Evidence of an Accumulation or “Big Drop” Zone

Graeme K. Mather, P.O. Box 1135, CloudQuest, Nelspruit, Republic of South Africa 1200

Abstract

On a pass through a vigorous cloud turret rising on the flank of a multicell storm, an accumulation or “big drop” zone was encountered. The zone was over 2 km wide and total cloud-water content averaged over the event was 22 g/kg.

The documentation of the event was made possible by new instrumentation on the Nelspruit Learjet that permits measurement of cloud-water contents that exceed adiabatic values.

1. Introduction

The possibility of an accumulation or “big drop” zone aloft (above the freezing level) and its implications in terms of hail growth, have been a source of speculation for decades (Kessler 1969; Browning 1977). The existence of such a zone is the cornerstone of the Soviet hail suppression hypothesis (Sulakvelidze 1974). Here, a clear case of an accumulation zone is reported, persisting over more than 2 km of a penetration through a cloud rising vigorously on the flanks of a multicell storm. New instrumentation on the Nelspruit Learjet convincingly measures the superadiabatic liquid-water contents that were found in the cloud.

2. Instrumentation

The instruments used in this case study have been described in a previous paper (Mather 1989). For convenience, a brief description of these instruments follows:

a) Radar flying range gate: The aircraft’s 3-cm radar is locked in a forward-facing position aligned along the flight path prior to cloud penetration. The return from a single range gate 1800 m ahead of the aircraft is recorded. This signal is suitably lagged during playback of the flight data to coincide with the other microphysical measurements.

b) Lyman-alpha engine vapor technique: This instrument is fully described in Morgan et al. (1989). Air is tapped off the eighth stage of the Learjet compressor. The water-vapor content of this air is measured using a Lyman-alpha hygrometer. This is a measurement of total cloud-water content, since it includes water vapor, cloud-liquid water, and the water stored in precipitation particles, all of which are evaporated by the jet engine compressors.

c) CSIRO-King liquid water probe: Although at Lear speeds this sensor saturates at around 4 g/m³, it has provided reliable measures of cloud water up to this limit. The probe also has a large droplet size limit. Response to liquid water contents with median-volume diameters of greater than 100 μm is significantly diminished (Biter et al. 1987).

3. Measurements

Figure 1a portrays selected time histories and Figure 1b some of the images from the laser cloud probe that were collected on this run. The two-dimensional (2-D) probe suffers from electrostatic interference initially, but recovers shortly after the 3-km distance marker. The event, over which averages are computed, is set to commence at 3.49 km into the run and end at 5.76 km. Table 1 is a summary of some of the means calculated over this event. The close correspondence between the 2-D probe and Lyman-alpha measurements of the water content of the precipitation particles is of great interest here. The Lyman-alpha estimate is obtained by subtracting the saturation mixing ratio (at the level of penetration) and the King measurement of liquid water from the Lyman-alpha total cloud water content measurement. (It should be noted here that the Lyman-alpha and King measurements of cloud liquid water content correspond closely when there are no particles larger than about 100 μm present and the King does not reach saturation.) The 2-D probe measurements are extracted from the images using a circle-fit procedure (Heymsfield and Parrish 1978) and the assumption that the particles are supercooled water drops.

The laser imaging probe on the Learjet is a Particle Measurement System’s 2D-C with a resolution (distance between diodes) of 35 μm. The images appearing in figure 1b have not been processed in any way (i.e., artifacts such as splasher and streakers removed, smoothing of drop images, etc.).
TABLE 1. Summary of cloud physics and environmental measurements.

<table>
<thead>
<tr>
<th>Measurement</th>
<th>Sensor</th>
<th>Average Value Over Event</th>
</tr>
</thead>
<tbody>
<tr>
<td>Cloud-water content</td>
<td>King</td>
<td>2.3 g/m³</td>
</tr>
<tr>
<td>Cloud water plus precipitation</td>
<td>Lyman-alpha</td>
<td>11.8 g/m³</td>
</tr>
<tr>
<td>Precipitation water content</td>
<td>(L-A) – (King)</td>
<td>9.5 g/m³</td>
</tr>
<tr>
<td>Precipitation water content</td>
<td>2-D probe</td>
<td>9.2 g/m³</td>
</tr>
<tr>
<td>Total cloud water</td>
<td>Lyman-alpha</td>
<td>22.0 g/Kg</td>
</tr>
<tr>
<td>Adiabatic limit</td>
<td>Sounding</td>
<td>15.4 g/Kg</td>
</tr>
<tr>
<td>Reflectivity</td>
<td>Aircraft radar</td>
<td>34.2 dBZ</td>
</tr>
<tr>
<td>Reflectivity</td>
<td>2-D probe</td>
<td>31.7 dBZ</td>
</tr>
<tr>
<td>Updraft speed</td>
<td>Angle of attack vane, rate gyro and accelerometer</td>
<td>9.2 m/s</td>
</tr>
<tr>
<td>Cloud-base temperature</td>
<td>Rosemount probe</td>
<td>17.3 °C</td>
</tr>
</tbody>
</table>

Fig. 1. (a) (left) Time histories recorded during a penetration through an accumulation or “big drop” zone. Time histories from the top of the figure down are: 1) static temperature, 2) true vertical-gust velocities, 3) King and FSSP cloud-liquid water, 4) equivalent radar and calculated 2D-C reflectivities, 5) 2D-C concentrations, 6) mean diameters, and 7) total cloud water measured by the Lyman-alpha humidiometer. (b) (above) Drop images from the 2-D particle size spectrometer recorded over the event marked in (a). The vertical dimension of the time bars separating the images is 1.12 mm. The ragged images (e.g., line 4 and 5, the end of line 7 and the beginning of line 8, the end of line 12 and the beginning of line 13) are produced by splashing drops and are rejected as artifacts before computing liquid-water contents and reflectivities.
The spikes in the Lyman-alpha time history are presumably caused by the extremely large drops that are encountered on this penetration. The average equivalent radar reflectivity calculated from the 2-D images (assuming water drops) underestimates the reflectivity recorded from the aircraft radar by about 2.5 dB. This is not surprising since drops over 1 mm in diameter are truncated to this size for the 2-D reflectivity computations (see Mather 1989). No such truncation takes place when calculating precipitation water contents.

The calculated updraft speed across this event is 9.2 m/s which is capable of supporting drops of about 6 mm in diameter.

4. Discussion

Given warm cloud bases and sufficient buoyancy, an active condensation-coalescence mechanism exists in convection clouds in the eastern Transvaal. This is revealed by the appearance of large supercooled water drops at around the −10°C level in these clouds. From this observation to the development of accumulation zones would merely require the right updraft profile.

Of what significance are such accumulation zones to the production of rain and, in particular, hail? This will, of course, depend upon their frequency and volume. It is not surprising that they are encountered so seldom by instrumented aircraft, given the sampling limitations of these systems (both in time and space). Good radar data should be able to detect such zones as enclosed volumes of high reflectivity aloft (>50 dBZ).

Acknowledgments. This research is fully supported by the Water Research Commission, Pretoria, South Africa.

References

A Historical and Personal Perspective of Model Evaluations in Meteorology

Abstract

Methods for scoring and evaluating weather forecasts have been used for more than a century. The early efforts were focused on verifying categorical forecasts, such as the prediction of temperature, precipitation, and tornado occurrences. Modern concepts of meteorology were developed a half-century ago, with the introduction of air-mass analysis by the Norwegian school, along with new theoretical ideas and operational weather-forecasting tools. Among those developing and using these new concepts were such pioneers as Rossby, Byers, Wexler, Namias, and Starr—working in close contact with the newly established Extended Forecast Division of the United States Weather Bureau. The need to test some of these theories and evaluate some of the new tools of the forecaster was soon evident: i.e., prediction of meteorological fields, sea-level prognostic charts, and 10 000-ft pressure patterns. Initially, the “model fields” were prepared subjectively by hand. Development of quantitative and objective model fields followed, based on the work of Charney, Von Neuman, and others, working with the Princeton group. Model-field development led to modern numerical weather-prediction (NWP) models, which went into operational practice. This paper presents some of the significant historical events leading up to modern developments in the forecasting and evaluation of model-generated fields.

1. A personal and historical perspective

If this joint Conference on Probability and Statistics with Weather and Analysis and Forecasting was being held a year later, I would be able to say, literally, that the events I'll be relating about the early history of modern meteorology in America took place a half-century ago. I consider it an honor to have been able to participate in some of the early events, and a privilege to have been associated with pioneers such as Rossby, Byers, Willett, Starr, Namias, and others.

What were some of the problems, especially in regard to verification and evaluation of the new and developing concepts in weather theory and practice? How did we, as statisticians, get involved, and what, if any, contributions did we make? In discussing the relationship between statistics and meteorology, it is necessary to go back about a century and introduce two questions: The first is, Who prepared or drew the first weather map, and coined the term “anticyclone?” We are all familiar with the term “regression,” as it is used in correlation analysis. So, the next question, directed primarily at the statisticians, is, Who proposed the term “regression” used in statistical analysis? The answer to both questions is Sir Francis Galton. This bit of history can be found in Shaw's Manual of Meteorology.

In 1939 Charles F. Sarle, an economic statistician, was a high official in the Department of Agriculture. Dr. Sarle (along with Henry Wallace) was interested in weather, especially in the need for, and possibility of long-range forecasting. He was responsible for the disbursement of the Bankhead-Jones Research Funds which were, at that time, supporting a small research project at the Massachusetts Institute of Technology (MIT), where one of the first departments of Meteorology had just been established. Members of this research group included, among others, Hurd, Willett, Harry Wexler, Jerome Namias, and Roger Allen. Working closely with the University of Chicago and the United States Weather Bureau (USWB [then under the auspices of the Department of Commerce]), Sarle worked out a plan to set up a new group for teaching and research, to be located in Chicago. Due to statistical problems arising in long-range weather forecasting, Sarle wanted to include a statistician as one of the senior members of the research team. He chose Horace W. Norton, a young mathematical statistician who had just finished his graduate work under R.A. Fisher, in London. But Sarle was reluctant to send a young, inexperienced statistician out alone “among the meteorological wolves” and looked for a junior member to assist the senior statistician. My name was suggested to him by a mutual friend. During my interview with Sarle, he discussed the work taking place at MIT, and the proposed plans at the University of Chicago. I asked him whether the MIT group was the same as the one Roger Allen was associated with. I had known Allen since school days, back in Iowa, and had kept in touch with him. In another year, I was sitting across the desk from Allen as a member of the Extended Forecast Section, which had just been established in the USWB, with Namias as head. I had been transferred from Chicago, and Harry Wexler had come to the central office of the


© 1990 American Meteorological Society

Bulletin American Meteorological Society
USWB. He became an officer in the Air Force at the beginning of World War II (WW II).

Air-mass analysis and other concepts of the Norwegian school had just been introduced in the USWB. Some of these concepts were being put into daily practice with experimental forecasts of weather elements, sea-level prognostic charts, and 10 000-ft pressure patterns. There was considerable interest in verification, not only for testing the validity of the new concepts but for the purpose of comparing the skill of different forecasters or competing forecast groups using different methods. There was no generally accepted "best" way to evaluate forecasts, and examination of the literature disclosed that this topic had been a very controversial one since the establishment of the weather service near the end of the nineteenth century. I remember reading a comment somewhere to the effect that the way to evaluate a forecast is not to compare the forecast with the observation, but to determine whether it was the proper forecast to have been made at the time. Also, a forecaster once told me that when a forecast turns out well, it represents "physical understanding," but if it turns out wrong, then it's just "bad luck."

The methods used for preparing forecasts of meteorological fields (e.g., sea-level prognostic charts and 10 000 foot pressure patterns) were subjective and prepared by hand. The objective and quantitative prediction of pressure patterns and geopotential fields came much later, of course, with the development of numerical prediction methods. Some early attempts were made to use new theories, such as the movement of "troughs" and "ridges" based on Rossby waves. Some years later, in searching for some historical upper-air data, I ran across a series of observed charts that had been used to test the Rossby ideas on trough movement. I was impressed with two things: 1) the agreement between the trough movements and the theory, and 2) the number of erasures and reanalyses of the charts. In those days, there was no such thing as objective analysis, as data were very sparse, and even aesthetic consideration could have an effect on how lines were drawn. I remember a story that Jerry Namias once told me about an experience he had when drawing an isotropic chart, with his wife, Edith (an artist), looking over his shoulder. She suggested that the analysis would look better if he redrew a few of the curved lines. After reexamining the plotted data, he decided she was right, and he made the changes.

Early in WW II, a number of weather officers of the Air Force were assigned to the Pentagon to work on numerous problems in climatology, forecasting, and verification. Ken Spengler, Kenneth Arrow, and Charles Stein, were among those assigned. We all know what happened to Ken Spengler, and where he is today. Stein became a well-known mathematician, frequently published in statistical literature. Kenneth Arrow went into the field of economics, where he won a Nobel Prize. Recently, Spengler told me that when he called Arrow to congratulate him after receiving the award, Arrow pointed out that his first published paper was in meteorology.

George Wadsworth, Joe Bryan, and Albert Bowker, three civilian statisticians, worked with this group on long-range forecasting research. Wadsworth and Bryan wrote a well-known book on statistics. Bowker eventually became chancellor at Berkeley. During this period, prognostic sea-level charts and 10 000-ft pressure prognostic charts were prepared on a routine basis by the Extended Forecast Section, as well as by several other groups connected with the combined USWB/Air Force-Navy Weather Central. A committee composed of several Air Force weather officers scored these forecasts on the basis of their subjective judgments and gave relative rankings to the forecasters. After several months, average scores were computed, and the forecasts prepared by the group headed by Namais had the top rating.

The subjective evaluation of prognostic charts had a number of difficulties, and an effort was made to find a more objective method. The idea occurred to me to use the observed and predicted values at selected grid points on the map, and obtain a measure (or measures) of agreement by computing a correlation coefficient, root mean square error, etc. A copy of a report I prepared (c. 1942) shows an example using a grid of 40 points for the United States and Canada. One paragraph reads as follows

The reading and entering of the observed and forecast values of the pressures takes about 10 min when a celluloid template is used on the maps. An experienced clerk using a modern calculating machine can perform the complete verification of Table 1 in about 20 min if the shortcut machine methods are used. However, if no machines are used, the computations might take three or four times longer than this, depending, of course, on the individual's speed in arithmetic.

How things have changed!

It was recognized at that time that the correlation coefficient (or RMSE) was not the complete answer to the verification problem. Murphy and Epstein (1988) have recently discussed skill scores and correlation coefficients in model verification.

I have been asked whether I participated in the development or evaluation of the numerical weather-prediction models in the USWB program. My only contact with the group at Princeton was when Wexler, Namias, Joe Fulks, Colonel Ben Holzman, and I traveled to New Jersey by train to present the results
of a statistical analysis on the “Langmuir periodicities.” This was in the early 1950s. Irving Langmuir, the Nobel Prize–winning scientist, had conducted some silver iodide cloud-seeding experiments in New Mexico, on a periodic basis, for a number of months. In following the daily weather maps, he noticed that widespread precipitation, over most of the central and eastern United States, seemed to correspond to the same pattern as the seeding cycle—a 7-day periodicity. Similar variations were found in surface- and upper-air pressure indices. He claimed that these weather periodicities were the result of the cloud seeding, and suggested that, because we cannot forecast the weather very well, it might be easier to control it than to predict it. (Now, we know the reverse is true: weather has to be predicted before we can know whether we have controlled it.)

The purpose of the trip to Princeton was to present to the research group some preliminary results of the statistical analysis I had performed on the data relevant to the Langmuir claims. Among those present were Charney, Rossby, Von Neuman, and John Tukey, a young mathematical statistician. For the statistical analysis, I used a method proposed by Kendall (1945) for the analysis of oscillatory time series. After my presentation, Tukey took the floor and described what he claimed to be a much better method for analyzing and/or testing for periodicities. His description of the method was very abstract and mathematical, and nobody had any idea what he was talking about (with the possible exception of Von Neuman). As it turned out, Tukey was describing spectrum analysis, which now is well understood and widely used by scientists in many fields. I recall this event in connection with a visit I once had with Gertrude Cox, former head of the Statistics Department at the North Carolina State University. During the course of our conversation, Tukey’s name came up. “What John needs is to get some experience with some real world data,” she remarked. Again, how things change! Now, we are all acquainted with the concepts of exploratory data analysis and the data-based tests and procedures advocated by Tukey.

References

SNOWSTORMS ALONG THE NORTHEASTERN COAST OF THE UNITED STATES: 1955 - 1985

by Paul J. Kocin and Louis W. Uccellini

The authors, NASA/Goddard Space Flight Center based meteorologists, provide the evolution and analysis of the structure of 20 of the most crippling snowstorms to affect the heavily populated Northeast region of the United States over a 30-year period. The events are examined from historical, climatological, and dynamical perspectives — how storms develop, what weather patterns provide clues, what factors delineate snow/no snow situations. An excellent reference for students, researchers and forecasters. This is volume 44 in the AMS Meteorological Monograph Series.

Cloth $35/List - $27.50/AMS Members

BLIZZARD! THE GREAT STORM OF ‘88
by Judd Caplovich edited by Wayne W. Westbrook
ISBN 0-9619282-0-4
$24.95

There is a $3.00 shipping and handling charge per item.
To order either of these publications, please contact

American Meteorological Society
45 Beacon Street
Boston, MA 02108
617-227-2425

COMING SOON...

The Blue Hill Meteorological Observatory:
The First 100 Years, 1885–1985

by John H. Conover

This account of the early developments in meteorological research brings to life the struggles of the young pioneers—the trials and tribulations in developing new instruments, the difficulty in sampling the atmosphere under challenging conditions—and adds to the already rich heritage of meteorological literature. All the “firsts” are documented, allowing contemporary meteorologists the chance to experience the traditions and practices that otherwise may have been lost. Photographs.

ISBN: 0-933876-89-0 528 pp. (approx.) $55.00/List-AMS Members/ $35.00*
Spring 1990

ALSO AVAILABLE FROM AMS....


Order your copies today by sending in prepayment to: AMS, 45 Beacon St., Boston, MA 02108. (*Please include $3.00 shipping and handling for each book)

Name_____________________________________
Shipping Address_____________________________________
City/State/ZIP______________________________

Vol. 71, No. 3, March 1990