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The use of probabilities in forecasts of maximum and minimum temperatures*

By Robert L. Winkler

(Graduate School of Business, Indiana University, Bloomington, Indiana 47401)

and Allan H. Murphy

(National Center for Atmospheric Research†, Boulder, Colorado 80307
and
Department of Atmospheric Sciences, Oregon State University, Corvallis, Oregon 97331)

Summary

The inclusion of probabilities in weather forecasts provides a means of quantifying the uncertainty inherent in such forecasts as well as potentially useful information not available in traditional categorical forecasts. Subjective probabilistic temperature forecasting is studied in this paper. Alternative summary measures of a forecaster's probability distribution of temperature are considered, and 'credible intervals' are suggested as a suitable choice for operational forecasts of this continuous variable. A credible interval temperature forecast is an interval of temperature values accompanied by a probability that expresses the forecaster's degree of belief that the temperature will actually fall in the interval. An experiment involving the formulation of variable-width and fixed-width credible interval forecasts of maximum and minimum temperatures by U.S. National Weather Service forecasters is discussed. The experimental results indicate that experienced forecasters can formulate reliable and skilful credible interval temperature forecasts. Reliability is measured in terms of the correspondence between the probabilities associated with the forecasters' intervals and the observed relative frequencies of temperatures, whereas skill is determined by comparing the precision (or accuracy) of the credible interval forecasts with that of forecasts based on standards of comparison such as climatology and persistence. It is important to note that these successful probabilistic temperature forecasts were prepared without the aid of objective probabilistic guidance information. Some implications of these results for operational procedures and practices in temperature forecasting are considered.

1. Introduction

Weather forecasts are traditionally expressed in categorical (i.e. deterministic) terms. However, it is widely recognized and generally acknowledged that an element of uncertainty exists in almost all such forecasts. At present, when uncertainty is mentioned in a forecast, it is most often described by means of one or more verbal qualifiers (e.g. possible, likely, occasional, frequent). However, studies of the

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understanding of such terms on the part of recipients of the forecasts indicate that these terms are subject to very wide ranges of interpretations (and misinterpretations). Such misinterpretations substantially degrade the quality and value of the information contained in many forecasts.

The language of probability provides an economical and unambiguous means of describing the uncertainty inherent in weather forecasts. In an effort to provide more precise and useful information concerning the occurrence of precipitation, U.S. National Weather Service (NWS) forecasters have expressed their forecasts of precipitation occurrence in probabilistic terms on an operational basis since 1965. Specifically, these probability of precipitation (PoP) forecasts represent the likelihood of occurrence of measurable ($\geqslant$0·01 inch) precipitation at a particular point during a specific period (generally 6 or 12 hours). For example, a typical PoP forecast for Denver, Colorado during the summer might state, 'the precipitation probability today is 20 per cent'. These probability forecasts initially encountered some resistance on the part of both forecasters and the general public, but they are now considered to be an important and integral part of public weather forecasts in the U.S.A. Moreover, extensive evaluations of these forecasts demonstrate convincingly that weather forecasters can quantify the uncertainty in forecasts of precipitation occurrence in a reliable and skilful manner.

In contrast to their forecasts of precipitation occurrence, NWS forecasters still express forecasts of maximum and minimum temperatures in categorical terms. More specifically, these forecasts generally are expressed in terms of an interval of temperature values. For example, 'the maximum temperature in Denver today will be between 85 and 90 °F' (all temperatures in this paper will be expressed in °F). However, the forecasters do not attempt to assign a probability to such an interval. Thus, the recipient of the forecast does not know whether the probability is 90 per cent that the temperature will fall in the interval or whether the chances are even. Recently, the inclusion of probabilities in temperature forecasts has been investigated in a few experiments (Peterson, Snapper, and Murphy, 1972; Sanders, 1973; Murphy and Winkler, 1974; Bosart, 1975; Gregg, 1977). The results of these experiments indicate that probabilistic temperature forecasting is quite promising in the sense that such probabilities provide information that should be valuable to many users of temperature forecasts (for example see Murphy and Winkler, 1979). Thus, further study of this potentially important area appeared to be warranted, and an experiment was designed and conducted in the NWS Forecast Office (WSFO) in Milwaukee, Wisconsin in order to obtain additional information from which to make inferences concerning probabilistic temperature forecasting.

The purposes of this paper are to discuss probabilistic temperature forecasting, to present some experimental results involving probabilistic forecasts of maximum and minimum temperatures, and to examine the implications of these results for operational weather forecasting. In section 2, some lternative formats for the expression of uncertainty in temperature forecasts are compared, and 'credible intervals' are suggested as a suitable choice for operational forecasts. Section 3 contains a description of the Milwaukee experiment, together with a presentation of some results from this experiment. Section 4 consists of a brief summary and a discussion of implications of the results of the Milwaukee and other similar experiments for operational temperature forecasting.

2. Probabilistic temperature forecasts

In order to consider probabilistic temperature forecasts, we first need to define the events of interest. It will be assumed that a maximum or minimum temperature forecast pertains to the high or low temperature recorded during a specific period of time (the forecast period) at a particular point (generally the local NWS office). Thus, the uncertainty in question relates strictly to the uncertainty about the temperature at the given point during the forecast period. It does not, for example, relate to spatial variability in temperature (e.g. the interpretation of an interval forecast as a range of temperatures for different points in the forecast area). This variability is an interesting and important but separate issue.

In contrast to precipitation occurrence, which is a dichotomous event, maximum and minimum temperatures are continuous variables. Thus, whereas a single probability is sufficient to represent formally a forecaster's uncertainty concerning precipitation occurrence, a complete description of uncertainty in forecasting temperature requires an entire probability distribution. Ideally, then, a probabilistic temperature forecast would consist of a probability distribution for a continuous variable, i.e. maximum or minimum temperature, and various techniques are available for assessing such a distribution (for example see Winkler, 1967; Hampton, Moore, and Thomas, 1973). Fractiles, cumulative probabilities, probabilities for intervals, probability densities, and summary measures are among the types of assessment that could be considered.

For important decisions in which uncertainty about temperature plays a key role, the time and effort required to assess an entire distribution may be worth while. On an operational basis, however, the consideration of an entire probability distribution is not practicable as regards either the time required of the forecaster or reporting to the general public. It is desirable to keep a temperature forecast (and forecasts of other weather elements as well) relatively simple to make the forecast understandable to its recipients and minimize the amount of time required to communicate the forecast. Instead of an entire distribution, then, a temperature forecast consisting of some summary measures from that distribution might be preferable.

'Summary measures' such as moments of a probability distribution provide useful information, but they are difficult to assess directly (thereby returning us to the assessment of an entire distribution) and would not, for the most part, be meaningful to the public. Intervals, on the other hand, seem feasible both in terms of assessment and interpretation. As noted previously, temperature forecasts in the U.S. are now expressed frequently in the form of intervals, but probabilities are not included in these forecasts. A credible interval temperature forecast, which consists of an interval of temperature values accompanied by a probability (e.g. 'the probability is 0·80 that the high temperature today will be between 66 and 70 °F'), represents a straightforward extension of the interval forecasts often used in current temperature forecasting practice. Credible intervals provide some probabilistic information without necessitating the assessment of an entire distribution, and a credible interval temperature forecast should be no more difficult to communicate effectively than a precipitation probability forecast.

The choice of credible intervals as the mode of expression for probabilistic temperature forecasts still allows some flexibility in the selection of a particular interval to be included in a forecast. In fact, the forecaster could be given complete freedom in the choice of an appropriate credible interval for a particular situation. A more systematic approach, however, might aid the forecaster by simplifying the assessment task. Moreover, if the same types of credible interval forecasts are issued on each occasion, then recipients of the forecasts should find them easier to interpret. Thus, certain restrictions on these interval forecasts seem desirable.

A restriction that is consistent with common practice in interval estimation in statistics is to predetermine the probability associated with the interval. For example, the forecast might always consist of a 50 per cent credible interval or a 75 per cent credible interval (or both). Sometimes a 50 (or 75) per cent credible interval for high or low temperature will be only 4° wide, whereas at other times such an interval may be 8° wide. Since the probability of the interval is fixed but the width of the interval will vary from occasion to occasion, such a forecast will be called a variable-width credible interval. Furthermore, the use of credible intervals that are central in terms of probability (i.e. the probability that the observed value falls below the interval equals the probability that the value falls above the interval) seems reasonable, so that all variable-width intervals considered in this paper will be central credible intervals.

An alternative to variable-width forecasts involves a restriction that predetermines the width of the interval but allows the forecaster to vary the probability associated with the interval. For example, the forecast might always consist of a credible interval that is exactly 5° or 9° wide (or both). In some cases the probability of such an interval might be 0·50, whereas in other cases it might be 0·90. Such a forecast will be called a fixed-width interval. Additional restrictions can be placed on fixed-width credible intervals, and in this paper all such intervals will be centred at the median of the distribution.

In summary, then, various types of probabilistic temperature forecasts could be considered. Credible interval forecasts seem to represent a reasonable compromise between current temperature forecasts, which ignore uncertainty (or probability) completely, and the reporting of an entire probability distribution, which is impracticable in terms of communicating to the public. Since intervals without probabilities are frequently included in current temperature forecasts, credible interval temperature forecasts do not represent a major change for the forecaster or the recipient of the forecasts. With regard to the time and effort required on the part of the forecaster, a variable-width credible interval that is central in terms of probability requires the assessment of two fractiles, whereas a fixed-width interval that is centred at the median requires the assessment of the median and the probability associated with the interval. Of course, forecasters applying these procedures may find it helpful to make additional assessments. In terms of interpretation by the public, the interval limits *and* the probability change from occasion to occasion with fixed-width forecasts, whereas only the limits change with variable-width forecasts.

3. An experiment in probabilistic temperature forecasting

(a) *Design of the experiment*

The experiment in probabilistic temperature forecasting of concern here was conducted in the Milwaukee WSFO. The five forecasters who participated in the experiment were experienced weather forecasters, averaging 10·5 years of forecasting experience and 5·1 years of experience of making precipitation probability forecasts. During the period of the experiment the forecasters made credible interval forecasts of high and low temperatures for 12-hour periods centred approximately 12, 24, and 36 hours in the future. On the morning shift, the forecasts were for 'today's high', 'tonight's low', and 'tomorrow's high', whereas on the evening shift they were for 'tonight's low', 'tomorrow's high', and 'tomorrow night's low'. The experiment was conducted from October 1974 to July 1975, thereby including all seasons and hence a wide variety of meteorological situations. The forecasters formulated 42, 44, 45, 45, and 57 sets of forecasts, for a total of 233 sets or 699 forecasts.

Three of the forecasters worked with variable-width, fixed-probability forecasts, using 50 per cent and 75 per cent central credible intervals. To obtain these intervals, the method of 'successive subdivisions' (for example see Raiffa, 1968, pp. 161–168) was used, requiring the forecaster to assess a median, a 0·25 fractile, a 0·125 fractile, a 0·75 fractile, and a 0·875 fractile, in that order. Each fractile necessitated an equal-odds indifference judgement to divide an interval into two equally likely subintervals (e.g. the median temperature is the temperature that the forecaster feels is equally likely to be exceeded or not exceeded). The 50 per cent and 75 per cent central credible intervals are the intervals from the 0·25 fractile to the 0·75 fractile and from the 0·125 fractile to the 0·875 fractile, respectively. All fractiles were assessed (and actual temperatures were measured) to the nearest degree, and all interval forecasts included their end points. After the fractiles were assessed, the forecasters were asked (as a check) if the resulting intervals seemed reasonable (e.g. if the temperature falling inside the 75 per cent interval was three times as likely as the temperature falling outside the interval) and were told to reconsider the assessments if they did not satisfy such consistency checks.

The remaining two forecasters worked with fixed-width, variable-probability forecasts, using intervals of width 5° and 9° centred at the median. First, the median was determined as for the variable-width forecasts, thus establishing the interval limits for the two fixed-width intervals. Then the forecasters assessed probabilities for the intervals, just as they might assess precipitation probabilities.

Before the start of the experiment, lengthy sets of written instructions were given to the forecasters, who were encouraged to read the instructions, to make several 'practice' forecasts, and to notify the experimenters if any difficulties or questions arose. The instructions included discussions of how credible intervals can be used to describe a forecaster's uncertainty about temperature, careful definitions of relevant terminology, hypothetical dialogues between an 'experimenter' and a 'forecaster' to illustrate the procedures, and brief summaries of the procedures. During the experiment proper, the forecasters formulated their credible interval forecasts without any assistance from (or contact with) the authors, and no difficulties were encountered.

(b) *Results of the experiment*

(1) *Medians.* The first task on each forecasting occasion was the determination of a median, and a comparison of these 699 medians (denoted by M) with the corresponding observed temperatures (denoted by T) is presented in Figure 1. The correlation between M and T is 0·966, and most of the points in Figure 1 are close to the $M = T$ line. In this regard, $M = T$ for 9·4 per cent of the forecasts, and the corresponding figures for $M < T$ and $M > T$ are 53·4 per cent and 37·2 per cent respectively. This result indicates a slight tendency for M to underestimate T.

Further evidence concerning the performance of the median temperatures is presented in Table I. For the entire sample, the average difference between M and T is —0·78°. If we assume that $M - T$ is normally distributed, an assumption that appears from the frequency distribution to be reasonable, standard normal theory can be used to make inferences about the population mean difference μ_{M-T}. These inferences (see Appendix, A.1) reveal a tendency for M to underestimate T, although this tendency is very slight in magnitude, particularly in view of the fact that all forecasts are recorded to the nearest degree.

Table I. *Averages (standard deviations) of forecast errors for medians (M), climatology (C_1), persistence (C_2), and autoregression (C_3)*

		Errors				Absolute Errors			
Forecasts	n	$M—T$	$C_1—T$	$C_2—T$	$C_3—T$	$\|M—T\|$	$\|C_1—T\|$	$\|C_2—T\|$	$\|C_3—T\|$
All	699	—0·78 (4·8)	—1·33 (8·9)	—0·46 (7·5)	0·52 (9·6)	3·66	7·10	5·89	6·71
Variable-width	432	—0·63 (4·4)	—0·68 (8·7)	—0·63 (7·1)	—0·57 (8·5)	3·39	6·75	5·54	6·11
Fixed-width	267	—1·03 (5·3)	—2·40 (9·2)	—0·19 (8·1)	2·28 (11·1)	4·10	7·67	6·46	7·70
Maximum	361	0·37 (4·8)	0·65 (8·9)	0·69 (7·8)	0·65 (9·2)	3·52	7·00	6·13	6·58
Minimum	338	—2·02 (4·5)	—3·45 (8·4)	—1·69 (7·0)	0·38 (10·1)	3·82	7·21	5·64	6·86
12-hour	233	—0·71 (4·6)	—1·01 (9·3)	—0·25 (7·2)	0·58 (8·7)	3·45	7·35	5·58	6·24
24-hour	233	—1·18 (4·2)	—1·75 (8·5)	—0·54 (6·7)	0·20 (8·0)	3·31	6·88	5·22	5·70
36-hour	233	—0·47 (5·5)	—1·24 (8·9)	—0·59 (8·5)	0·78 (11·7)	4·22	7·07	6·88	8·20
Forecaster 1	135	—1·30 (5·2)	—1·31 (9·9)	1·17 (9·0)	4·67 (13·0)	4·07	8·21	7·26	9·18
Forecaster 2	132	—0·76 (5·4)	—3·52 (8·3)	—1·59 (7·0)	—0·17 (7·9)	4·14	7·12	5·65	6·18
Forecaster 3	126	—0·16 (4·1)	—1·09 (6·3)	—0·66 (5·6)	—1·50 (6·7)	3·02	5·28	4·45	5·23
Forecaster 4	171	—1·39 (4·4)	—0·47 (9·2)	—0·20 (7·8)	0·52 (9·8)	3·44	7·04	6·04	6·94
Forecaster 5	135	—0·12 (4·6)	—0·55 (9·9)	—1·13 (7·4)	—1·08 (7·9)	3·67	7·75	5·92	5·87
Denver experiment	254	—0·50 (4·9)	0·60 (12·0)			3·80	8·90		

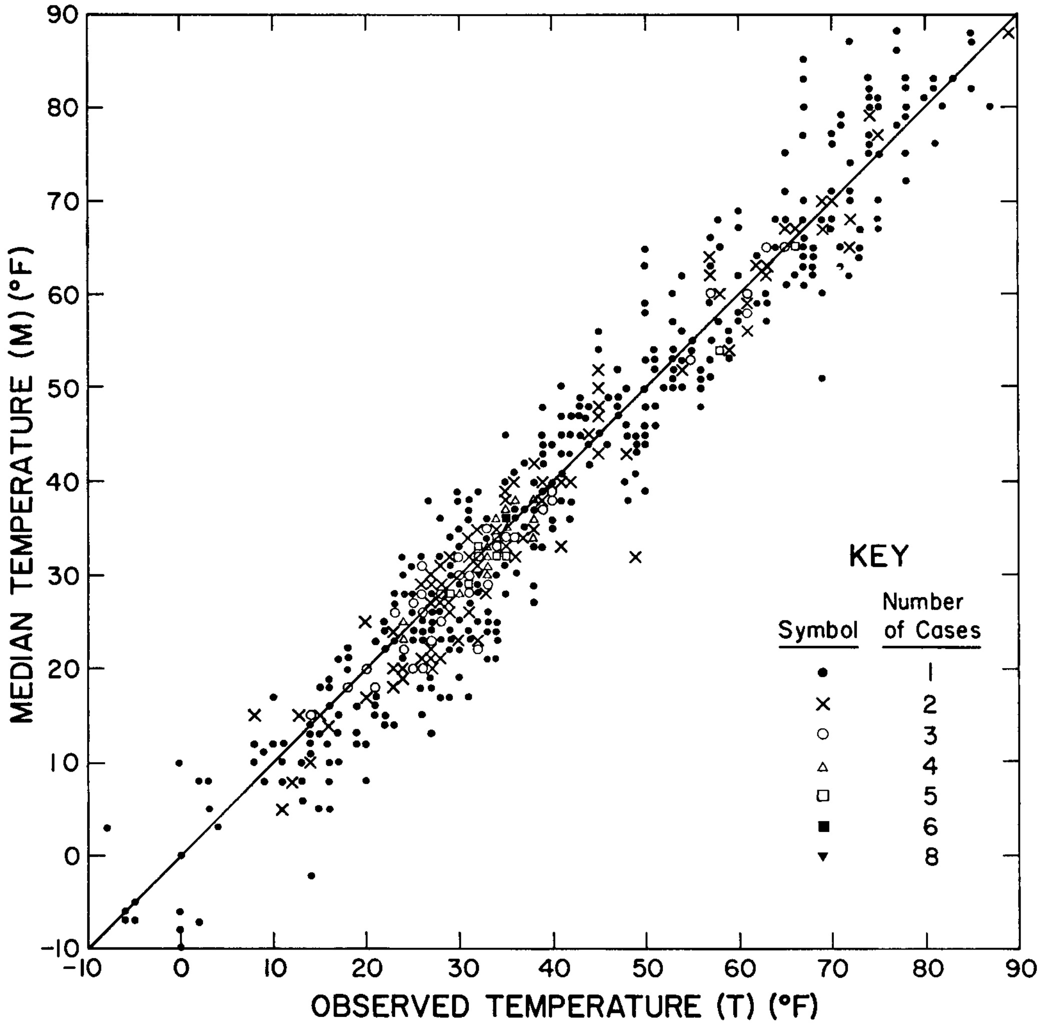

Figure 1. Median temperature (M) versus observed temperature (T) (n = 699).

The average $M - T$ is negative for all five forecasters, with underestimation greatest for Forecasters 1 and 4 and virtually nonexistent for Forecasters 3 and 5. No pattern in average $M - T$ appears to exist for the three lead times (12, 24, and 36 hours), but forecasts of maximum and minimum temperatures differ considerably. Forecasts of minimum temperature exhibit the most extreme average $M - T$ in Table I, $-2{\cdot}02°$, whereas forecasts of maximum temperature are slight overestimates on the average. One possible explanation for such differences involves asymmetries in the forecasters' distributions of temperature that could cause differences between median and mean temperatures. However, differences between maximum and minimum also occur in the frequencies with which $M < T$ and $M > T$, and these frequencies should not be affected by such asymmetries. Moreover, similar results pertaining to forecasts of maximum and minimum temperatures were obtained in a previous experiment conducted in the Denver WSFO (Murphy and Winkler, 1974).

To investigate the precision of the medians, the standard deviation of $M - T$ can be used, and we also consider the absolute error, $|M - T|$. From Table I, the average $|M - T|$ is 3·66°. We would expect $|M - T|$ to increase with increasing lead time, but a reversal occurs for the 12-hour and 24-hour forecasts, with the latter being slightly more precise. This result might be due in part to the fact that, with respect to the point in time that the forecasts are formulated, 24-hour forecasts are made for approximately the same point on the diurnal temperature curve, whereas the 12-hour forecasts are made for the 'opposite' point on this curve. In any case, the 36-hour forecasts are less precise than the forecasts for the two shorter lead times.

Note that Forecasters 1 and 2, who worked with fixed-width forecasts, have larger values of $|M - T|$ than the remaining forecasters, who worked with variable-width forecasts. In the variable-width scheme, the forecasters made several assessments that were similar to the assessment of a median (i.e. equal-odds indifference judgements), whereas the fixed-width scheme involved only probabilities for fixed intervals once the median was chosen. Perhaps the additional experience with similar assessments enabled the variable-width forecasters to improve in terms of precision of median forecasts.

In order to evaluate the medians further, three standards of comparison are considered: (i) climatological forecasts, (ii) persistence forecasts, and (iii) autoregression forecasts. The climatological forecasts (C_1) are median maximum and minimum temperatures for Milwaukee for the five-year period immediately preceding the experiment, computed on a 30-day basis twice a month (i.e. for each half-month C_1 is based on the climatological data for that period as well as for the preceding and following quarter-months). The persistence forecasts (C_2) simply represent the most recent observations of maximum and minimum temperatures (e.g., yesterday's high temperature is the forecast for today's high temperature). The autoregression forecasts (C_3) are derived from a first-order autoregression computed from the five years of data used to generate C_1.

The results for C_1, C_2, and C_3 are presented in Table I along with the results for M. First, note that for almost all sets of forecasts considered, the average $C_1 - T$ is further from zero than the corresponding average $M - T$. In addition, the percentage of occasions on which $C_1 < (=, >) T$ is 54·8 (4·6, 40·6), which indicates that the temperatures during the period of the experiment are above the five-year average. A comparison of such percentages and values of $C_1 - T$ for maximum and minimum temperature forecasts shows that the minimum temperatures are considerably above average, whereas the maximum temperatures are slightly below average. These results might explain the tendency of M to underestimate T slightly as well as the previously noted difference between forecasts of maximum and minimum temperatures. The results for C_2 are similar to but not as extreme as those for C_1, whereas the results for C_3 actually indicate overestimation instead of underestimation on the average. The fact that $C_2 - T$ and $C_3 - T$ tend to be less extreme than $C_1 - T$ undoubtedly is due to the consideration of yesterday's temperature in the determination of C_2 and C_3, which therefore are adjusted in part for the above-average temperatures during the experimental period.

The most striking difference between M and the other forecasts relates to precision. The most precise of the three standards of comparison is C_2, with an average absolute error of 5·89°, yet this error is still 1·61 times as large as the average absolute error for M. The median forecasts, therefore, represent considerable improvements over the climatological, persistence, and autoregression forecasts. In general the medians seem to be very good point forecasts.

(2) *Variable-width forecasts.* The three forecasters who formulated 50 per cent and 75 per cent variable-width credible intervals made a total of 432 forecasts, and some results from these forecasts are presented in Table II. The variable-width forecasts are very reliable in the sense that the degree of correspondence between the probabilities and the observed relative frequencies associated with the intervals is quite high. If the occurrences of observed temperatures within the 50 per cent and 75 per cent intervals are

Table II. *Relative frequency of temperature in variable-width intervals, average (standard deviation of) interval width, and average loss.*

Forecasts	n	Relative Frequency 50% Intervals	Relative Frequency 75% Intervals	Width 50% Intervals	Width 75% Intervals	Loss 50% Intervals	Loss 75% Intervals
All	432	0·539	0·794	5·90 (1·98)	10·06 (3·08)	11·53	14·63
Maximum	216	0·579	0·829	5·87 (2·02)	10·06 (3·15)	11·24	14·50
Minimum	216	0·500	0·759	5·94 (1·94)	10·06 (3·01)	11·83	14·77
12-hour	144	0·500	0·819	5·48 (1·83)	9·31 (2·79)	10·90	13·76
24-hour	144	0·569	0·778	5·99 (2·08)	10·27 (3·29)	11·32	14·44
36-hour	144	0·549	0·785	6·24 (1·97)	10·60 (3·02)	12·38	15·71
Forecaster 3	126	0·532	0·722	4·76 (0·89)	8·10 (1·30)	10·95	14·89
Forecaster 4	171	0·591	0·825	6·50 (2·46)	10·53 (3·44)	11·39	14·46
Forecaster 5	135	0·481	0·822	6·21 (1·55)	11·30 (2·90)	12·25	14·62
Climatology (C_1)	432	0·569	0·817	14·52 (3·91)	23·68 (4·91)	23·02	29·51
Persistence (C_2)	432	0·576	0·813	11·22 (2·94)	18·53 (4·98)	18·83	23·94
Autoregression (C_3)	432	0·590	0·780	11·55 (1·75)	18·97 (3·32)	21·58	29·35
Denver experiment	132	0·455	0·735	6·23 (1·28)	11·67 (2·23)		

treated as Bernoulli processes with a parameter for each interval, then we can make inferences about the parameters. These inferences (see Appendix, A.2) indicate that 95 per cent credible intervals for the parameters contain, or very nearly contain, the 0·500 and 0·750 values.

In experiments involving probability assessment in other contexts (e.g. in psychological laboratory experiments), assessors often appear to be too confident in the sense that a surprisingly high proportion of observations fall in the tails of the assessed distributions, indicating that the distributions are too tight (for example see Hogarth, 1975). In contrast, the variable-width temperature forecasts from Milwaukee (as well as those from Denver) are extremely reliable, particularly in view of the fact that the forecasts and observations are given only to the nearest degree. With respect to specific subsets of forecasts, the forecasts of minimum temperature are almost perfect in terms of reliability, whereas the relative frequencies for forecasts of maximum temperature are slightly high in relation to the probabilities. The relative frequencies are also too high for Forecaster 4, but no systematic deviations from the overall results seem to exist for the other two forecasters or for the three lead times, as can be seen from Table II.

The variable-width intervals considered here are supposed to be central credible intervals, and the overall relative frequencies are 0·280 (0·125) above the intervals and 0·181 (0·081) below the intervals for the 50 (75) per cent intervals. The disparity between these relative frequencies is consistent with the slight tendency to underestimate noted in the analysis of the medians, and variations in this disparity for particular subsets of forecasts are consistent with the results involving medians. For example, only for forecasts of maximum temperature is the relative frequency below the interval higher than that above the interval.

Of course, central credible intervals need not be symmetric about the medians in terms of width, and asymmetries in width indicate that the forecasters' distributions of high and low temperatures are not symmetric. Only 54·2 (47·9) per cent of the 50 (75) per cent intervals in the Milwaukee experiment are symmetric in terms of width. Furthermore, the average absolute differences in width between the two equally likely subintervals (created by dividing the interval forecasts at the median) are 0·64° and 1·13° for the 50 per cent and 75 per cent intervals, respectively. These figures are consistent over most of the subsets of forecasts, although Forecaster 4's intervals are quite symmetric (average absolute differences 0·29° and 0·26°) and Forecaster 5's intervals are especially asymmetric (1·25° and 2·45°). It should be noted that intervals based on climatology are also asymmetric, which suggests that an underlying meteorological basis may exist for such asymmetries.

The precision of the variable-width forecasts, as measured by the average widths of the intervals, is also of considerable interest. From Table II, the average widths are 5·90° and 10·06° for the 50 per cent and 75 per cent intervals respectively. As expected, the average widths are increasing functions of lead time. Among the forecasters, Forecaster 3's intervals are considerably narrower (and have much smaller standard deviations of width) than the other forecasters' intervals, and this increased precision is not attained at the expense of a reduction in reliability.

A positive relationship between interval width and the error associated with the median forecasts would be expected. In this regard, the correlations between interval width and $|M - T|$ are 0·348 and 0·383 for the 50 per cent and 75 per cent intervals, respectively. Moreover, the correlation between the widths of the 50 per cent and 75 per cent intervals is 0·898.

Variable-width forecasts can also be evaluated in terms of average losses, where these losses can be considered to represent the average 'expenses' incurred by an individual who makes decisions on the basis of the forecasts. A loss function of this type is defined in the Appendix, A.3, and the average losses according to this function are presented in Table II. These average losses reflect both the reliability and precision of the relevant forecasts.

As with the medians, forecasts based on climatology, persistence, and autoregression are considered to provide standards of comparison for the variable-width intervals. These forecasts are simply C_1, C_2, and C_3, as defined earlier, with the appropriate limits (0·125, 0·25, 0·75, and 0·875 fractiles) based on the temperature data for Milwaukee for the five years immediately preceding the experiment. The results for these standards of comparison, which are included in Table II, indicate that the forecasts produced by the different procedures are comparable in terms of reliability but that C_1 is less precise than C_2 and C_3. In terms of average loss, persistence performs best, followed by autoregression and climatology.

When C_1, C_2, and C_3 (collectively referred to as C) are compared with F, the forecasts formulated by the forecasters, the superiority of F clearly emerges. The reliability, as measured by the divergence of the probability associated with an interval from the corresponding relative frequency, is slightly better for F than for C, although the difference is not great. In terms of observations above versus observations below the intervals, C and F are very similar. With regard to average interval width and average loss, however, F is vastly superior to C. The smallest average widths and losses among C_1, C_2, and C_3 are almost twice as large as the corresponding average widths and losses for F. In general, then, the variable-width forecasts seem to be very good interval forecasts.

(3) *Fixed-width forecasts.* The two forecasters who formulated 5° and 9° fixed-width credible intervals made a total of 267 forecasts, and some results involving these forecasts are presented in Table III. In this case the interval probabilities are not fixed, but reliability can be defined in terms of the correspondence between the average probabilities and the relative frequencies associated with the intervals. In this sense, the fixed-width intervals do not appear to be quite as reliable as the variable-width intervals. Unlike the variable-width case, in which the relative frequencies were slightly higher than the probabilities, the average fixed-width probability is higher than the corresponding relative frequency by 0·07 and 0·06 for the 5° and 9° intervals respectively. Moreover, the average probability exceeds the relative frequency for all specific subsets of forecasts included in Table III with only one exception, the 5° intervals for a 24-hour lead time. We can offer no explanation for this exception, and no systematic results involving maximum versus minimum temperature forecasts, different lead times, or individual forecasters can be discerned from the data. Note from Table III, however, that the reliability of the fixed-width intervals is much better in the Milwaukee experiment than in the Denver experiment.

The precision of the fixed-width forecasts can be investigated by examining the average probabilities

Table III. *Average (standard deviation of) interval probability and relative frequency of temperature in fixed-width intervals.*

Forecasts	n	Probability 5° Intervals	Probability 9° Intervals	Relative Frequency 5° Intervals	Relative Frequency 9° Intervals
All	267	0·47 (0·11)	0·72 (0·11)	0·40	0·66
Maximum	145	0·48 (0·11)	0·73 (0·12)	0·43	0·69
Minimum	122	0·45 (0·11)	0·70 (0·11)	0·37	0·62
12-hour	89	0·54 (0·12)	0·80 (0·10)	0·45	0·75
24-hour	89	0·48 (0·09)	0·73 (0·08)	0·49	0·71
36-hour	89	0·39 (0·07)	0·63 (0·09)	0·27	0·52
Forecaster 1	135	0·50 (0·12)	0·73 (0·11)	0·40	0·67
Forecaster 2	132	0·44 (0·09)	0·71 (0·11)	0·41	0·64
Climatology (C_1)	267	0·22 (0·06)	0·37 (0·11)	0·19	0·36
Persistence (C_2)	267	0·27 (0·07)	0·46 (0·11)	0·24	0·42
Autoregression (C_3)	267	0·26 (0·06)	0·44 (0·09)	0·26	0·45
Denver experiment	122	0·60 (0·16)	0·80 (0·11)	0·46	0·66

assigned to the intervals. These average probabilities are 0·47 and 0·72 for the 5° and 9° intervals respectively. If the distributions of probabilities for the 5° and 9° intervals are approximated by normal distributions, then standard normal theory can be used to determine distributions for the mean probabilities. The results of such an analysis (see Appendix, A.4) indicate that these distributions are very tight (i.e. virtually all the probability is concentrated close to the sample means). The relationship between the probability assigned to an interval and the error associated with the median forecasts would be expected to be negative, and the correlations between probability and $|M - T|$ are $-0{\cdot}222$ and $-0{\cdot}285$ for the 5° and 9° intervals respectively.

Forecasts based on climatology, persistence, and autoregression also can be determined in the form of fixed-width intervals. The intervals are simply 5° and 9° intervals centred at the point forecasts C_1, C_2, and C_3 discussed in connection with the median temperature forecasts. The probabilities assigned to the intervals are based on the relative frequencies obtained for such intervals from the Milwaukee data for the five-year period immediately preceding the experiment. The results for C (C_1, C_2, and C_3 collectively), which are included in Table III, indicate that the average probabilities are closer to the corresponding relative frequencies than is the case with F (the forecasters' probabilities). The autoregression procedure yields almost perfectly reliable forecasts on the average. In percentage terms, however, the differences between the average probabilities and relative frequencies for C_1 and C_2 are similar in magnitude and direction to those for F.

As with the variable-width intervals, F is much more precise than C. The average probabilities for the forecasters' 5° and 9° intervals are almost twice as high (more than twice as high in one case) as the corresponding average probabilities for C. Of course, some of the precision in F was attained at a cost in reliability, and slightly lower probabilities would have improved reliability while still maintaining a high precision. In general, the basic difference in performance between the fixed-width and variable-width forecasts consists of the slightly lower reliability of the former compared to the latter.

4. Discussion and conclusion

Unlike forecasts of precipitation occurrence in the U.S.A., forecasts of maximum and minimum temperatures there and elsewhere generally do not include probabilities. Nevertheless, many different decisions are made each day that depend at least in part on temperature forecasts, and the absence of information concerning the uncertainty in these forecasts frequently may lead to decisions that are less than optimal. Uncertainty regarding maximum or minimum temperature can be expressed in terms of a probability distribution, and summary measures of this distribution can be used to communicate the uncertainty in a temperature forecast to specific users or to the general public. Of the alternative summary measures that might be considered, credible intervals seem to provide the best compromise between (a) current practice in temperature forecasting, in which probabilities are not assessed but forecasts are frequently expressed in terms of intervals, and (b) the reporting of the entire distribution or of other summary measures that would be difficult for users of forecasts to interpret.

The results of the experiment reported in this paper demonstrate that weather forecasters can successfully use credible intervals to quantify the uncertainty inherent in their temperature forecasts. The credible interval forecasts were very reliable—the variable-width forecasts outperformed the fixed-width forecasts in this respect—and very precise. Moreover, the credible intervals assessed by the forecasters were considerably more reliable and precise than corresponding intervals based on climatology, persistence, and autoregression (except for the reliability of the fixed-width forecasts). More sophisticated standards of comparison that would also be expected to outperform climatology, persistence, and autoregression could be developed, but as yet no such procedures exist that provide probabilistic forecasts.

In terms of point (i.e. categorical) forecasts of temperature, the model output statistics (MOS) system (Klein and Glahn, 1974) is used in the U.S.A. to generate objective statistical forecasts of maximum and minimum temperatures, based on the output of numerical models. Some recent results of the MOS program (Klein, 1978) indicate that the average absolute errors for these forecasts tend to be slightly larger than the average $|M - T|$ obtained in the Milwaukee experiment but much smaller than the average absolute errors for climatology, persistence, and autoregression at Milwaukee. Unfortunately, the MOS system does not as yet provide *probabilistic* forecasts of maximum and minimum temperatures.

The performance of the forecasters in the Milwaukee experiment is particularly noteworthy in view of the fact that they never had made probabilistic temperature forecasts before the experiment, although they did have a considerable amount of experience in formulating precipitation probability forecasts. For precipitation occurrence, however, NWS forecasters have access to MOS guidance forecasts expressed in probabilistic terms (Klein and Glahn, 1974). As indicated above, such guidance forecasts are not available in probabilistic form for maximum and minimum temperatures. In addition, the forecasters did not receive any feedback concerning their performance during the course of the experiment. Some feedback might make forecasters aware of any systematic deviations from perfect reliability or any differences between, say, forecasts of maximum and minimum temperatures and thus might enable the forecasters to improve by a process of self-calibration. Of course, the excellent results obtained in the Milwaukee experiment suggest that the prospects for further improvement are limited.

The temperature scale used in the Milwaukee experiment was the Fahrenheit scale. However, since almost all the countries in the world except the U.S.A. now report temperatures on the Celsius scale, perhaps it should be mentioned that the concept of probabilistic temperature forecasting in general, and the use of credible intervals as summary measures of probability distributions in particular, are equally applicable in the Celsius system. Of course, since a degree Celsius is nearly twice as wide as a degree Fahrenheit, credible interval forecasts in the Celsius system will be narrower than corresponding intervals in the Fahrenheit system. Moreover, the Celsius system generally will allow forecasters less flexibility in formulating credible interval forecasts than the Fahrenheit system. However, if temperatures in degrees Celsius were reported to the nearest 0·5°, then both the width and amount of flexibility would be almost the same in the two systems. In any case, uncertainty exists in temperature forecasts regardless of the scale that is used to report the temperatures, and this uncertainty should be quantified and included in the forecasts provided to potential users.

In conclusion, credible interval forecasts could be very useful in temperature forecasting, and consideration should be given to formulating such forecasts on a routine operational basis to supplement (not replace) point forecasts of maximum and minimum temperatures. The choice of types of credible intervals (e.g. variable-width versus fixed-width; selection of particular probabilities or widths) need not be limited to the types considered in the Milwaukee experiment, although the latter seem especially promising. In some cases, it may be desirable to formulate probabilistic forecasts of critical temperature events (e.g. the probability of below-freezing temperatures). These issues, as well as issues related to the implementation of an operational program of probabilistic temperature forecasting—including the dissemination of such forecasts to specific users and the general public—are discussed in some detail in a recent paper by Murphy and Winkler (1979).

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APPENDIX

A.1. Under the assumption that $M - T$ is normally distributed, standard normal theory can be used to make inferences about μ_{M-T} (Lindley, 1965; De Groot, 1970). Assuming an improper diffuse joint prior distribution for μ_{M-T} and σ_{M-T}, the posterior distribution for μ_{M-T} is Student with mean $-0{\cdot}78°$, standard deviation $0{\cdot}18°$, and 698 degrees of freedom. A 95 per cent credible interval for μ_{M-T} is then $(-1{\cdot}13°, -0{\cdot}43°)$. If instead of using a diffuse joint prior distribution, some results ($n = 254$, $\overline{M - T} = -0{\cdot}50°$, $S_{M-T} = 4{\cdot}9°$) from the Denver experiment (Murphy and Winkler, 1974) are considered as prior information, then the posterior distribution for μ_{M-T} is Student with mean $-0{\cdot}71°$, standard deviation $0{\cdot}16°$, and 952 degrees of freedom. In this case, a 95 per cent credible interval for μ_{M-T} is $(-1{\cdot}02°, -0{\cdot}40°)$.

A.2. Treating the occurrence of observed temperatures within the 50 per cent and 75 per cent intervals as Bernoulli processes with parameters p_1 and p_2, respectively, inferences can be made about these parameters (Lindley and Phillips, 1976). Assuming a uniform prior distribution in each case, 95 per cent credible intervals for p_1 and p_2 based on their posterior distributions are (0·490, 0·584) and (0·755, 0·831) respectively. On the other hand, assuming Beta prior distributions that reflect the results from the Denver experiment, 95 per cent posterior credible intervals for p_1 and p_2 are (0·477, 0·559) and (0·745, 0·813) respectively.

A.3. The loss function used to evaluate the variable-width forecasts in this paper can be defined as follows:

$$L(a,b,T) = \begin{cases} k(a - T) + (b - a + 1) & \text{if } T < a, \\ b - a + 1 & \text{if } a \leqslant T \leqslant b, \\ k(T - b) + (b - a + 1) & \text{if } T > b, \end{cases}$$

where a and b are the lower and upper limits (or end points) of the interval forecast. The interval that minimizes expected loss must satisfy the relationship $G(a) = 1 - G(b) = k^{-1}$, where G is the forecaster's cumulative distribution function for T (Winkler, 1972). For 50 per cent and 75 per cent central credible intervals, $k = 4$ and $k = 8$, respectively.

A.4. If the distributions of probabilities for the 5° and 9° intervals are approximated by normal distributions, then inferences can be made concerning the distributions for the mean probabilities (Lindley, 1965; De Groot, 1970). If a diffuse prior distribution is used, then the posterior distributions indicate that virtually all the probability is concentrated within 0·015 of the sample means.

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On relationships between tropospheric circulation patterns and both the date of spring reversal of stratospheric winds and the strength of winter stratospheric flow

By Janina Pawłowska*

(Polish Meteorological Service)

Summary

Two independent but similar investigations into possible connections between stratosphere and troposphere are reported with regard firstly to the date of the spring reversal of winds in the stratosphere and secondly to the strength of flow at 30 mb in winter. On a half-monthly scale there is little evidence to suggest that it is possible to predict both stratospheric parameters on the basis of preceding tropospheric developments, but there is some evidence that the date of the spring reversal may have a predictive value as far as tropospheric circulation patterns are concerned. However, more data are needed to confirm this suggestion.

Introduction

For the purposes of forecasting over time-scales of one to six months, it is essential to be aware of any relationships between variations in the seasonal evolution of the stratosphere and subsequent development in the circulation patterns of the troposphere. Conversely, it is of interest to know whether the stratosphere is influenced by previous developments in the troposphere.

Observational and theoretical studies have been carried out using dynamical models into connections between planetary wave motions in the troposphere and stratosphere (O'Neill and Taylor 1979, Holton 1975, Hines 1974, Murgatroyd and O'Neill forthcoming publication). Additionally, investigations have been made into two particular features of the stratosphere and their possible connections with tropospheric weather patterns: the first, and perhaps most important, phenomenon, is the fairly regular change in direction and strength of stratospheric winds in equatorial regions, known as the 'quasi-biennial oscillation' (QBO); the second is the change from winter westerly to summer easterly stratospheric winds at middle and high latitudes, which is termed either the 'final warming' (to distinguish it from temporary winter stratospheric warmings) or the 'spring reversal'. It has been found that there are some significant relations between the QBO and both local weather conditions (Perry 1977, Ugrjumov 1971a) and the tropospheric circulation over the northern hemisphere (Ebdon 1975). The existence of such connections may be useful, especially since it appears to be possible to predict future phases of the QBO (Parker 1976).

Investigations into whether the date of the spring reversal is related in any way to subsequent developments in the troposphere and is therefore of value for weather forecasting have been made in the United Kingdom (Ebdon 1966, 1972), Germany (Labitzke 1962), the Soviet Union (Ugrjumov 1971b, Ped' 1973a, 1973b), Japan (Wada and Asakura 1967) and Poland (Suryjak 1974, Pawłowska 1976). Some results have been found to be statistically significant and these are used in conjunction with other methods in preparing long-range forecasts.

The purpose of this paper is to report the results of two independent but similar investigations into possible stratosphere/troposphere relationships: the first is concerned with the date of the spring reversal, and the second with the strength of stratospheric flow during the period January to March, henceforth

* This work was carried out whilst the Author was with the Meteorological Office on a World Meteorological Organization Fellowship.

referred to as the 'reference period'. The possibility of predicting these two features of the stratosphere on the basis of preceding tropospheric circulation patterns is discussed, and also whether they themselves are of value as predictors of subsequent tropospheric developments. A half-month was considered to be a reasonable time-scale having regard to the possible practical application of any relationships found, and both half-monthly mean surface pressure and 500 mb geopotential data for the northern hemisphere from the period 1958–78 were used.

Spring reversal

The date of spring reversal was classified by reference to 5-day (pentad) mean zonal wind components at 30 mb at Shanwell, Scotland, for which almost complete data were available from 1958. Although it would have been desirable to use zonal components averaged round the hemisphere, the problems created by missing data could not be overcome in the time available. Following Ebdon (1972) the date of the spring reversal was taken as the commencement of that pentad over which the mean zonal wind component was easterly exceeding 5 kn and after which it remained either easterly or less than 5 kn westerly.

Eight years from a total set of 21 cases were selected as having an early change-over; these were 1959, 1964, 1969, 1972, 1974, 1975, 1977, and 1978. It was then necessary to test whether tropospheric circulation patterns associated with this set of years differed significantly from those associated with the other set (the remainder), considering separately periods preceding and succeeding the spring reversals. For each of the 12 half-months preceding (September–February) and 14 half-months following (June–December) spring reversals, composite surface pressure and 500 mb geopotential charts were produced for both groups of years independently. The composite charts were differenced to give an areal representation of mean differences in any half-month which might be related to the time of the spring stratospheric reversal but which would inevitably include a considerable random component.

The difference charts were examined for significance using a computer program to carry out Welch's test (Mack 1966), i.e. t-test when population variances may differ. Ratcliffe (1974) used this program in his study of 500 mb anomalies in long-range forecasts and also estimated the significance of the results obtained. He considered 198 grid points from 50°N to the north pole and found, using a random number generator, that an average of 32 points passed Welch's test at the 5 per cent level by chance. He was also able to compute the 95 per cent confidence interval about this chance value.

In this case, the area considered was extended southwards to 30°N and Ratcliffe's results cannot be strictly applied. However, by taking into account only those difference charts which have 50 or more significant values at grid points, or in excess of 35 significant values in consecutive half-months, it is unlikely that any real circulation differences have been excluded. These criteria were applied and the results provide little evidence to support the suggestion that the mean tropospheric circulation patterns of preceding half-months may provide an indication of the date of forthcoming spring reversal. However, one case is of interest, namely that of the mean surface pressure pattern in the second half of November, for which 53 grid-point values passed the 'W' test. The difference chart with significant areas is shown in Figure 1. It is probable that even in this instance the differences were random. If they were not then it would be seen from this figure that the second half of those Novembers preceding an early spring reversal would tend to have enhanced cyclonic activity over northern Russia and south of Greenland, with an increase of pressure in polar regions.

There is more evidence to suggest some relationship between the date of final warming and subsequent tropospheric circulation patterns beginning from the second half of the following October. The significance of the differences shown in Figures 2–6 is such that they are unlikely to have occurred by

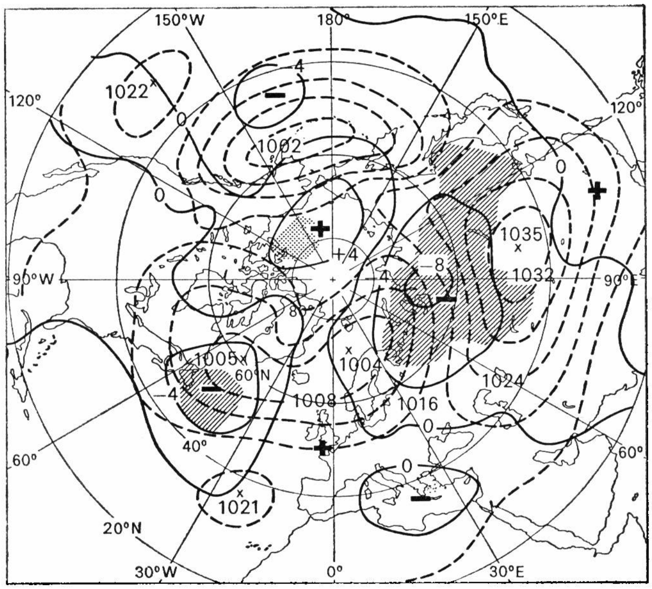

Figure 1. Differences in half-monthly mean surface pressure in millibars for the second half of November in years preceding early and late spring reversal of winds in the stratosphere. Dashed lines indicate half-monthly mean surface pressure from 1958 to 1978, solid lines show differences between means of two subsets of years. Stippling denotes significant areas of positive differences and hatching significant areas of negative differences.

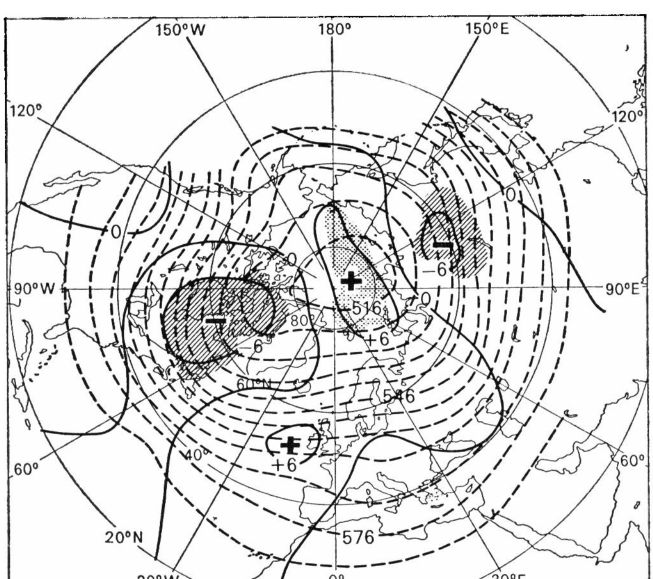

Figure 2. As Figure 1 but for 500 mb geopotential in the second half of October following spring reversal. Values are in decageopotential metres.

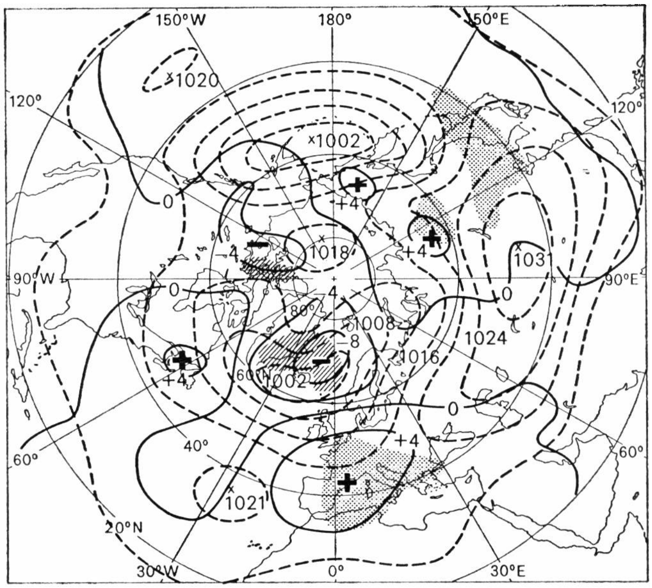

Figure 3. As Figure 1 but for surface pressure in millibars in the first half of November following spring reversal.

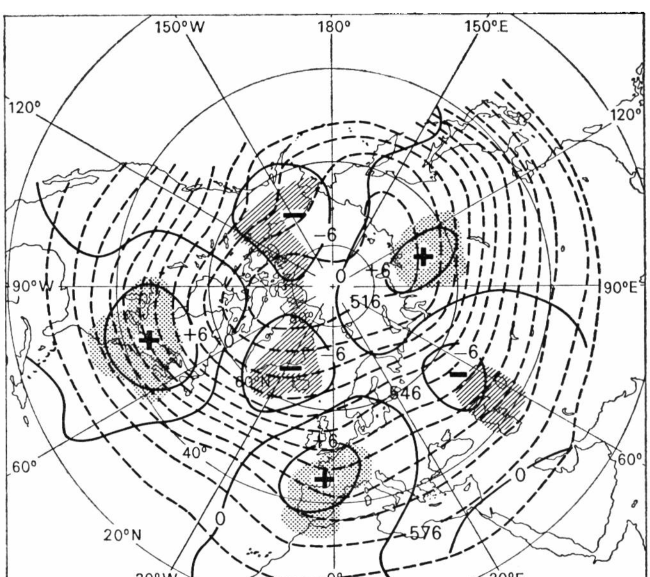

Figure 4. As Figure 1 but for 500 mb geopotential in the first half of November following spring reversal. Values are in decageopotential metres.

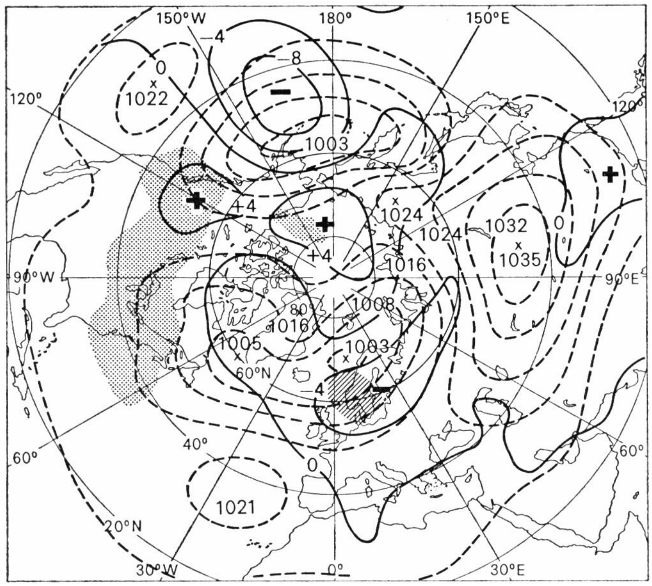

Figure 5. As Figure 1 but surface pressure in millibars in the second half of November following spring reversal.

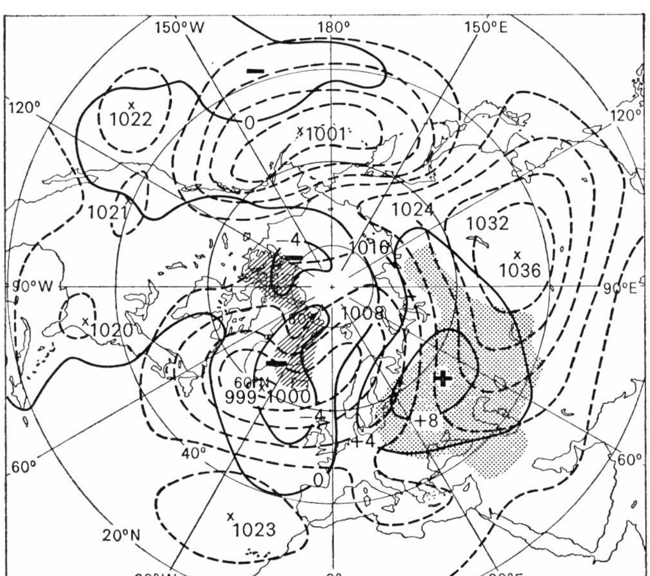

Figure 6. As Figure 1 but for surface pressure in millibars in the first half of December following spring reversal.

chance, since these charts include consecutive half-months; however, more data are needed to confirm this suggestion. If the behaviour of the past 21 years is repeated, then it is reasonable to expect in the second half of October (Figure 2) that 500 mb troughs over eastern Canada and over Siberia will be much deeper after early spring reversals than after late reversals, and the polar low less intense.

For the first half of November charts of differences between sets of years with early and late reversals are shown in Figure 3 for surface pressure and in Figure 4 for 500 mb geopotential. Two significant areas of particular interest in Figure 3 are those over Iceland and south-west Europe, which together indicate a substantial increase in surface westerly flow over north-west Europe during the first half of November following early spring reversals. Another interesting feature is the eastward shift of the centre of the Russian 'high', with more pronounced ridging to the north-east. The main effect at 500 mb (Figure 4) is seen to be increased zonality around much of the hemisphere after early spring reversals, with weaker troughs over Canada and western Europe, compensated to some extent by a more marked trough from central northern Russia to the Caspian Sea.

In the second half of November (Figure 5) the distribution of areas with positive and negative differences in surface pressure is the reverse of that in the first half of the month over much of the hemisphere, with the exception of the north-east Atlantic and northern Europe. There is a large area with positive differences extending southwards and then eastwards across most of North America and the western North Atlantic. The area of negative differences over northern Europe may represent a south-eastward movement of the centre near Iceland in the first half of November, which was noted earlier.

The difference chart for the first half of December (Figure 6) shows two significant areas, the first suggesting increased anticyclonicity over central and eastern Europe after early spring reversal, and the second suggesting lower pressure over the Canadian Arctic and Greenland, one result of this being a much strengthened cyclonic south-westerly airstream over the British Isles and Scandinavia.

Winter stratospheric flow

The second investigation into possible stratosphere/troposphere relationships was concerned with the strength of winter stratospheric flow. The pentad means of the zonal 30 mb flow at Shanwell, Scotland, were used to select years when the average zonal wind component over the 18 pentads from January to March exceeded 38 kn. This threshold was chosen to obtain adequate sample sizes for the application of the available statistical computer program.

Nine out of a possible 20 years (1962 was omitted because of missing data) were found to have strong westerly flow; they were 1959, 1964, 1965, 1966, 1967, 1969, 1974, 1976, and 1977.

In exactly the same way as described earlier for the spring reversal investigation, composite surface pressure and 500 mb geopotential charts were produced both for the group of years with strong stratospheric flow and for the remainder, and they were then differenced. The period considered extended from the September preceding the January–March period, when stratospheric winds were determined (reference period), to the following December. Grid-point differences were tested for significance using the same criteria as for the spring reversal investigation.

The numbers of grid-point values which passed Welch's test at the 5 per cent level are given in Figure 7 for each of 32 half-months considered. There is little evidence for any relationship between the strength of winter stratospheric flow and the tropospheric circulation. However, the significance of the four difference charts shown in Figures 8–11 might be real, because the number of points passing the W test exceed both the criteria for significance adopted in this note (see previous section) and that used by Ratcliffe. If this is the case then it can be seen from Figure 8 that there tends to be enhanced meridional

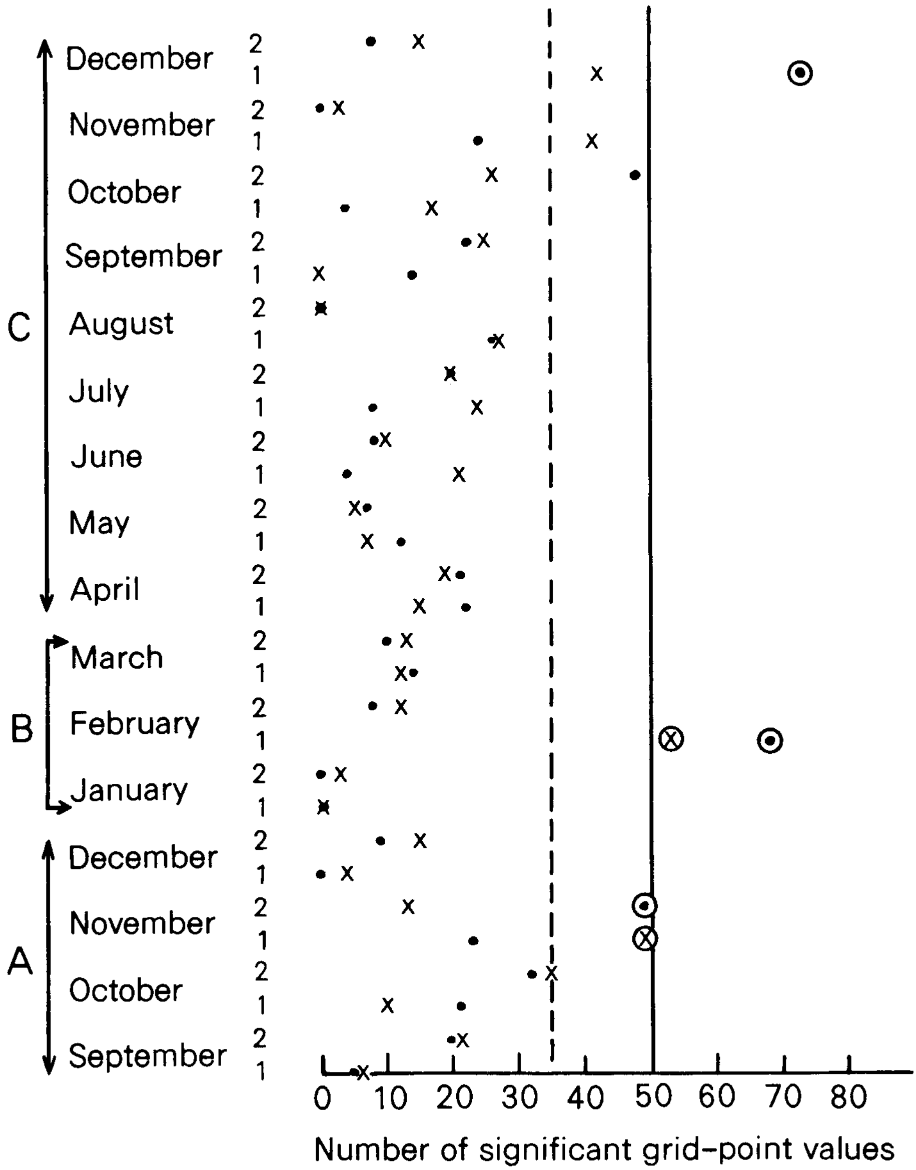

Figure 7. Numbers of grid points where differences were significant between the subset of years with strong winte westerly flow at 30 mb over Scotland and those with weak flow for half-monthly mean surface pressure and 500 mb geopotential.
A, B and C indicate half-months before, during and after the period of measurement of the strength of the stratospheric flow; 1, 2 indicate the first and second halves of a month. Dots indicate the number of grid points for surface pressure, crosses the number for 500 mb geopotential, and dots or crosses within circles the number which satisfied the criteria used by Ratcliffe.

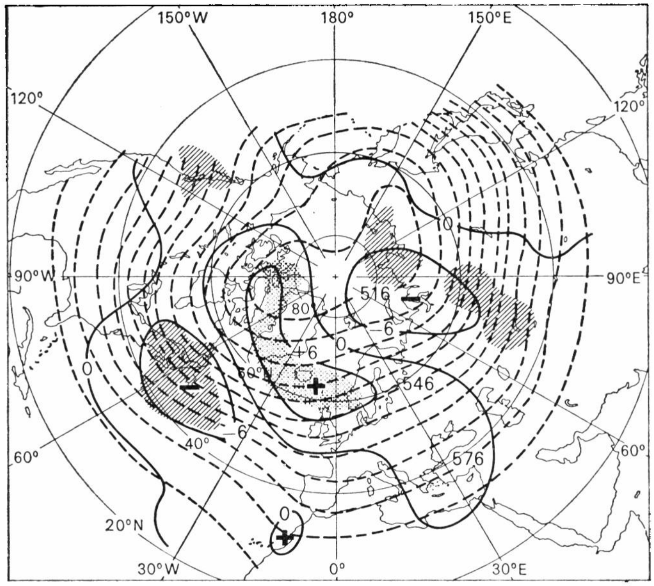

Figure 8. As Figure 1 but differences in half-monthly mean 500 mb geopotential for the first half of November in years preceding strong and weak winter stratospheric flow. Values are in decageopotential metres.

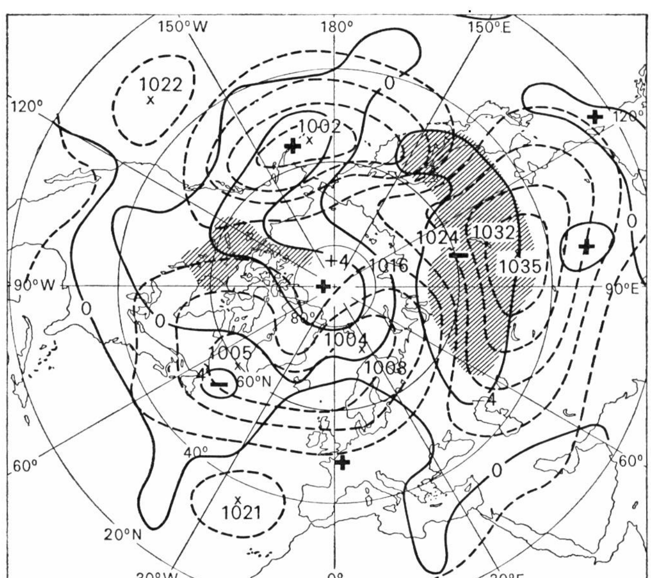

Figure 9. As Figure 1 but differences in half-monthly mean surface pressure in millibars for the second half of November in years preceding strong and weak winter stratospheric flow.

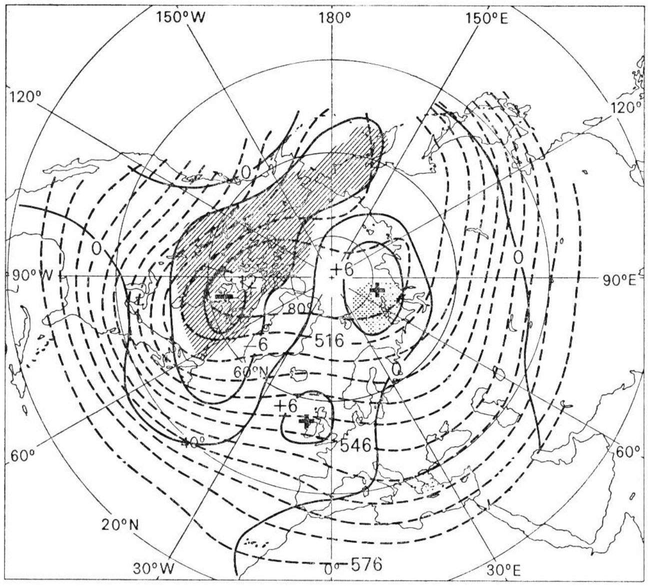

Figure 10. As Figure 1 but differences in half-monthly mean 500 mb geopotential for the first half of February in years during strong and weak winter stratospheric flow. Values are in decageopotential metres.

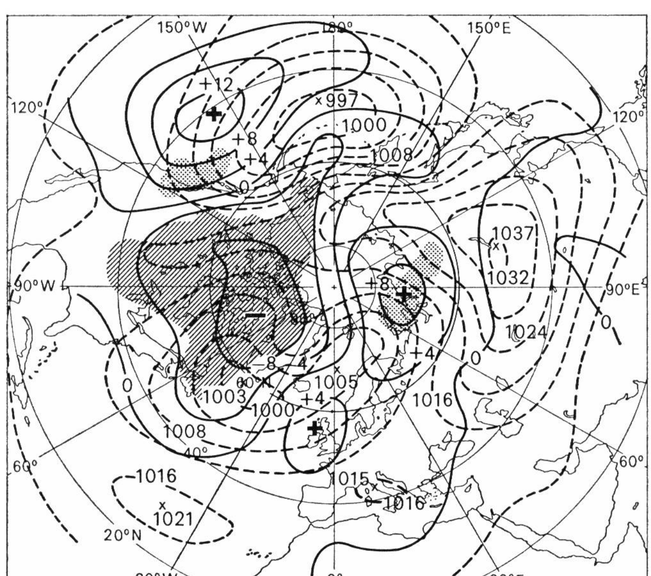

Figure 11. As Figure 1 but differences in half-monthly mean surface pressure in millibars for the first half of February in years during strong and weak winter stratospheric flow.

flow at 500 mb over much of the northern hemisphere in the first half of November preceding strong winter stratospheric flow. The surface pressure in the second half of November (Figure 9) shows the centre of gravity of the Russian anticyclone to be weaker and displaced slightly southwards.

In the first half of February (Figure 10), which is part of the 'reference period', the strong westerly flow at 30 mb is accompanied by a deeper 500 mb trough over North America and a ridge over the British Isles. The areas of significant positive and negative differences on the surface pressure chart (Figure 11) correspond to those at 500 mb.

It is also evident from Figure 7 that no coherent relationship between the strength of stratospheric flow and subsequent tropospheric circulation appears to exist.

Conclusion

It has been shown that, using data averaged over a half-month period, there is little evidence to suggest that it is possible to predict the date of spring reversal in the stratosphere or the strength of winter stratospheric flow from preceding tropospheric circulation patterns. Similarly, both stratospheric parameters appear to have little predictive value as far as subsequent developments in the troposphere are concerned. If there are such connections, then it seems from this analysis that they are most likely between the troposphere in autumn and the date of preceding spring reversal. However, more data are needed to confirm these suggestions.

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Memoirs of an Army Meteorologist

By H. Cotton, M.B.E., D.Sc.

Part 3

(Continued from the September 1979 issue.)

[From Le Touret Dr Cotton was posted to Querrieu]

Querrieu is a small town situated at a right-angle bend in the road from Amiens to Albert. In the corner, well back from the road, are the great iron gates of the Château, a building like the Château at Sailly Labourse but with much more extensive grounds. When I was there it was the Headquarters of the Fourth Army. Opposite the gates is a white-washed estaminet almost big enough to be called a small hotel. Practically the whole of the town is on the west side of the bend. About one hundred yards or so towards Albert the road crosses a little stream and a single-track railway line. A rough side road at the right-hand side doubles back sharply, ascends for about a hundred yards and then falls more steeply to the stream which by now has widened into a series of lagoons called *les étangs*. Strung along this side road is the village of Pont Noyelle.

To the south of the Albert road the land rises to a height of perhaps two hundred feet and then falls away southwards to the valley of the Somme River. I recalled a beautiful painting of 'The sleepy river Somme' in the Manchester Art Gallery. A pleasant land very different from the Artois I had known up to now; undulating, with wide fields and many scattered woods, but with none of the hedgerows which add so much beauty to the English landscape. At the highest point there is a monument to the 1870 war, but why they should want to commemorate the defeat of gross stupidity—being repeated even now in this far greater war—by machine-like efficiency I couldn't imagine.

The meteorological station was in a house at the top of the street of Pont Noyelle. It had a very complete equipment: inside the house a meteorological pattern barometer, a modification of the more familiar Fortin type, and a barograph which recorded on a clockwork-driven chart the changes with time of the barometric pressure. The rate of change, and whether up or down, was a valuable aid to forecasting. On the opposite side of the road, at the highest point, was a high platform with a cup anemometer mounted at one corner. This recorded not wind speed, but the number of feet travelled by the wind. The difference of the two readings, one at the beginning and the other at the end of a time interval as measured by stop-watch, divided by that time interval, gave the average wind speed over that interval. There was also the graduated plate mounted at a height of five feet from the ground and used with the portable anemometer as at Le Touret and Sailly Labourse. The cup anemometer was valuable because it could withstand high winds which would damage the delicate portable type. In such a case the wind direction had to be estimated and with experience this could be done with good accuracy. There were also a louvered Stevenson screen containing dry-bulb and wet-bulb thermometers, a rain-gauge, and a sunshine recorder.

Last, and of very great importance, was the theodolite specially designed for the observation of pilot balloons, like toy balloons which, when filled with hydrogen so as just to lift from the ground a specified weight, rose, when released, with a vertical speed of 500 feet per minute. Strictly speaking this could only be the rate of vertical climb if there was no vertical component of the wind's total velocity, a condition which was reasonably fulfilled with all but very light winds and with thundery conditions. The balloon's trajectory through the air was therefore the resultant of this free lift and the horizontal motion imposed by wind force and direction. This varied with altitude, sometimes considerably, and it was then difficult to get the balloon into the field of view again after the eye had been taken from the eye-piece in order to read the two verniers giving orientation and elevation. The theodolite, an expensive instrument, was taken indoors after each ascent and it was therefore necessary to orient it each time on some object whose exact bearing was known. We used the 1870 memorial for this purpose.

The Meteor Office, where I received telegrams and drew the synoptic charts, was one in a little town of Armstrong huts in the grounds of the Château. I was delighted to find that Corporal George was the other observer, two being required for pilot balloon ascents, one to use the theodolite, the other to record the readings. There was also the officer's batman, a young Irish boy, Banaghan, who should not have been in the army because of his youth, although he was safe enough at an Army Headquarters. He also acted as messenger; he was very simple. Our billet was the top storey of a barn at the top of the street at Pont Noyelle. It did not have the luxury of my hut at Le Touret, but it was comfortable enough except during the bitter winter of 1916/17 when the nights were exceedingly cold. I then missed my little stove.

The purpose of the balloon ascents was the computation of wind corrections for the artillery, this being necessary for three reasons: the use of many calibres of gun with ranges of a few thousand yards to several miles, each range having its own time of flight and its own height to climb into the upper air; map firing at targets which could not be seen but whose position was known on the map, this being particularly important at night and at all times when the weather was too unfavourable for artillery spotting; the use of howitzers with their high-angle fire and a trajectory quite different in shape from the flat trajectory of ordinary artillery—with times of flight as long as sixty seconds the absence of wind corrections could render a costly bombardment almost useless, or even result in the shelling of our own trenches. Four ascents were made during the twenty-four hours, at the fundamental hours. For the 1 a.m. observations the balloon carried a little home-made Chinese lantern and its weight had to be taken into account when filling the balloon. During the day-time observations the one who was recording was able, after some practice, to compute the upper winds in the time intervals between successive readings, a twenty-inch slide rule being useful for this purpose. The artillery corrections were computed indoors and the telegrams to Meteor and to the various artillery Headquarters were prepared in a remarkably short time.

Early in the September of the apparently never-ending Somme battles the adjutant came by H.Q. car and told me that I was to be taken to G.H.Q. He brought with him a replacement. As he sat at the front with the driver and I sat at the back, I had no chance to ask him where I was to go. I did get the information that I should only be staying at G.H.Q. for a few days and I had visions of miserable nights at the infantry barracks. Fortunately I was given a very comfortable room over the sweet-shop of M'sieu Brely Fatou in the Grand' Place. Each night before going to bed, I was given a cup of coffee with a generous lacing of cognac, and the only snag was the mattress, one of those things about two feet thick and, of course, no bedclothes. Still I didn't grumble when I thought about the infantry barracks.

As I was only staying at Meteor for a few days, no attempt was made to fit me into the organization. Because of my draughtsmanship I drew the weather charts based on the 7 a.m. observations and on the local observations. Copies were made on a jellygraph, the best copy going to the Commander in Chief, Sir Douglas Haig, and others to various headquarters.

My most important task, and the real reason for my staying several days, was the analysis of wind observations taken over a long period in the neighbourhood of Armentières. If a straight line is drawn in a south-easterly direction from Cap Gris Nez, the land to the east is, on the whole, very flat, the Flanders plain, while to the west it is much more elevated, there thus being an irregular escarpment running in a south-easterly direction. My job was to find out if some peculiarities in the early morning winds were due to this escarpment. Later events were to show that this was not a mere theoretical exercise but that, on one occasion at least, it was to be of very great importance. The investigation consisted of determining the north-east flowing and south-east flowing components of all the winds in this locality for which data were available. A comparison of the two components showed that, no matter what the total wind direction, there was a marked component in the north-east flowing direction but not in the other in the early hours of the morning. This component was particularly marked during anticyclonic conditions when, with very small or even zero barometric gradient, the gradient wind, that is, the wind appropriate to the concentration of isobars—the ordinary wind—was also either very light or zero. This component was a katabatic wind produced by the draining of cold, and therefore heavy, air from high to lower ground.

When I had completed the investigation the C.O. sent for me. He asked,

'Are you afraid of heights?'

'I don't think so, Sir', I replied 'although I have had no experience of great heights. I can look over the edge of a high steep cliff or from a top floor window of a tall building without feeling in any way dizzy.'

He then abruptly changed the subject.

'Pilot balloons give only the wind velocity and direction, and therefore the results can be used only to determine the correction necessary to neutralize the wind pressure on a projectile. They give no indication of the resistance to motion. For this it is necessary to know the density of the air at various heights, and as it is impossible under war conditions to attach instruments to pilot balloons it follows that an observer must be employed for this purpose.'

'But we have someone doing this', I pointed out.

'Yes', he said 'but he doesn't like heights, that is why I asked you if you are afraid of heights.'

'You want me to undertake this work, Sir?'

'That is the idea. Lieut. Young is going to England tomorrow and will be away for a fortnight. I want you to take his place for that time to see if you can do the work successfully. If you can it may be that it will be your contribution until the end of the war.'

So the following day I went by H.Q. car to a kite balloon almost due east of the ruined city of Ypres. The accommodation was in a large farm so that there was plenty of room for officers' and men's messes and, very important, for the packing of the parachutes, this necessitating a long room and a long wide table. When I arrived it was almost time for the 1 p.m. ascent, the fundamental hours of 7 a.m., 1 p.m. and 6 p.m. being kept whatever the purely military requirements of artillery spotting. Actually this combination of two entirely different duties did not work well and soon afterwards the balloon was withdrawn from the fighting zone and used solely for meteorological purposes.

It was an ideal day for one's first essay in upper-air observations, and I asked if I might make the ascent. There was very little wind, the sky was blue and there were many small detached cumulus clouds, all having the horizontal base indicating the level at which condensation took place. I estimated that height to be 3700 feet—I had become expert by this time; actually it was just under 4000 feet. I wore the leather Sidcote suit, a leather flying helmet, leather gauntlets with a flap which could be fastened back so as to leave the fingers free; also parachute harness. If one has to jump, the velocity of free descent before the parachute opens can be quite high and there is a violent jerk at the moment of opening. Knowing this, I decided against a harness which had a strap passing from front to back between the legs. If it was not quite in the right place one might suddenly lose interest even in the problem of landing. There was also a razor-sharp knife for cutting myself free of the parachute on landing, if this should be necessary.

I climbed into the glorified clothes basket along with the balloon officer who was making the ascent with me. A rigger tied by special knot the parachute rope to a ring in the harness and I then checked my instruments; a beautiful portable aneroid barometer, graduated in hundreds of feet and so sensitive that the needle would deflect for the small change in height if one placed it on the ground; a prismatic compass; Pitot tube for indicating the wind velocity; psychrometer. This was an elaborate form of wet and dry thermometer. Each thermometer was housed in a metal tube which projected from a chamber containing a clockwork-driven fan. When the fan rotated air was drawn up the tubes and therefore past the thermometer bulbs, this being much superior to the static thermometers used in the usual Stevenson screens. The compass was used to determine the wind direction. The balloon, being attached at one end of the cable, set itself along the wind direction and thus all that was necessary was to take a bearing on the attachment to the cable, this being in the form of a steel V. When I had checked the instruments and also the telephone connection with a signaller on the ground, the balloon officer gave the thumbs-up sign to the flight sergeant. He in turn gave the order:

'Let go the guys',

and I began to laugh. It was a joke I enjoyed every time I made an ascent but, strangely enough, nobody else seemed to see it. As the balloon's height increased everything appeared progressively smaller so that eventually it was, I suppose, something like looking at the surrounding country from the top of a mountain except that there was no terra firma under one's feet. The balloon was stopped at five hundred foot intervals, the heights being given by the barograph. At the height of the cloud base there was a sudden and beautiful change. There was a very slight haze on the ground although hardly enough to affect visibility. At this particular height it had the appearance of the surface of a dead-smooth, milky coloured sea and the clouds looked like little icebergs floating on it. At the level of the cloud base condensation commences and the latent heat of evaporation is given up, thereby producing a rise in temperature instead of the progressive fall in temperature as the height increases. I therefore stopped the balloon at this level so that I could take this temperature. I knew about inversion, as the phenomenon is called, but it was very exciting actually to be in the middle of one. As far as I remember we reached four thousand feet, the inclination of the cable due to the slight wind causing the winch to pay out more than would have been the case with a dead calm. At this height I could just make out the white cliffs of Dover.

It was during an ascent at sunrise that I had the most beautiful experience. It was perfectly calm with a cloudless sky apart from a few pink wisps of cirrus. To the east the sun had just cleared the horizon, an orange-coloured disc. For some reason I turned round and looked to the west and there, diametrically opposite the sun and about to set behind the ruins of Ypres, was the full moon, looking very pale as though shocked by what she had seen. The sun and full moon both in the sky together; God-made beauty in the heavens, man-made hell on earth.

While there was still happiness in the world, and love instead of hatred and slaughter, Graham Peel composed a lovely little song called 'The Early Morning' to Hilaire Belloc's words:

'My brother, good morning:
My sister, good night'.

Another beautiful phenomenon was the rainbow round the shadow of the balloon when we ascended into sunshine above a uniform cloud sheet. The bow was a complete circle with the balloon's shadow at the centre and occasionally, but not always, there was a faint secondary bow of greater diameter and with the order of the spectral colours reversed. I was to see this several times but I never again saw the rising sun and the setting full moon in the sky together. Cecil Lewis in *Sagittarius Rising* also saw the circular rainbow but apparently only once. It was early evening with the sun's rays almost horizontal and his shadow and the surrounding rainbow were on the almost vertical face of a vast cumulonimbus cloud. He entered the cloud through his own shadow.

Since I was engaged in war duties it was inevitable that there should be a certain amount of unpleasantness. For example there was a German six-inch high-velocity gun a few miles away. It was spotted by the flashes and located by intersections from my own balloon and those on either side. It fired at us from time to time but I think it was merely devilment on the part of the gunners since a visibly small object several miles away and several thousand feet up in the air is an exceedingly difficult target.

More disturbing were the antics of young pilots flying Sopwith Camels. They had great fun, to them, zooming on to the balloon and almost running their wheels along the top. A very small error and they would have ripped the balloon from end to end. We could see their grinning faces and on such occasions one of the balloon officers used the most awe-inspiring language I have ever heard. Of course they were only having fun, and as the average life of a pilot was only about three weeks they had some justification in getting their fun when they could. All the same I wished that they would go somewhere else for it.

In spite of the gun and in spite of the crazy pilots I was sorry when my fortnight with the balloon came to an end and I was taken back to Fourth Army Headquarters and the Somme Battles.

The long drawn-out struggle called the Battle of the Somme has been written about many times. At the time of the Battle of the Marne in 1914, a battle which decided the ultimate outcome of the war, that Germany should not win and thereby achieve mastery over the whole of Europe, Joffre, at the most critical moment, turned to Sir John French and said 'Monsieur le Maréchal c'est la France qui vous supplie'. And now Britain was answering Joffre's prayer and tens of thousands of young men to whom France meant nothing whatever were watering the land of France with their blood. It looked as though the mass sacrifice would go on for ever and with nothing gained, for with each obstacle overcome they were confronted with another one almost similar. Our High Command, not having been brought up to control vast armies of hundreds of thousands, was learning the hard way, and the troops paid the bill.

For me and for all of us at Meteor life continued unchanged. Occasionally I would go to Amiens on the Section motor bike, a Douglas horizontally opposed twin cylinder machine, and very good too. This was to make purchases for the officers' mess, and it gave me a chance to look at the shops, especially a big bookshop in the Rue des Trois Cailloux. I also visited the Cathedral, the front heavily sandbagged. Also Corporal George thought it was time he gave me a few drawing lessons as he thought my efforts were all too niggling. In return I gave him lessons in Physics although without Mathematics one cannot get very far. There was a shortage of the little tin holders for the candles of the home-made lanterns and the Headquarters workshops received an indent for a fresh supply, surely the most strange assignment ever given in wartime.

The winter of 1916/17 was bitterly cold and the 1 a.m. balloon ascents on nights of clear sky and bitter east wind were something to be dreaded. On one such night but with hardly any wind I was at the theodolite, and as my sight in those days was very keen I kept the lantern in sight up to 20 000 feet, which meant that the observations took forty minutes. As I could not move about and stamp my feet and as I was only wearing mittens so as to keep the fingers free, I was frozen by the time I lost the balloon. I hurried to the office and was foolish enough to warm my hands by the fire. The resulting aching was excruciating and I never did that again. We had a jar of ration rum, thick like treacle and red like wine, and I took a good dose of that and went to bed, but it was a long time before the intolerable ache went from my fingers.

The office was next door to a cottage occupied by an old lady who also remembered the German occupation of 1870 (although she, apparently, did not experience any ill treatment), her daughter and little granddaughter, aged about five. We used to visit them just for a talk, and instead of the thin rather sour white wine they favoured I brought a packet of tea which they had not tasted before. On one occasion while talking to the little girl, pointing to various things so that she could say what they were, I put salt in my tea thinking it was sugar. After that I was always M'sieu Sel.

In the early spring of 1917, the German Army of the Somme disappeared. One day they were there, the next day they were gone. They withdrew, a strategic withdrawal in the real sense, a victory not a defeat. They withdrew a distance of thirty miles to a position so strong as to be thought impregnable. And it would have been impregnable to the bull-at-a-gate tactics currently employed. They turned the beautiful countryside they evacuated into a desert, destroying towns, roads, crops, orchards, everything which could contribute to human habitation. The wrecked towns and villages became the back areas and not a single civilian was left in them. They shortened their communications by thirty miles, they increased the British lines of communication by the same distance. The land they gave up and destroyed was French, not German, and was therefore of no emotional significance to them.

There was nothing for it but for the British Army to move after them, and that meant Meteor as well. We loaded all our equipment and our belongings on to a large lorry and said goodbye to the little family next door. The little girl's goodbye to her M'sieu Sel was very tearful. We climbed the hill leading to Corbie and I looked at the Calvary for the last time. At Corbie we joined the road from Amiens to St Quentin which runs due east, straight as an arrow, except for a small conformity with the gradient at Foucancourt. There was no mistaking the battle areas, there were so many shell-holes that there was hardly one which did not overlap its neighbours. The trees were mere stumps giving a disquieting air of desolation and some were blown completely out of the ground. The sides of the road were littered with the debris of motor vehicles of all kinds.

At Estrées, or what was once Estrées but now almost in the real Biblical sense had not one stone on another, a branch road runs south to Villers Carbonnel and then to a group of once pleasant towns, Marchelepot, Briost, Misery, all deserted. At Villers Carbonnel a narrow road runs eastwards, parallel to the main St Quentin road. There must have been a bridge over the river, but this had been mined and the river was now widened into a very extensive swamp. By the river bank were the ruins of a small village and in the churchyard, leaning at a crazy angle, a large iron cross decorated with elaborate strap-iron curlicues. The Somme River takes a roughly northerly course as far as St Quentin, where it turns west and then takes a very meandering route to Amiens and finally Abbeville and the sea. It formed the junction between the Third Army to the north and the Fifth Army to the south.

I went fishing occasionally but there was not much pleasure in it as I only caught eels. These are dreadful creatures for, apart from refusing to die, they swallow bait, hook, and as much of the line as they can get down. To recover the tackle it is often necessary to slit them to almost half-way down. I would not have eaten anything from that river no matter what I caught.

The road east from Villers Carbonnel is at first flat, then rises gently to about half a mile from the river to which it then descends, rather sharply. Fourth Army Headquarters was in an enormous Nissen hut erected on the flat area. This was the operations room. The Headquarters staff were housed in small Nissen huts, lining both sides of the road, almost like a street. Small gardens had been constructed in front of these and they looked very gay when the flowering plants were in bloom. If it had not been for the war, and if the life of the neighbouring towns had been that of peace time, the situation would have been almost idyllic.

The Meteor office was a large hut sited at the highest point and opposite, on the other side of the road, was a high platform with a cup anemometer exactly as at Pont Noyelle. The underneath of the platform was closed in so as to make a room in which the theodolite could be stored when not in use. There were also the Stevenson screen, the pole-mounted graduated brass plate for surface wind measurements, and the rain-gauge. The sunshine recorder was on the platform placed so no shadow would fall across it. It consisted of a glass sphere which brought the sun's rays to a focus on a strip of special paper which was blackened by the heat, the intensity of the blackening being a rough measure of the intensity of the light. This paper strip was graduated in hours, the times of sunshine and of no sunshine being indicated.

The cross-section of a Nissen hut is roughly semicircular, the walls leaning inwards. Also they are not very rigid and are therefore quite unsuitable for the mounting of important scientific instruments. For this reason a solid brick pillar had been built in one corner, the barometer fixed on one side and the barograph resting on the top. The furnishing of the hut was a large centre table, stools, and at the end remote from the door three bunks. There was a shelf for books, a cupboard for odds and ends and hooks for greatcoats, hats and revolvers. Banaghan had no arms of any kind; if he had been issued with a rifle he must have been allowed to hand it in when he became a batman.

The fighting front was very quiet for the whole of the time Fourth Army Headquarters was at Villers Carbonnel and it would have been crazy to have attacked the new German defences at that time and without vast preparation also for the Germans to leave these defences so soon after their withdrawal and to cross the desert which they themselves had made. They could afford to wait. I had a feeling that our stay here was meant to be a quiet interlude before the fireworks started again. I had no complaints and neither had the others, particularly as the food was now very much better than that we had been getting at what the troops called 'Bacon fat corner' at Querrieu. There was precious little bacon.

An idyllic situation can only be enjoyed if one is in an idyllic state of mind and that was far from the case. The whole of the terrifying casualties suffered during the course of the Somme battles were not disclosed, although we now know that on the very first day alone there were losses of about 60 000 men. One of the regiments to be decimated on that tragic first day was the 5th North Staffs., the regiment I most probably would have joined. If it had not been for that letter from the War Office I realized that most probably I should have been one of the 60 000.

(To be continued.)

Notes and News

Retirement of Mr G. A. Howkins, M.B.E.

Mr G. A. Howkins, M.B.E., Assistant Director (Data Processing), retired from the Meteorological Office on 3 October 1979 after a meteorological career of 39 years, the first six years of which were spent in uniform.

Having won a County Major Scholarship to King's College, London in 1938, Gordon Howkins obtained his degree two years later and immediately entered the RNVR where he received his introduction to meteorology. Early in his period of service, he spent 18 months in the Falkland Islands. After receiving a commission in 1943, he returned to the south as a member of Operation Tabarin which laid the foundation for the subsequent establishment of the Falkland Islands Dependencies Survey.

On demobilization in 1946, he joined the Office as a Scientific Officer and immediately took charge of the meteorological office in the Falkland Islands. He was promoted to Senior Scientific Officer in 1950 and was subsequently seconded to oversee the creation of the Dependencies' own meteorological service.

In 1956, Mr Howkins returned to the United Kingdom and, after attending the Training School, spent eight years at London (Heathrow) Airport, initially on upper-air forecasting, and from 1961 as a Senior Forecaster following promotion to Principal Scientific Officer. In 1964, he moved to Bracknell where he spent two years on the development of computer-based methods of forecasting upper winds for aviation. This was followed by a spell as Senior Forecaster in the Central Forecasting Office, and he later took charge of the team which developed the operational numerical forecast suite.

In 1968 he joined the Data Processing Branch as Secretary of the Working Group which was set up to select and acquire the new COSMOS system based on the IBM 360/195 computer. Two years later, he became its first Computer Manager in which post he played an active part in the progressive transfer of computing work from the KDF-9 system, and implementation of the present-day wide-ranging computing services to branches throughout the Office.

When, in 1973, Mr Howkins took charge of the Data Processing Branch, he was soon occupied in planning for the acquisition of the front-end 370/158 computer and its integration into the COSMOS system. As users of the system repeatedly found, he took endless pains to tailor services to meet their requirements and was apt to worry until the smallest details were settled. He was fascinated by complex systems and was often tempted to reorganize areas outside those under his own control.

Of late, his colleagues have been increasingly concerned that Gordon Howkins's tendency to work too hard might seriously affect his health. Fortunately, he has discovered that hard physical exertion helps his condition. For this reason, we wish him and Mrs Howkins a long and active retirement, with plenty of gardening at their new home in Ascot and vigorous interludes of hill walking to add variety.

M. J. Blackwell

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CONTENTS

NOTICES

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