LETTERS TO THE EDITOR

Modification of polar air over water

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It may be of interest to review the article by C. F. Burke on "Transformation of polar continental air to polar maritime air" in the June 1945 issue of the JOURNAL on the basis of an investigation of a similar problem conducted independently at the AAF Weather Station, Massachusetts Institute of Technology, by R. B. Espy, A. J. Palladino, and myself during the winter of 1944–45.[1] Our results support the following assumptions made by Burke:

1. He assumes that the lapse rate over the ocean during the transformation of cP to mP air is dry adiabatic from a height of 15 meters to the condensation level and then saturation adiabatic up to the limit of temperature convection. This vertical temperature distribution closely resembles that found in our study of 54 ship soundings during polar-air outbreaks over the North Atlantic and also that found by the British Meteorological Office during polar-air outbreaks over the North Sea.[2]

2. In Burke's forecast of surface air temperature, the final value of the sea-surface temperature (that is, the temperature at the end-point of the trajectory) is used in place of the average sea-surface temperature when the trajectory over water is long (greater than 600 km). In our analysis of 77 Atlantic ship reports of surface air temperature during outbreaks of polar air with long trajectories over water, the final sea-surface temperature was similarly found to be more important than the mean sea temperature in determining final air temperature.

3. Burke assumed a surface relative humidity of 80 per cent as typical of cP air over the sea. This compares favorably with the average of 75 per cent found in our study of 100 Atlantic ship humidity reports.

On the other hand, our findings are not in accord with Burke's in the following particulars:

1. He assumed constant specific humidity in the dry adiabatic layer and therefore suggested that the average surface relative humidity of 80 per cent be used in forecasting the condensation level. Theoretically, specific humidity should be constant throughout a *thoroughly mixed* dry adiabatic layer.[3] However, analysis of the ship soundings at our disposal showed that in almost every case specific humidity decreased with elevation in the dry adiabatic layer. As a result, the condensation level calculated from the surface relative humidity averaged about 1,000 feet lower than the actual condensation level, the point where the lapse rate was observed to change from dry to moist adiabatic. To account for this difference, it was necessary to assume an average surface relative humidity of 62 per cent, 13 per cent less than that actually observed. The upward gradient of specific humidity implied by these results can be attributed to continual evaporation of moisture from the ocean surface to the overlying colder air. This prevents the attainment of a thoroughly mixed equilibrium state until the cP air has been completely transformed to mP air.

2. Burke developed a method of forecasting the surface air temperature at any point over the ocean in terms of four variables: initial surface air temperature, initial lapse rate, distance of over-water trajectory, and sea-surface temperature. In our investigation, where the air traveled over the ocean for more than 400 miles in 88 per cent of the cases, only the last of these was found to be significant. It was possible to replace the first two by a single factor, the initial free air temperature at 5,000 feet, because the adiabatic layer due to turbulence and surface heating nearly always extended up to the 5,000-foot level at the great distances used in our study. In addition, the effect of distance of over-water trajectory was found to be negligible in our temperature forecasting, apparently because heat gained from the sea had to be distributed over a deep turbulent layer (average 7,000 feet). We were able to forecast surface air temperature over the ocean almost as accurately as Burke by means of a simple empirical expression approximately equal to four fifths of the final sea-surface temperature plus one fifth of the initial 5,000-foot air temperature (all temperatures in °C).

3. Finally, it should be noted that in constructing his "final" soundings over the ocean Burke neglected the important effects of vertical motion due to horizontal convergence and divergence. He assumed that the cloud top would be located at the level where the saturation adiabat from the condensation level intersected the original land sounding. In many of the soundings we studied, however, it was apparent that the lapse rate at the cloud-top level had been considerably modified by vertical motion during the trajectory from land to ship, so that the simple unmodified assumption of advection was untenable. We were able to reduce the error of our cloud-top forecasts by an average of 900 feet by applying an approximate correction for horizontal convergence and divergence. Undoubtedly, a thorough solution of the problem of vertical motion is necessary before complete accuracy of cloud forecasts can be attained.

[1] AAF Weather Station, Massachusetts Institute of Technology, "Forecasting tops of non-frontal cumulus clouds in polar air outbreaks over the North Atlantic," Weather Central Division Report No. 971, 1945.
[2] Air Ministry, Meteorological Office, "Notes for the guidance of forecasters on the subject of forecasts of ice accretion on aircraft," *M.O.M.* 393, 1943 (U. S. Navy Reprint, NAVAER 50-IR-80, 1944).
[3] Sverre Petterssen, *Weather analysis and forecasting*, New York, McGraw-Hill Book Company, 1940, pp. 93–94.

Radar and weather

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Several comments are here submitted with regard to the article "Radar and weather" by Commander R. H. Maynard in the December 1945 issue of the JOURNAL OF METEOROLOGY.

(a) A typographic error occurs in the footnote of page 215. According to standard usage, "S" band is in the vicinity of 10 cm and "X" band, 3 cm.

(b) Extensive studies and tests of the relative performance of S and X band radars during the past two years by personnel assigned to the Signal Corps Engineering Laboratories have shown that, other factors remaining equal, X band is superior to S band except for rare cases where attenuation due to rain becomes severe on X band. Cases of moderate to strong attenuation of X band have been observed along very intense squall lines, or with severe thunderstorms. In such cases the superiority of S band was of short duration (5–10 min). It may be shown from theoretical considerations that the echo power from clouds at close ranges is inversely proportional to the fourth power of the wavelength. Because of this, X band realizes an advantage in received power of the order of 80 times. This is, however, somewhat reduced by larger atmospheric attenuation factors. A further advantage of X band is the greater definition obtainable since the antenna beam width is proportional to wavelength.

(c) In Figure 4, it is stated that "the radar beam is producing echoes from a precipitating cloud portion between 9,000 and 11,000 feet." Since the nearest and farthest echoes are approximately 3 and 13 miles respectively, this implies that the highest and lowest portions of the beam would have to be at angles of about 30 and 8 degrees respectively. It is improbable that a practical microwave radar would have a 22 degree beam width. It is assumed that either a computational error was made or else the heights quoted were not determined by use of the radar.

(d) By comparing Figures 2 and 3, when the front is 22 miles NW of the station and directly at the station, respectively, the writer concludes that the front has intensified. Similarly, when the front has passed on to 25 miles SE of the station (Fig. 6), he states that the precipitating clouds show decreasing frontal activity. It is considered that such conclusions are of doubtful validity when made on the basis of PPI observations, in view of the fact that the power of radar echoes from clouds and rain is inversely proportional to the square of the range at short ranges, grading to the fourth power at long ranges. It is conceivable, however, that a well calibrated radar could determine the relative reflectivity of activity within a factor of two or four. No such technique is known to be in current use.

Reply

By R. H. MAYNARD, Commander, U.S.N.
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10 July 1946

With respect to the particular comments of Mr. Wexler and Lt. Swingle's letter to the editor, I offer the following:

(a) Because of security reasons my paper delivered at the joint AMS–IAS Meeting in New York in January avoided any mention of radar wavelength associated with terminology such as S-band and X-band. The footnote on page 215 was added by the Editorial Board, I presume, of the JOURNAL OF METEOROLOGY. It is agreed that there is a typographical error in this footnote.

(b) The comment in regard to the relative performance of S- and X-band radars as noted by Signal Corps Engineering Laboratory personnel is very interesting. In connection with discussion comparing the performances of S- and X-band radars in storm detection work, I wish to point out that the article published by the JOURNAL OF METEOROLOGY on "Radar and weather" was but a part of the total effort of the article presented in New York. The original complete article contains what I believe to be a most interesting photographic comparison of radar scopes of simultaneous pictures of a hurricane passing near Naval Air Station, Corpus Christi, in 1945. The simultaneous pictures were made utilizing scopes of S- and X-band radars and bear out the pertinent thoughts in Mr. Wexler's comments.

(c) With regard to the comment on the 22 degree beamwidth of the radar from which Figure 4 was made, the heights quoted were not determined by the use of the radar but were determined by visual observation of aircraft operating from Naval Air Station, Lakehurst.

(d) Although the radar at Naval Air Station, Lakehurst, was well calibrated and was considered nearly ideal for the type of work undertaken in storm detection, it is agreed that the straight conclusions made from Figures 2 and 3 can be of doubtful validity when made solely on the basis of PPI observations. Again, however, the PPI observations were checked by aircraft operating for this specific purpose of visual observation and such conclusions were made purely on the basis indicated.

The comments made by Mr. Wexler and Lt. Swingle are worthy ones. Their interest and original work have been most thorough and searching in the field of radar and weather in their work at the Army Signal Laboratory. They are indeed to be considered among the pioneers in radar and meteorology who were mentioned in the editor's note preceding my article.