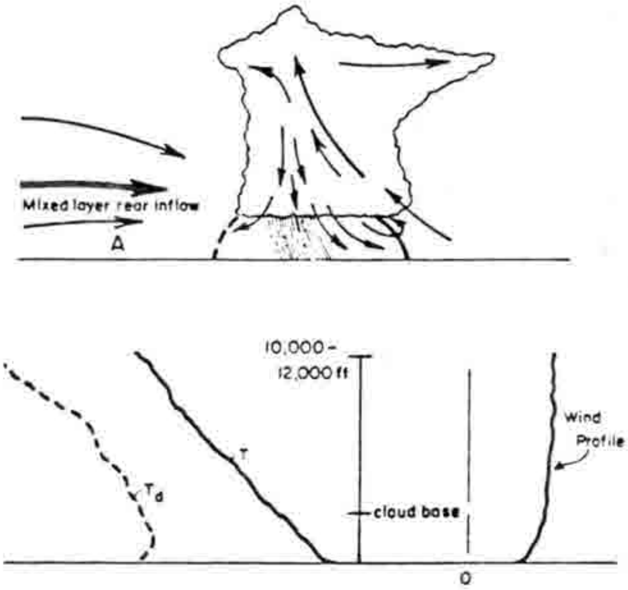

Fig. 18a Schematic illustrating early storm development, with a well mixed boundary layer to the rear of the storm at A.

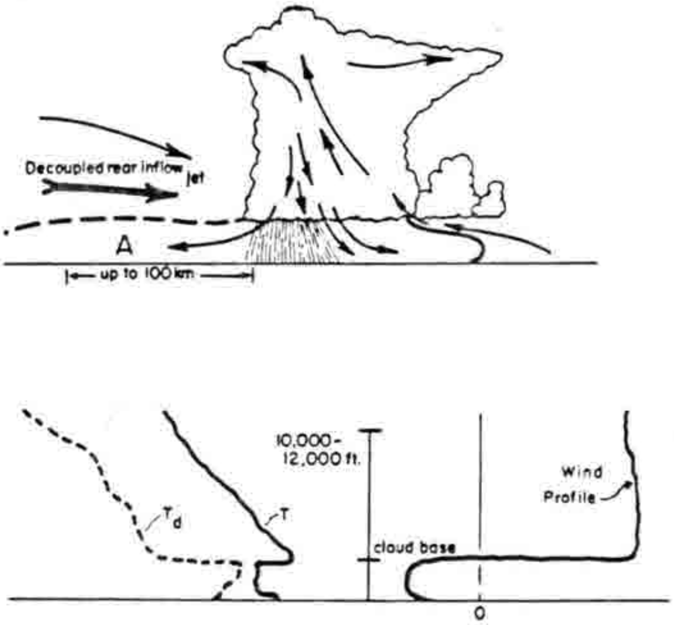

Fig. 18b Schematic illustrating later stage of storm development and the formation of a rear inflow jet due to the storm's modification of the boundary layer to its rear at A.

Large Time and Space Scale Studies (Climate)
Small Group Summary
Garrett Campbell

There are many new opportunities for climate measurements from the new AVHRR and GOES systems. These advances in research and operational products will require merging of several data streams for realization of their potential.

The two systems will provide big improvements in themselves: The 1.6 μm AVHRR channel will distinguish snow/cloud and aerosol properties. Also better soundings in cloudy areas will be possible, removing a bias in the current global atmospheric analysis. For climate studies the big improvement of GOES will be better calibration. The opportunities with full disc soundings many times a day may reveal new weather situations which are unknown with our present view.

We discussed the current state of the art for measurements of: 1. precipitation, 2. clouds, aerosols and their interaction, 3. trace constituents and their transports, 4. surface parameters of the land, ocean and cyrosphere. In all these areas there are new analysis methods which are being tested on extended data sets. But who will take the responsibility of long term implementation for climate monitoring? NOAA has this general responsibility with limited resources.

Most other new products will depend upon new processing strategies using multiple-channel, multi-instrument analysis. As an example, ISCCP (International Satellite Cloud Climatology Project) prepares cloud analyses by accumulating statistics for many days to estimate the clear sky situation. Then clouds are detected as different than clear. But this combines data at different times for GOES and AVHRR, a formidable problem with current data systems. Rainfall estimation is another problem which is now being attacked with empirical regression, but future systems will provide more accurate estimates and even direct observations (TRMM). NOAA has plans for derivation of many surface properties (see the attachment by Tarpley). This will require an improvement in the data and information system of NOAA.

To most efficiently take advantage of new ideas, NOAA will need to develop intimate contact with outside scientists. The external (and internal) researcher with an idea faces the barrier of accessing the data (ordering tapes, learning how to read them, etc.). If an algorithm is shown to be useful, NOAA faces the task of implementing it in the semi real-time system for climate analysis. For instance, Durkee has processed several years of AVHRR data to estimate atmospheric aerosol properties (thickness, amount..). Now NOAA is considering implementation of a real-time aerosol climatology. In an ideal world, the development and implementation could have occurred in one information system. The Earth Observing System of NASA is considering this potential in great detail with the probable commitment of a large of resources. Clearly NOAA will not be able to duplicate that type of effort, but any opportunities to move in that direction should prove very effective.

As with all climate monitoring efforts, long-term stability of products is required to measure climate fluctuations and potential climate changes. This involves instrument calibration and algorithm validation. We should not be afraid to go back an reprocess data if a better analysis method appears. Also, wide sampling of events at different locations are needed. For instance, soundings by geosynchronous satellites of the South Pacific will be needed even though they may have no impact on severe weather in the U.S.

Vonder Haar pointed out that these validation efforts often depend upon special observing projects like the First ISCCP Regional Experiment. NOAA will need to provide special data collection efforts for the preplanned events. Also, a high space and time resolution rolling archive of data would provide retrospective analysis of unusual or extreme events for further validation. It is also very important that results from these efforts get feedback into the operational algorithms in a timely manner. One should not have to wait years for the definitive analysis method to be published before it is implemented experimentally.

We must recognize that NOAA cannot do all these things alone. This will require a concerted effort on the part of the operational users as well as research users of climate data sets.

Campbell Attachment

OPERATIONAL SATELLITE DATA BASES WITH POTENTIAL FOR MONITORING SURFACE CLIMATE

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Beginning with the launch of NOAA-K in 1992, NOAA will have new sensors and products with potential for global climate monitoring. NOAA-K is the first of a new generation of polar orbiters with an Advanced MIcrowave Sounding Unit (AMSU) as the primary instrument. The AMSU is a 20 channel microwave radiometer designed to make atmospheric temperature and water vapor profiles and to provide data for detecting precipitation, sea ice, and snowcover. An advantage of the AMSU over infrared sounders is its ability to make measurements through cloud cover and so provide information from meteorologically active regions and areas with persistent cloudiness. Resolution of the AMSU is 50 km at nadir for 15 of the channels; for the other 5, the resolution is 15 km at nadir. Unlike previous microwave radiometers, the AMSU provides same size fields of view with coincident centers at the different frequencies. This will be advantageous for deriving products from multifrequency algorithms. disadvantages of the AMSU are the lack of polarization information and the varying nadir angles that characterize across track scanners. The AMSU will scan +50 degrees in nadir angle, providing global coverage twice per day from 2 satellites.

Another change on NOAA-K will be the channel configuration on the AVHRR. Channel 3 will be modified to switch between 1.6 μm during the daylight hours and 3.7 μm at night. The purpose of the 1.6 μm band is to better discriminate between clouds and snow. A possible modification to the AVHRR is the narrowing of Channel 2 from 0.7-1.1 μm to 0.85-0.89 μm.

The proposed list of NOAA operational surface products reflects the potential of the new sensor and bands. Table 1 contains the specifications of the proposed surface products. NOAA operational products are provided in response to requests from operational agencies within NOAA or other parts of the government. The coverage, frequency, resolution, etc., are determined by user requirements and by what is technically and operationally feasible. In recent years lack of resources has become an obstacle to developing and implementing new products at NOAA. Potential climate products will face similar limitations.

Products designed for climate monitoring are subject to different constraints than quantities derived for conventional weather or environmental purposes. Long term stability is more important for climate products because of the need to detect small, long term changes in environmental conditions. The highest possible accuracy is more important to conventional weather variables, such as precipitation, which may be used for flood or agricultural forecasts. Maintenance of maximum accuracy can require seasonal and geographical adjustments to algorithms that make climatological interpretation difficult. Vegetation index and global snow cover are products that are produced under climate-type constraints on stability. The reason is that monitoring year to year changes are important uses of these products. With a new sensor, such as the AMSU, where no prototype has been flown, it is very likely that the initial algorithms for deriving the products will not be the most accurate. A considerable number of changes and improvements are likely to be made during the first year or two of operations.

Observations of the land surface and cryosphere make use of measurements in window regions of the atmospheric spectrum. These windows are in the visible, near infrared, thermal infrared, and microwave regions of the spectrum. although variable surface features are of primary interest (snow, vegetation, etc.), nonvarying background features (surface type, roughness, elevation, etc.), may also contribute to the signal so geographic location of the data is important. For this reason the data are mapped before processing through the algorithms into surface products. Production of land surface and cryospheric products will be done from mapped data bases of window channel data from the AMSU and AVHRR. These data bases should be of great usefulness in global climate monitoring.

The global mapped data bases (Fig. 1) will be produced twice daily from the polar orbiter in the daytime orbit. It is not yet decided whether data from the twilight satellite will be mapped. The mapped data will consist of the window channels of the AMSU (24, 31, 50, 52, 89 and 157 GHz) and all of the AVHRR channels. The map projections will be polar stereographic submesh grids of the global NMC grid. The mapping will be at two resolutions to accommodate the different resolutions of the AMSU and AVHRR data. One map will be at 1/8 mesh, corresponding to a resolution of 26 to 52 km, and will contain the low resolution AMSU channels. The other will be at 1/16 mesh, with resolution of 13 to 26 km, and will contain the AVHRR data and the AMSU 89 and 157 GHz channels. The relation between the map cells in the low and high resolution maps is shown in the attached figure. One cell in the 1/8 mesh map will be exactly coincident with a 2x2 array in the 1/16 mesh grid. This will facilitate use of multispectral and multisensor data to retrieve surface products.

The data in the maps will be samples rather than area averages. Poleward of about 30 latitude the orbital swaths overlap. The insure that the data from the AVHRR and the AMSU are coincident in time and closest to nadir, the mapping procedure will save in the map the pixel with the smallest nadir angle.

When data are mapped information on observation geometry is lost. To preserve as much of this information as possible, ancillary information will be mapped along with the data. This information will include time, scan angle, satellite azimuth angle, and solar zenith angle. The mapping procedure will keep the angular data in each map cell that goes with the pixel in that cell. Thermal and microwave data will be calibrated with the operational calibration before mapping. visible data will be mapped as satellite counts. Under current procedures the visible sensors are note recalibrated after launch.

The mapped data bases are not official NOAA products, but are intermediate data bases for product generation. However, if the data bases were archived they would provide an excellent data source for climate products. The data volumes are tractable and the necessary information for climate algorithms (calibration, viewing geometry) will be saved with the data.

Table 1

NOAA K,L,M ... SURFACE PRODUCTS*

Quantity	Coverage	Frequency	Resolution
Precipitation - Rain/no rain - High - Medium - Low	Global	12 hours	46 & 23 km Polar stereo
Sea ice - Concentration - Type	Global	Weekly	46 km Polar stereo
Snow cover	Global	Weekly	23 km Polar stereo
Vegetation index	Global	Weekly	23 km Polar stereo

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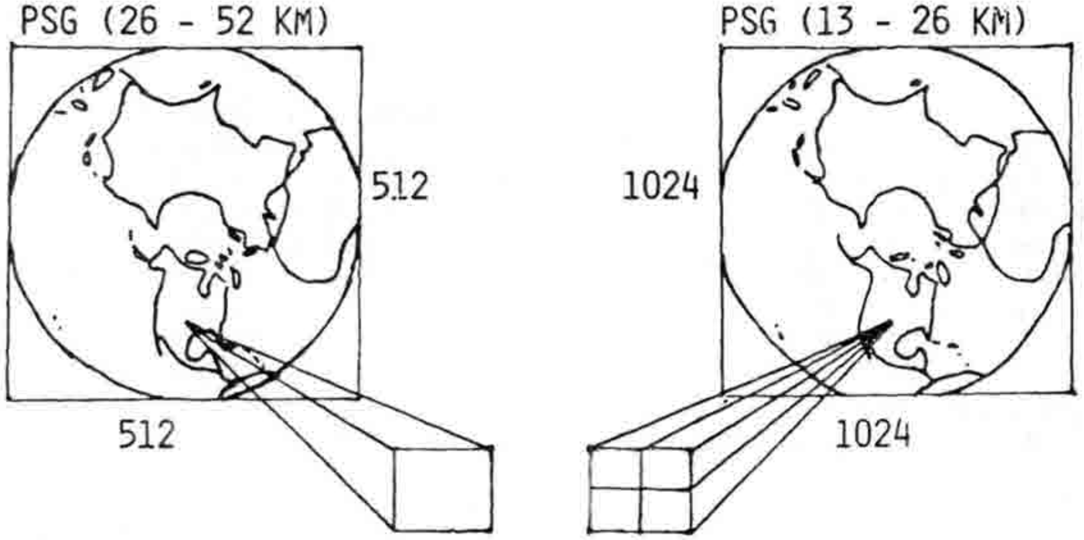
* Precipitation is included as a surface product because it is derived from observations in an atmospheric spectral window.

Figure 1

GLOBAL MAPPED DATA BASES (KLM)

(TWICE DAILY SNAPSHOTS OF THE WORLD)



AMSU

- 24 GHz
- 31 GHz
- 50 GHz
- 52 GHz
- 89 GHz

OTHER

- SOLAR ZEN ANGLE
- TIME
- SCAN ANGLE
- SAT AZIMUTH ANGLE

AVHRR

- ALL BANDS

AMSU

- 89 GHz
- 157 GHz

OTHER

- SOLAR ZEN ANGLE
- TIME
- SCAN ANGLE
- SAT AZIMUTH ANGLE

Synoptic Scale Phenomena
Small Group Summary
Alan Lipton

1. Information extraction

The new microwave sounding data should be particularly useful for diagnosis of tropical storms. We should also not neglect use of those new data and the other new satellite data for improvement of analyses of thickness fields, in particular with regard to polar lows. We should explore using satellite sounders on multiple satellites with different viewing angles to get sensitivity to atmospheric characteristics at lower levels in the atmosphere than we can get using a single set of weighting functions. It may be possible to use stereo retrieved cloud base heights to infer some characteristics of the boundary below cloud base. In doing analysis we should make use of what we know about how temperature, water vapor, and atmospheric winds interact in order to analyze each of those quantities. Given data that we have that cover each of those quantities. Retrievals of surface temperatures from the satellite can be important input for boundary layer modeling, and in turn boundary layer models might be helpful in providing vertical resolution to sounding products by specifying things such as inversion height. For determination of wind by tracking water vapor features we might see improvements by tracking features in water vapor retrievals instead of tracking features of the radiance fields. This might help reduce the ambiguity in height assignments of the winds. Pattern recognition techniques and other related techniques might be useful for inferring atmospheric characteristics from patterns in satellite imagery and motions that are found through examining satellite imagery. This kind of application could give information regarding cloud dynamics and microphysics. Data from new channels on the new imagers might be particularly useful for this type of application.

2. The need to retrieve more parameters from the satellite and make use of more retrievable parameters in numerical models

We now have cloud analyses from the Air Force's RTNEPH which may be useful for numerical modeling and apparently has not been used for that purpose. Not all of the cloud track winds that are currently available are being used in numerical models and there appears to be potential for improvements by using that data. Some work has been done on using ground surface temperature retrievals as forcing in mesoscale models. More work should be done on that. We now have information regarding things such as the location of ice edge which are very important for the formation of polar lows, that kind of information could be used to a greater degree. There are kinds of information that we need to retrieve from satellites and provide to the modelling community so that it can be used. Included among these things are cloud cover in terms of location and thickness, the location of snow cover, and the snow depth and soil moisture. To make full use of these kinds of parameters for mesoscale studies we will need better resolution than is available with the current generation of geostationary satellites and the new generation which should bring some improvement in detecting and using this information.

3. Communication between satellite data producers and numerical modelers

We learned from one particular modeler, Dr. Pielke, that we need to very wary about using satellite retrievals of air temperatures at scales down to around 10 km. We can only expect to use data on those scales when the signal that is retrievable is much greater than the error that we can expect in the retrieval. In most cases we would expect that we would not need data on those scales, in any case because the circulations are driven by the lower boundary or by large scale temperature features. A couple of problems that must be addressed in putting the satellite data to numerical models is to avoid aliasing problems, and we also must be careful that on the inputting the information from the satellites we are not disturbing model fields that are not resolvable with the satellite data. Much of the problem in putting the satellite data into numerical models stems from the fact that satellite data processors are generating products without necessarily knowing exactly what the modelers need and what they can use, and numerical modelers are not very familiar with the types of satellite data that are available and the problems and shortcomings of those data, and they are not necessarily aware of the greatest strengths of those data. It is necessary therefore to have much greater communication between satellite data producers and numerical modelers. However there are limits to that, in that we cannot have every numerical modeler also be an expert in radiative transfer, or maybe needed is close cooperation between the producers of satellite retrieved products and modelers so that we are not in a position of producers generating data and modelers trying to just put that data into their models. There has to be an interaction between the way atmospheric information is retrieved from satellite data and the way that information is introduced to the numerical model. Numerical modeling can be looked at as a three part process; first being the observations, second being data assimilation, and the third being actual modeling. More of our attention needs to be devoted to that second part, the data assimilation, that should be a substantial part of the effort devoted to numerical modeling. It is the responsibility of the satellite community to take the initiative in bringing closer communication between the satellite data producers and numerical modelers. It will be to our benefit if modelers are getting the things they need and use it and therefore are interested in getting more information from satellites.

4. Validation

We need independent validation of some parameters about which we do not have information now. One is soil moisture. Another is the spectral variation of ground surface emittance within infrared wavelengths. We might request that specific stations be set up specifically for validating satellite data, such that the types of observations that they make and the timing of their observations are set up to coordinate with satellite overpasses and satellite validation needs. Because the number of data sets that we will have available for validation is going to be quite limited, we might best off concentrating on doing high quality validations getting the right data for the validation instead of just a lot of data. Numerical models can be useful tool for validating the satellite data in terms of such parameters of cloud development, the satellite data can also be very useful in validating the numerical model. Changes that we see in the satellite data that are not seen in the numerical model simulation might be indicative of deficiencies in the model simulation.

5. Archival of data and accessibility of data

If we want all of the data from the new generation satellites to be archived, this is something we will have to be pushing for and we should explore alternatives for more efficient storage of those data including such things as optical disks. International cooperation might also be a way to make our archiving efficient. We need to have readily available quick look visuals to find our way through archive data in order to make it easier to identify promising data set. Data archives need to be much more user friendly to make it easier for people to get access to data. By making the data available to as many people as possible and by listening to their concerns, it will be easier for us in the long term to make the case for resources for satellites and data processing and related satellite activities. One aspect of user friendly and accessible archives is to keep together data sets that come from a single satellite but may be from different instruments, so that you can have access to data from all instruments on a given satellite without having to search far and wide. For quick look visuals video disk technology might be particularly useful. We should promote the use of satellite data in education at least at the college level and possibly even at the secondary level, to create greater awareness of what satellite data are and what they can be useful for, and in the long term bring greater demand for the data and greater support to the satellite community.

Synoptic/Sub-Synoptic Scale
Prepared by Alan Lipton
Session 2

1) Promising Phenomena for Study With New Systems

A) Tropical Cyclones

- Use microwave sounders to estimate the magnitude of the warm core and infer storm intensity.
- Use microwave imagery (SSM/I) to monitor precipitation locations and intensities.
- Estimate storm intensity with digital infrared imagery.
- Make more extensive use of wind data that we currently generate from tracking cloud and water vapor features. Also, improve our estimates of the mid-level steering winds with stereo imagery and new water vapor channels for better height specification.
- Use rapidscan data to look at short time scale changes in winds and clouds. This should help improve understanding of genesis and intensification. These data may also be used to study structure of the eye wall and rain bands.
- Track upper-level clouds to estimate outflow mass flux and thus infer intensification.

B) Polar Lows

- Geostationary data will be of limited use because horizontal resolution is poor at high latitudes.
- Should be able to estimate surface wind speed over the ocean using microwave scatterometers, sounders, and imagers.
- Microwave sounders should help monitor the effect of latent heating.
- Multispectral imagery (microwave and IR) should help us understand structure of cloud and precipitation.
- It would be useful to try animation of images from polar orbiters via cross-registration and remapping.

C) Mid-Latitude Storms

- Many of the approaches listed under A and B apply here too.

2) Modeling

We could readily pursue using cloud analyses from satellites (such as RTNEPH) in models. We should also go further in our methods of using cloud information and precipitation information. Research has been conducted on enhancing initial humidity, and altering initial vertical motion. We need to take account of the fact that clouds are a major source of energy for atmospheric motion. This is an issue of making the initial model fields consistent with our observations.

3) Data Set Combination

We need ready access to data from all the satellite instruments that are relevant to any given study. It may be best to store data by area and time rather than by sensor. We as a community must encourage development of systems to combine data sources. It was suggested that we establish an international center devoted to global analyses using satellite data exclusively.

4) We still need to look ahead to what comes beyond the next generation of satellites. Observation system simulation experiments (OSSEs) should be done to examine the usefulness of active radars and so on.

Nowcasting
Small Group Summary
Jan Behunek

The nowcasting small group discussed a wide range of issues related to the application of satellite data to nowcasting and short range forecasting. The focus of the discussion was on how satellite data can and should be used to address the scientific challenges being faced in these subdisciplines. The group recognized at the outset that satellite data provide a very important tool for nowcasting, but that those data alone are not sufficient to meet the challenge. Satellite data must be combined with data from other sources in order to generate the best analysis and prognosis of mesoscale weather. Furthermore, satellite data should be applied to aspects of the nowcasting task where they can be helpful. Those data occasionally have been applied inappropriately.

A particular application of satellite data favored by many workshop participants was for nowcasting support during field programs. Satellite data provide observations of atmospheric events on a wide range of spatial scales. The broad perspective supplied by satellite data can be used to analyze regions and scales of greatest interest, and to direct other observing resources toward those places. Satellite data also furnish valuable information regarding scale interaction in the atmosphere. One method that was suggested for increasing the use and availability of satellite data for research field programs is to include satellite meteorologists on the steering committees of those programs. Participants in this small group also believed that the boundary between operational and research meteorology would become less well-defined in the future, resulting in the execution of some research tasks in the operational environment. Thus, some advanced satellite data processing techniques and products must become available to scientists who are not satellite data experts. Those techniques should include retrieval of atmospheric soundings from multispectral data and the ability to perform animation of images.

Another important and recurring theme was the role of conceptual and numerical models in solving the nowcasting challenge. Efforts to provide numerical models with required initial and boundary data from satellites, as well as other information used during model execution, should be continued. Satellite data also furnish valuable information for conceptual models because of the unique perspective of the platform. Understanding of topics such as convective initiation, scale interactions, and boundary layer processes should be enhanced by incorporating satellite data into the observing strategy leading to the development of conceptual models. Use of satellite as a source of information for expert systems applied to meteorological questions could facilitate the development of those conceptual models.

The requirement to combine satellite data with data from other platforms in order to accomplish improved nowcasting and short range forecasting was another main topic in the small group discussion. Combination of data sets is important for validation of satellite analysis methods. Fused data sets also provide the most complete and accurate view of atmospheric conditions because each data set has individual strengths and weaknesses. For example, satellite rainfall estimation studies using data from visible, infrared, or microwave frequencies should consider the utilization of data from (Doppler) radar, lightning data, surface observations, and the futuristic Wotan system that will listen to raindrops falling on the ocean surface. Those data sets could serve as ground truth for the satellite analysis, or they could augment that analysis. Application of the boundary layer height determined from Doppler radar data also could improve the retrieval of other boundary layer parameters from satellite sounding data. Thermodynamic information from microwave vertical profilers and from satellite sounding data are complementary in that the profiler data are most accurate near the ground, whereas satellite data are more useful for analyzing conditions aloft. Fusing satellite data with data from other sources will require coordination with specialists in the acquisition and use of those other data sets. Attention also will have to be paid to the matching of temporal and spatial resolutions and collection strategies for the various data sources.

Characteristics of the satellite data themselves and techniques for processing the data generated some discussion separate from the consideration of meteorological objectives. Methods for improving satellite sounding retrievals was one of the topics considered. Research on improved retrieval techniques is needed in order to overcome some of the shortcomings of those sounding data. Although the signal-to-noise ratio of GOES-Next VAS data will be better than that provided by the current instrument, dramatic improvements in VAS data will not occur. Image processing techniques using 10-bit data and 8-bit display systems is another topic that should be addressed because many users of satellite data will be faced with that analysis task in the next decade. Careful study of other GOES-Next and future NOAA satellites data characteristics and collection strategies is needed in order to determine what else we need to do to prepare for the effective use of those data.

Several of the topics discussed in the nowcasting and short range forecasting group which already have been summarized are related to the future development and use of satellite data. Many other items pertaining to the future of meteorological satellites and their data also were considered. Data from the 3.9 µm GOES-VAS channel and the 1.6 µm, AMSU channel on future NOAA satellites should be examined to see what new information they contain regarding atmospheric processes. The 1.6 µm data could be particularly exciting for the analysis of multi-layered clouds and water versus ice phase detection. Cloud liquid water content estimates derived from satellite microwave data would be useful within the context of weather modification programs. New information contained in 30-second interval rapid scan data from GOES-Next could be applied to the reassessment of winds derived by cloud tracking and to the study of atmospheric gravity waves.

Better diagnosis of fog was one of the meteorological objectives that was mentioned on which satellite data could have a significant impact. The methodology for improved fog detection could employ data from spectral channels not previously applied to that problem and should utilize data from other sensors such as ASOS and celiometers. Another objective that was discussed is the use of satellite data for quantitative analysis of boundary layer processes leading to convective initiation. Satellite observations of cloud presence and optical depth would be valuable for diagnosis of solar heating, and satellite sounding data could trace boundary layer destabilization.

Future archival of GOES-Next data and access to all satellite data were topics of concern and discussion within the nowcasting group as a continuation of the large group session. The GOES-Next data archive was a particular concern because of recent suggestions within NESDIS that those data should be archived only at reduced spatial and temporal resolution. The nowcasting group was strongly opposed to that proposal. It is not clear at present how all types of satellite data will be accessed in the future. Data dissemination programs such as Unidata and NOAA-Port currently are being planned or are in progress. Although these programs may provide wider access to satellite data, they have not resolved all related questions yet.

Improved methods for extracting (especially) quantitative information from satellite data was one of the more important topics to be addressed in the future. A significant portion of past work with satellite data has been qualitative. This may be caused at least partly by the high content of pattern information in satellite data. More appropriate methods of quantitatively processing that pattern information are needed than those which currently are available.