M.O. 581 AIR MINISTRY

METEOROLOGICAL OFFICE

THE METEOROLOGICAL MAGAZINE

VOL. 83, No. 981, MARCH 1954

WEATHER FORECASTS ON TELEVISION

By THE DIRECTOR OF THE METEOROLOGICAL OFFICE

On January 11, the B.B.C. began the transmission of a new type of weather news item in the regular television programmes. Previously the forecasts had been read by an unseen announcer during the display of two "still" charts. As many readers of the *Meteorological Magazine* are aware, in the new programmes a member of the Office staff appears in the studio every night and spends five minutes discussing the weather situation, past, present and future, with the aid of simplified isobaric charts.

The new series has barely completed a month's run at the time of writing so that it is far too early to attempt any serious evaluation of the techniques adopted, but it is safe to say that, judged by the reaction of the press, the "live" presentation is generally looked upon as a big improvement on the old "static" programme. To a certain extent the present period is regarded by the Meteorological Office and the B.B.C. as experimental, and it was recognized that the forecasters chosen for this exacting work would need a little time to accustom themselves to their new duties. Actually, the three meteorologists who have initiated the new series—T. H. Clifton, G. Cowling, and P. McAllen—seem to have struck the right note from the start.

There is, of course, nothing essentially new in this use of television, and the weather talk has been accepted as an essential ingredient of television programmes in the United States and in Canada for some time. However, the British system differs from those in vogue across the Atlantic in several ways. In at least one widely distributed American programme the forecaster speaks from his office to the announcer in the studio who draws a simplified map according to the forecaster's instructions. In other programmes the forecasts are given by one selected "weather man". I believe that the present policy of bringing the forecasters to the studio and of using a team, instead of an individual, is the right choice for this country. The method of presentation, namely the "serial story" which starts by a brief recital of the successes and mishaps of yesterday's forecasts followed by a quick run-over of present weather as a lead-in to the prebaratic chart and the area forecasts, is one which will have to be judged by results over a long period. Before the programmes were put on the air, the B.B.C. and the Meteorological Office co-operated in a long series of experiments with various types of presentation, including animation, but there is no finality in the choice of the present method and suggestions for improvements would be welcomed.

I believe that the introduction of this direct "forecaster to public" service is an event of considerable significance for the Meteorological Office. The communication of the conclusions of the meteorologist to the user is at least as important as the preparation of the forecast, and in this connexion it must be realized that the general public needs special attention. I believe it is generally recognized by the specialist users that a forecast is a statement of probabilities, a warning that the physical conditions obtaining in the atmosphere at a given time may lead to certain eventualities, and the prudent professional user makes his plans accordingly. Meteorologists should try to get the same view adopted by the general public to a greater extent than at present. In the present state of our knowledge of atmospheric processes it is often not possible to go beyond admitting a risk of certain unpleasant forms of weather, such as thunderstorms, in certain areas, but the forecast is not necessarily valueless if these possibilities do not materialize. The meteorologist, in fact, should regard himself as much an adviser as a prophet. The direct broadcast, especially on television, allows this approach to be made more easily and more convincingly than does the written bulletin, which tends to give the impression of hedging if it contains too many expressions of uncertainty. It remains to be seen if the new venture will achieve success in promoting a better understanding of the work of the forecaster; that it will engender a more lively interest in meteorology can hardly be in doubt.

For these reasons, if no other, the advent of the television weather programme is to be welcomed, and the Meteorological Office is grateful to the British Broadcasting Corporation for the advice and assistance which have been given so freely in the preparation and maintenance of the new programme.

MEASURES OF SUCCESS IN FORECASTING

By A. F. CROSSLEY, M.A.

Information on the accuracy of forecasts is required in order to provide a measure of progress and to compare the merits of different techniques. While it is now amply recognized that in general 100 per cent. accuracy can never be achieved[1], there is nevertheless still room for progress in many directions and considerable effort is being expended to make advances wherever possible. It is thus important to be provided with a quantitative assessment of the progress achieved by any developments in techniques. Since in practice it is seldom that more than one technique is applied to precisely the same set of data, it is necessary to be able to compare the success of different techniques applied to different sets of data. Hence suitable indices of success need to be devised which will be, as far as possible, independent of the technique used and of the varying circumstances in which the forecasts are made. Further, it is desirable to minimize the effects of persistence in the elements being forecast, since repetitive cases can often be forecast without much aid from highly developed techniques, with the result that the presence of persistence tends to conceal any essential usefulness of those techniques. In other words, a correct forecast of a substantial change in conditions is usually reckoned as being more meritorious than a correct forecast when conditions remain steady.

Indices for forecasts of the "black or white" type will be considered first, and subsequently those for forecasts of scalar or vector elements.

Useful effort, and the condition for worth-while forecasting.—In an earlier paper[2] it was shown how the usefulness of forecasts of two mutually exclusive events is related to the accuracy of the forecasts. Such forecasts are said to be of the "black or white" type. If the proportion of whites in a given period is denoted by b, and the proportions of whites and of blacks correctly forecast are denoted by c, c' respectively, then the results may be shown symbolically by Table I.

TABLE I—FORECAST CONTINGENCY TABLE

	Forecast	Not forecast	Total
White	bc	$b(1-c)$	b
Black	$(1-b)c'$	$(1-b)(1-c')$	$1-b$
Total	$bc+(1-b)c'$	$b(1-c)+(1-b)(1-c')$	1

The useful effort, E, defined as the successful proportion of forecasts of white, is given by

$$E = \frac{bc}{bc + (1-b)(1-c')}, \qquad \ldots\ldots (1)$$

while a similar expression defines the useful effort in regard to forecasts of black. In this connexion, one desires to know whether, and to what extent, forecasting constitutes an improvement on a policy of merely waiting for suitable opportunities to come along. If forecasting is to be worth-while in this respect, it is necessary that the number of successful forecasts in a given period should exceed the number of occurrences of the event; that is, one requires E to exceed b. The value of E in relation to b may therefore be taken as a measure of the usefulness, but this leads to certain difficulties to which Mr. P. M. Shaw and Mr. F. E. Lumb have drawn attention. Thus the value of E become unity when $c' = 1$, whatever the value of c; and it becomes zero when $c = 0$, whatever the value of c'. Moreover, the possible range of both E and E/b varies with b, so that two or more values of either index are not readily comparable. An alternative condition for worth-while forecasting was given in the earlier paper, where it was seen that the condition $E > b$ leads, after reduction, to

$$I_1 = c + c' > 1 \qquad \ldots\ldots (2)$$

which shows that the sum of the two forecast accuracies must exceed unity for worth-while forecasting of whites. This condition is, however, symmetrical with regard to both c and c', so that the same inequality follows from consideration of the useful effort in regard to the black occasions. The value of $c + c'$, or some function of this expression, may therefore be considered as a more appropriate index of the usefulness of the forecasts. Various other indices may also be derived to express the same condition, and their respective merits will now be considered.

Types of index of usefulness.—When the results of a set of forecasts are summarized, the contingency table takes the form of Table II.

TABLE II—SUMMARY OF RESULTS OF FORECASTS

	Forecast	Not forecast	Total
White	A	B	$A+B$
Black	C	D	$C+D$
Total	$A+C$	$B+D$	$A+B+C+D$

From this, if E relates to the forecasts of whites,

$$E = \frac{A}{A + D},$$

$$b = \frac{A + B}{A + B + C + D},$$

$$c = \frac{A}{A + B},$$

$$c' = \frac{C}{C + D}.$$

The condition for worth-while forecasting, $E > b$, then becomes

$$\frac{A}{A + D} > \frac{A + B}{A + B + C + D}.$$

This leads to an index in the form

$$I_2 = \frac{A(A + B + C + D)}{(A + D)(A + B)} \qquad \ldots\ldots (3)$$

which also will exceed unity for worth-while forecasting. Although both are derived from the same inequality, the numerical value of I_2 is in general different from that of I_1. Either of the inequalities, expressions (2) and (3), when reduced to their simplest form in terms of A, B, C, and D, leads to

$$AC > BD$$

and this gives yet another possible index,

$$I_3 = \frac{AC}{BD} \qquad \ldots\ldots (4)$$

One may, in fact, derive an unlimited number of indices of this type, since from any inequality a new one may be formed by adding the same arbitrary quantity to both sides. The problem then is to choose the one most suitable for the purpose under consideration. The general characteristics of such indices will be appreciated from a discussion of the three given above.

Consider the range of variation of each of these indices. It may be shown that each of them increases whenever c or c' increases. The lower limits of the indices therefore occur when the forecast is wrong every time, $c = c' = 0$, or $A = C = 0$; the upper limits arise when the forecast is correct every time, $c = c' = 1$, or $B = D = 0$. The following are the ranges of variation:—

for I_1, $\quad 0 \leqslant c + c' \leqslant 2$

for I_2, $\quad 0 \leqslant \dfrac{A(A + B + C + D)}{(A + D)(A + B)} \leqslant 1 + \dfrac{C}{A}$

for I_3, $\quad 0 \leqslant \dfrac{AC}{BD} \leqslant \infty$.

Of these ranges, the first is constant, the second varies with the data, and the third is infinite. In terms of b, c and c',

$$I_2 = \frac{c}{bc + (1 - b)(1 - c')}, \qquad \ldots\ldots (5)$$

so that the value of the index depends on the frequency of occurrence of the element which is being forecast. Thus for two different periods, different values of the index may be obtained even though the accuracy of forecasting is the same in both cases. Moreover the symmetry between forecasts of blacks and whites is lost with this index, and the expressions (3) or (5) derived from the forecasts of whites are not identical with the corresponding expressions for the blacks. For these reasons, I_2 does not constitute a satisfactory index.

Such considerations suggest that the most suitable index should be a symmetrical function of c and c', and independent of b. This is true of I_1 and also of I_3, which can be expressed as

$$I_3 = \frac{cc'}{(1 - c)(1 - c')}.$$

Of the two, clearly I_1 is to be preferred on account of its simplicity, its linear form, and its finite range. Since, however, it is more convenient that the range should run from 0 to 1, the index will be adopted in the form

$$I = \tfrac{1}{2}(c + c'). \quad \ldots\ldots (6)$$

It is then the mean of the two forecast accuracies; for worth-while forecasting of either whites or blacks, in the sense used here, its value must exceed one half, or 50 per cent. It will be termed the "usefulness". In terms of the elements of Table II, it may be expressed in the form

$$I = \tfrac{1}{2}\left(\frac{A}{A + B} + \frac{C}{C + D}\right). \quad \ldots\ldots (7)$$

While this index therefore gives a suitable measure of the usefulness of the forecasts, the value is in part dependent on persistence. It is necessary to proceed a stage further in order to obtain a measure of the success of forecasting, which, for present purposes, may be defined as the degree of improvement beyond the point reached by persistence. This will be considered in the next section.

Elimination of the effects of persistence.—Next consider how estimates of success in forecasting are related to persistence or repetition of the element concerned. In the extreme case, if the element were known to be a constant, no skill would be required once that fact had been observed. In ordinary cases there is a repetitive tendency in the element which is reflected in a tendency to forecast a recurrence of the event. Often this does not require much skill and is more or less independent of the particular technique used; on the other hand, where a change is expected but the time of occurrence is uncertain, much care may be exercised before a forecast of no change is issued. Generally, however, persistence is a factor which operates towards accurate forecasting irrespectively of the techniques used. In assessing the value of a technique it is therefore desirable that the effects of persistence should be removed.

Suppose then that the value of the index I or $\frac{1}{2}(c + c')$ has been obtained for a particular set of black and white forecasts. Another index I_0 can similarly be computed for the same occasions on the assumption that the forecast every time is one of repetition of the existing conditions. Then the difference $I - I_0$ would give a measure of the success of the technique used, over and above that to be obtained by use of persistence. To be more precise, we need to consider the improvement in the value of I over that of I_0 in relation to the maximum possible improvement. Since the maximum of either index is unity, this leads to the following expression* for the measure of success of a forecasting technique,

$$J = \frac{I - I_0}{1 - I_0}. \quad \ldots\ldots (8)$$

If I exceeds I_0, the value of J lies between 0 and 1. If I is less than I_0, then J is negative and may take any value; in this case the forecasting technique is worthless, since the elementary method of persistence gives better results.

The particular form in which forecasts of persistence are made will depend on the nature of the element being forecast. Forecasts of no change from current conditions will be appropriate in certain circumstances; if on the other hand there is a marked diurnal variation, a forecast of repetition after 24 hr. may be more appropriate. Again, if the element is largely influenced by advection, the persistence forecast might consist of the value of the element in a location displaced up wind by the time interval of the forecast.

It is seen then that the index J, expression (8), measures the success in forecasting beyond that which would be achieved by the application of persistence. Moreover, in virtue of the definitions of I and I_0, the forecast accuracies of both white and black occasions are treated on an equal footing, while any effects due to the frequency of occurrence of the element considered are eliminated. As regards one set of data alone, the value of the difference $I - I_0$ would be a sufficient indication, but in comparing different sets or different techniques the expression J should be used to evaluate the success.

Improvement of one technique over another.—A formula of the same type as (8) may be used as a measure of the improvement of one method of forecasting over another. If I, I' are the respective indices of usefulness, the improvement of the second method over the first, in relation to the maximum possible improvement, is given by

$$\frac{I' - I}{1 - I}. \quad \ldots\ldots (9)$$

As this expression is independent of I_0, it is unnecessary to consider persistence—unless I itself corresponds with persistence forecasting, in which case the formula reduces to (8).

Applications to black and white forecasts.—*Forecasts of change of weather type* made in a series of 4-day forecast trials have been discussed previously[2]. The accuracy of forecasts of a change of type, c, was found to be 0·53, the accuracy of forecasts of no change, c', was 0·82, hence $I = 0{\cdot}675$. If one had forecast no change of type, i.e. persistence, on every occasion, the result would have been $c = 0$, $c' = 1$, $I_0 = 0{\cdot}5$. This again illustrates that I must exceed 0·5 for the results to be worth-while. In this case the success over persistence forecasting is given by $J = 0{\cdot}175/0{\cdot}5 = 0{\cdot}35$.

Formation of secondary depressions.—Another example which was also discussed previously[2] concerns the application of Sawyer's criterion for the formation of secondary depressions at points of occlusion. The values were $c = 0{\cdot}82$ for the formation, and $c' = 0{\cdot}86$ for the non-formation of a secondary. These give $I = 0{\cdot}84$. In this example, the question of persistence is irrelevant.

*This expression resembles one proposed by Meetham[3] in which an arbitrary marking system was used for assessing terms corresponding with I and I_0.

Forecasts of frost on conductor rails.—The results of certain forecasts of the temperature of conductor rails were supplied by Mr. F. E. Lumb, Meteorological Office, Preston, from which Table III has been prepared.

TABLE III—FORECASTS OF CONDUCTOR-RAIL TEMPERATURE

Temperature	Forecast	Not forecast	Total
≤32°F.	33	8	41
>32°F.	112	13	125
Total	145	21	166

This gives $c = 0 \cdot 80$ for occurrence of frost, $c' = 0 \cdot 90$ for its non-occurrence, and $I = 0 \cdot 85$. No information is available in this case regarding persistence.

Forecasts of rain.—An analysis of forecasts of rain contained in broadcasts by the B.B.C. at 5.55 p.m. each day during a period of 12 months in 1946–47 is given by Gold[4]. The parts of the forecasts concerning rain in south-east England within the ensuing 24 hr. were checked by comparison with observations at Kew Observatory. The 267 unqualified forecasts of rain or no rain are summarized in Table IV.

TABLE IV—FORECASTS OF RAIN IN SOUTH-EAST ENGLAND

	Forecast	Not forecast	Total
Rain	167	21	188
No rain	59	20	79
Total	226	41	267

For the accuracy of forecasts of rain, $c = 0 \cdot 89$, and of no rain, $c' = 0 \cdot 75$, giving $I = 0 \cdot 82$, a high figure. Information is not given from which the corresponding value for persistence forecasting could be obtained, but the result would clearly be quite low.

Forecasting the fog point.—Corby and Saunders[5] give the results of a test of a method of predicting the temperature at which radiation fog forms. On potentially foggy occasions during the period August 1 to October 5, 1950, the fog point on the ensuing night was estimated from upper air soundings at 1500 G.M.T. by a method due to Saunders, and the results were compared with observations at a number of stations. The results were separated into two groups according to whether fog did or did not form. In the fog group, the forecast was considered successful if the predicted fog point did not exceed the actual fog point by more than 1°F.; in the other group, the forecast was considered successful if the predicted fog point was less than the night minimum temperature. Table V summarizes the findings.

TABLE V—FORECASTS OF THE FOG POINT

	Forecast	Not forecast	Total
Fog	44	11	55
No fog	78	15	93
Total	122	26	148

This example is concerned with forecasts of one type of event in two different sets of conditions, but at the time of the forecast it is not known which set applies. Since $c = 0 \cdot 80$, $c' = 0 \cdot 84$, the results show that the forecast accuracy is practically independent of the subsequent occurrence of fog. Interpreted as forecasts of fog, the usefulness is $I = 0 \cdot 82$, but this is an over-estimate, since in practice it is necessary also to forecast the night minimum temperature.

Forecasts of visibility at Northolt.—Although visibility is an element subject to continuous variation, the forecasts are conveniently considered under the head of black or white, since the position of an observation on the conventional visibility scale is usually of more importance than the precise visibility distance. Table VI contains results in regard to forecasts of the local visibility above and below certain limits; they were made four times daily between November 1951 and March 1952, the total number of forecasts being 556.

TABLE VI—FORECASTS OF VISIBILITY AT NORTHOLT
November 1951—March 1952

Visibility limit	Forecast period 1 hr. c	c'	I	$4\frac{1}{2}$ hr. c	c'	I	12 hr. c	c'	I
yd.					*per cent.*				
220	100	71	85	99	42	71	99	7	53
550	99	76	87	98	60	79	99	32	65
1,100	99	90	95	96	61	79	95	44	69
2,200	95	90	93	91	73	82	88	63	75
4,400	92	89	91	87	76	81	78	64	71
miles									
$6\frac{1}{4}$	68	99	83	37	96	67	13	95	54

c and c' are the percentage accuracies of forecasts above and below the limits indicated, and $I = \frac{1}{2}(c + c')$ is the percentage usefulness.

Among the several points of interest to be inferred from this table, attention is drawn to the following:—

(i) The forecast accuracies c and c', as well as the index I, decrease as the period of the forecast increases. This is only to be expected.

(ii) The values of c decrease, and the values of c' increase, as the visibility limit increases. This too is to be expected.

(iii) For each forecast period, the value of I is greatest for moderate limits, and least for the extreme limits.

These show that the forecasting is most useful in regard to the moderate limits with a maximum usefulness at about 2,200 yd., and that it becomes less useful as the limits become more extreme. In particular, forecasts for 12 hr. ahead for the limits of 220 yd. and $6\frac{1}{4}$ miles are only just within the useful range, for which I must exceed 50 per cent. This is due almost entirely to the very low accuracy of forecasting visibility less than 220 yd. or greater than $6\frac{1}{4}$ miles, for a 12-hr. period.

The results which would have been obtained by persistence forecasting, i.e. by making the forecast visibility always the same as the actual visibility at the time of issue, are shown in Table VII.

Comparison of Tables VI and VII shows that:—

(i) For forecasts of visibility greater than a given limit, there is little difference between c and c_0 except at $6\frac{1}{4}$ miles, where persistence gives definitely better results.

(ii) For forecasts below a given limit, c' is little different from c_0' for 1 hr. ahead, but rather better at $4\frac{1}{2}$ hr. and 12 hr. with the exception of 220 yd. at 12 hr., where persistence is the better.

TABLE VII—PERSISTENCE FORECASTS OF VISIBILITY AT NORTHOLT

November 1951—March 1951

Visibility limit	Forecast period 1 hr. c_0	c_0'	I_0	4½ hr. c_0	c_0'	I_0	12 hr. c_0	c_0'	I_0
yd.				per cent.					
220	100	71	85	98	33	65	98	29	63
550	99	86	93	97	48	73	97	24	61
1,100	98	89	93	97	63	80	95	40	67
2,200	96	85	91	91	55	73	89	47	68
4,400	90	89	89	80	73	77	70	55	63
miles									
6¼	85	97	91	52	91	71	30	84	57

For explanation see note to Table VI.

The usefulness as indicated by I or I_0 can be similarly compared, but for possible comparison with other data, the index of success, J, is computed, the values of which are shown in Table VIII.

TABLE VIII—INDEX OF SUCCESS OF VISIBILITY FORECASTING AT NORTHOLT

November 1951—March 1952

Visibility limit	Forecast period 1 hr.	4½ hr.	12 hr.
yd.		per cent.	
220	0	15	−29
550	−67	24	13
1,100	15	− 5	6
2,200	21	33	23
4,400	9	21	23
miles			
6¼	−83	−18	− 7

From this table it is seen that:—

(i) For the limit of 6¼ miles better results would have been obtained by forecasting existing conditions for each period.

(ii) For 220 yd., existing conditions would have given better results at 12 hr. ahead.

(iii) For forecasts for 1 hr. ahead, the actual results show an improvement over persistence only for the limits 1,100 to 4,400 yd.

(iv) On the whole the forecasting was most successful in regard to the middle ranges 2,200 and 4,400 yd. and least successful for the extreme ranges.

Similar data for visibility forecasts are available for a number of other stations, the checking of which can be carried out in the same manner.

(To be continued)

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ESTIMATION OF METEOROLOGICAL AVERAGES FOR OTHER MONTHS GIVEN AVERAGE VALUES FOR JANUARY, APRIL, JULY AND OCTOBER

By A. F. JENKINSON, B.A.

Introduction.—Annual and semi-annual harmonic terms can be fitted to the averages for January, April, July and October, thus giving a general expression from which averages for the remaining months may be computed. The formula may also be arranged to give monthly average values when only seasonal values, say December–February, March–May, June–August, September–November are given.

Annual variation.—In compiling data for a large number of stations to show the average annual variation of a meteorological element it is often customary to present averages for 4 mid-season months. The time required to work up data for all 12 months is usually prohibitive, especially for world maps, and data for January, April, July and October are considered sufficiently representative. Interpolation from values for these 4 months however may not be easy. Linear or graphical methods of interpolation are often quite inadequate or subjective.

Fortunately, the annual variation of most meteorological elements is usually adequately represented by two harmonics; and when annual and semi-annual terms are fitted to the averages for January, April, July and October, the values which can then be estimated for the other months are very close to the true values.

Harmonic Analysis.—For a two-term analysis $M_t = \overline{M} + a_1 \cos(30t)^\circ + b_1 \sin(30t)^\circ + a_2 \cos(60t)^\circ + b_2 \sin(60t)^\circ$ where M_t $(t = 0, 1, \ldots 11)$ are the values taken by the element in mid January, mid February, . . . mid December, regarded as nearly the average values for January, February, . . . December, and $\overline{M}$ is the average annual value.

a_1, a_2, b_1 and b_2 can be computed by least-square procedure taking

$$\overline{M} = 0\cdot25(M_0 + M_3 + M_6 + M_9).$$

Then
$$\begin{aligned} M_t = {} & M_0[0\cdot25 + 0\cdot5\cos(30t)^\circ + 0\cdot25\cos(60t)] \\ & + M_3[0\cdot25 + 0\cdot5\sin(30t)^\circ - 0\cdot25\cos(60t)^\circ] \\ & + M_6[0\cdot25 - 0\cdot5\cos(30t)^\circ + 0\cdot25\cos(60t)^\circ] \\ & + M_9[0\cdot25 - 0\cdot5\sin(30t)^\circ - 0\cdot25\cos(60t)^\circ] \qquad \ldots\ldots(1) \end{aligned}$$

Table I gives the multiplying factors for computing values for each month and for each season in terms of the values for January, April, July and October.

TABLE I—MULTIPLYING FACTORS TO COMPUTE MONTHLY AND SEASONAL VALUES FROM THE VALUES FOR JANUARY, APRIL, JULY AND OCTOBER

	Feb.	Mar.	May	June	Aug.	Sept.	Nov.	Dec.	Dec.–Feb.	Mar.–May	June–Aug.	Sept.–Nov.
Jan. ...	0·81	0·37	−0·12	−0·06	−0·06	−0·12	0·37	0·81	0·88	0·08	−0·04	0·08
Apr. ...	0·37	0·81	0·81	0·37	−0·12	−0·06	−0·06	−0·12	0·08	0·88	0·08	−0·04
July ...	−0·06	−0·12	0·37	0·81	0·81	0·37	−0·12	−0·06	−0·04	0·08	0·88	0·08
Oct. ...	−0·12	−0·06	−0·06	−0·12	0·37	0·81	0·81	0·37	0·08	−0·04	0·08	0·88

e.g. $M_{\text{Feb.}} = 0\cdot81\ M_{\text{Jan.}} + 0\cdot37\ M_{\text{Apr.}} - 0\cdot06\ M_{\text{July}} - 0\cdot12\ M_{\text{Oct.}}$

Given seasonal values for Dec.–Feb., Mar.–May, June–Aug. and Sept.–Nov., the values for January, April, July and October may be computed by using the lower part of Table I and hence those for all months from expression (1). Table II gives the multiplying factors.

TABLE II—MULTIPLYING FACTORS TO COMPUTE MONTHLY VALUES FROM SEASONAL VALUES

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
Dec.–Feb. ...	1·18	0·92	0·34	−0·13	−0·22	−0·04	0·08	−0·04	−0·22	−0·13	0·34	0·92
Mar.–May ...	−0·13	0·34	0·92	1·18	0·92	0·34	−0·13	−0·22	−0·04	0·08	−0·04	−0·22
June–Aug. ...	0·08	0·04	−0·22	−0·13	0·34	0·92	1·18	0·92	0·34	−0·13	−0·22	−0·04
Sept.–Nov. ...	−0·13	−0·22	−0·04	0·08	−0·04	−0·22	−0·13	0·34	0·92	1·18	0·92	0·34

Comparison of estimated with actual values.—*Vapour pressure.*—Table I was used to estimate values of vapour pressure for other months given the values for January, April, July and October. The results are shown in Table III.

TABLE III—COMPARISON OF ESTIMATED VALUES OF VAPOUR PRESSURE WITH ACTUAL VALUES

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
	millibars											
ATHENS, 1901–40												
Estimated value		8·1	8·9		12·5	14·5		16·4	15·9		12·3	10·0
Actual value	8·5	8·5	9·2	10·5	12·8	15·2	15·9	16·0	15·1	14·4	12·0	10·0
Difference		−0·4	−0·3		−0·3	−0·7		+0·4	+0·8		+0·3	0·0
NAPLES, 1866–1925												
Estimated value		7·8	9·4		14·3	17·5		19·2	17·4		11·9	9·5
Actual value	8·1	8·4	9·2	11·2	13·9	17·2	19·3	19·6	17·6	14·7	11·1	9·2
Difference		−0·6	+0·2		+0·4	+0·3		−0·4	−0·2		+0·8	+0·3
BEIRUT, 1933–52												
Estimated value		11·5	13·2		20·6	24·0		26·1	24·0		17·0	13·6
Actual value	11·7	11·9	13·5	16·3	20·0	23·3	26·1	26·5	24·3	20·7	16·8	13·2
Difference		−0·4	−0·3		+0·6	+0·7		−0·4	−0·3		+0·2	+0·4

Wind.—Table II was used to estimate monthly values for wind direction given seasonal values. An example is shown in Table IV.

TABLE IV—PERCENTAGE WIND FREQUENCIES AT RHODES, 1400 ZONE TIME, DURING THE PERIOD 1933–1940

Wind direction	Seasonal frequencies Dec.–Feb.	Mar.–May	June–Aug.	Sept.–Nov.	Monthly frequencies, November Estimated	Actual
	per cent.				*per cent.*	
N.	10	3	0·5	7	10	13
NE.	4	2	0·5	1	2	2
E.	5	5	0·3	2	3	2
SE.	22	16	2	14	19	20
S.	10	6	0·4	4	7	6
SW.	7	8	16	7	5	3
W.	11	32	66	41	26	22
NW.	23	22	13	19	21	23
Calm	8	6	1	5	7	9

Temperature.—Table I was used to give estimated values for average daily maximum temperature for other months given those for January, April, July and October. The results are shown in Table V.

TABLE V—COMPARISON OF AVERAGE DAILY MAXIMUM TEMPERATURE WITH ESTIMATED VALUES

	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
NAPLES, 1901–25	*degrees Centigrade*											
Estimated value		11·1	13·4		21·8	26·1		27·7	24·6		16·2	12·7
Actual value	11·0	12·0	14·1	17·2	21·8	25·3	28·2	28·3	25·2	20·4	15·5	12·2
Difference		−0·9	−0·7		0·0	+0·8		−0·6	−0·6		+0·7	+0·5
KEW, 1871–1950	*degrees Fahrenheit*											
Estimated value		44·7	48·8		62·2	68·5		69·4	64·6		50·2	45·1
Actual value	43·8	45·1	49·3	55·0	62·0	67·8	71·1	70·1	65·0	56·6	49·0	44·6
Difference		−0·4	−0·5		+0·2	+0·7		−0·7	−0·4		+1·2	+0·5

AN EXTENSIVE DOUBLE TROPOPAUSE

By D. H. JOHNSON, M.Sc.

J. Bjerknes and Palmén in a paper[1] which has become one of the classics of aerological analysis, commented on the multiple tropopause which occurred on a series of ascents made at short intervals from Uccle on February 15 and 16, 1935. The multiple tropopause was defined as a system of successive changes in lapse rate (double, triple etc.), occurring in the boundary region between troposphere and stratosphere. It was implied that such a system should be recognizable from one ascent to another in time or space, and it was suggested,

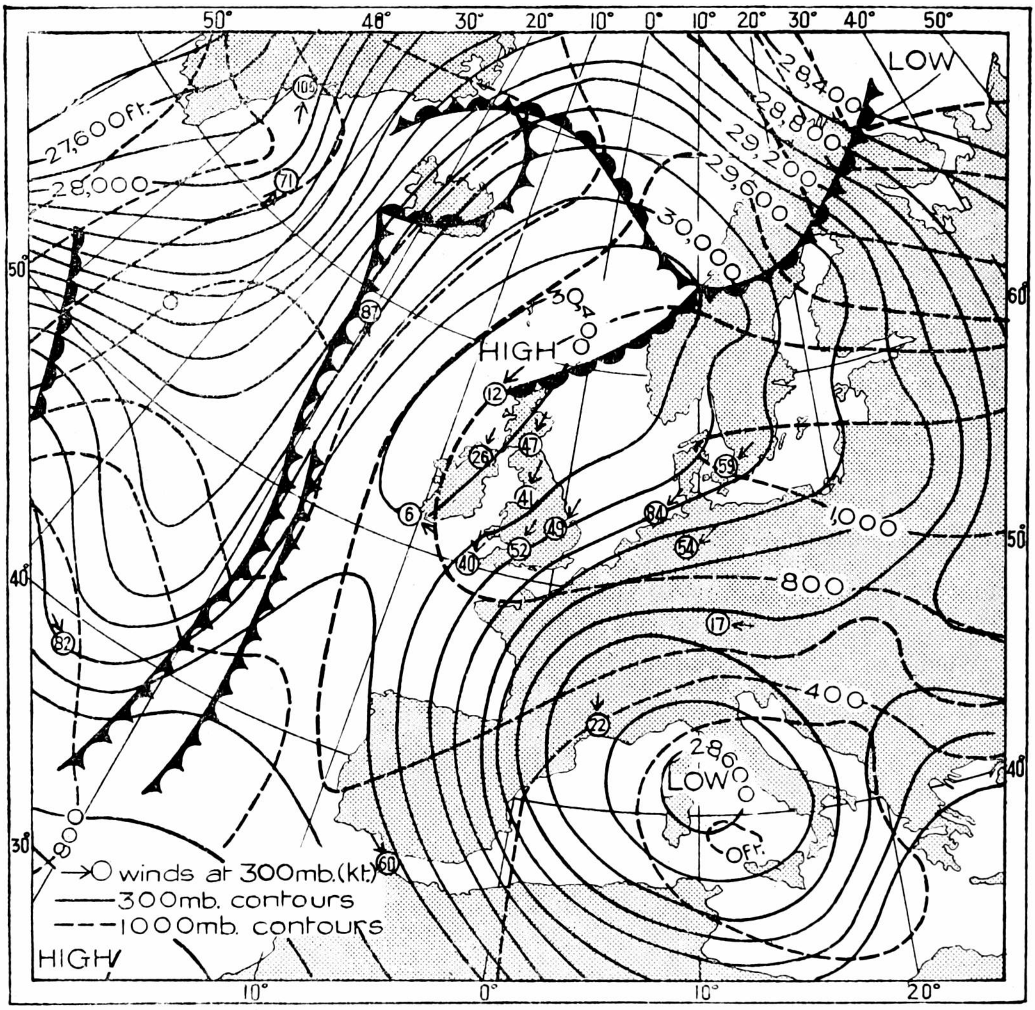

FIG. 1—CONTOURS OF THE 1000-MB. AND 300-MB. SURFACES AT 1500 G.M.T., JANUARY 19, 1950

as a working hypothesis, that the potential temperature is relatively constant over each tropopause layer, although the height of the layer varies. Although complexities in the structure of the tropopause are frequent, it is unusual for a multiple tropopause to be identifiable over a wide area, and the following account of a particularly extensive double tropopause may be of interest. It extended from Iceland to central Europe and from northern Norway to ocean weather station JULIET (54°N., 18°W.).

Fig. 1 shows the 1000-mb. and 300-mb. flows at 1500 G.M.T., January 19, 1950. The westerlies which had prevailed for the earlier part of the month were becoming blocked, with the establishment of a cold pool in the Mediterranean and an upper high to the north-west of the British Isles which developed towards Scandinavia. At the surface, the continental anticyclone, with a ridge extending across the British Isles, gave Europe a seasonable spell of winter easterlies.

The double tropopause appeared on many of the 1500 G.M.T. ascents made from stations in western Europe, Scandinavia and the north-east Atlantic on this occasion. Fig. 2 contains tephigrams of the upper parts of several of these ascents. A double tropopause can be found without difficulty on each of those illustrated with the possible exception of that from Larkhill where the upper tropopause is not very well defined. Examination of the original plots of frequency against time for signals received from the temperature elements of the British radio-sondes confirmed that each of the apparent tropopauses was a real singularity; they did not arise accidentally because temperatures were reported only at a discrete set of pressures in regions where the lapse rate might have been varying continuously with pressure over a fairly thick layer.

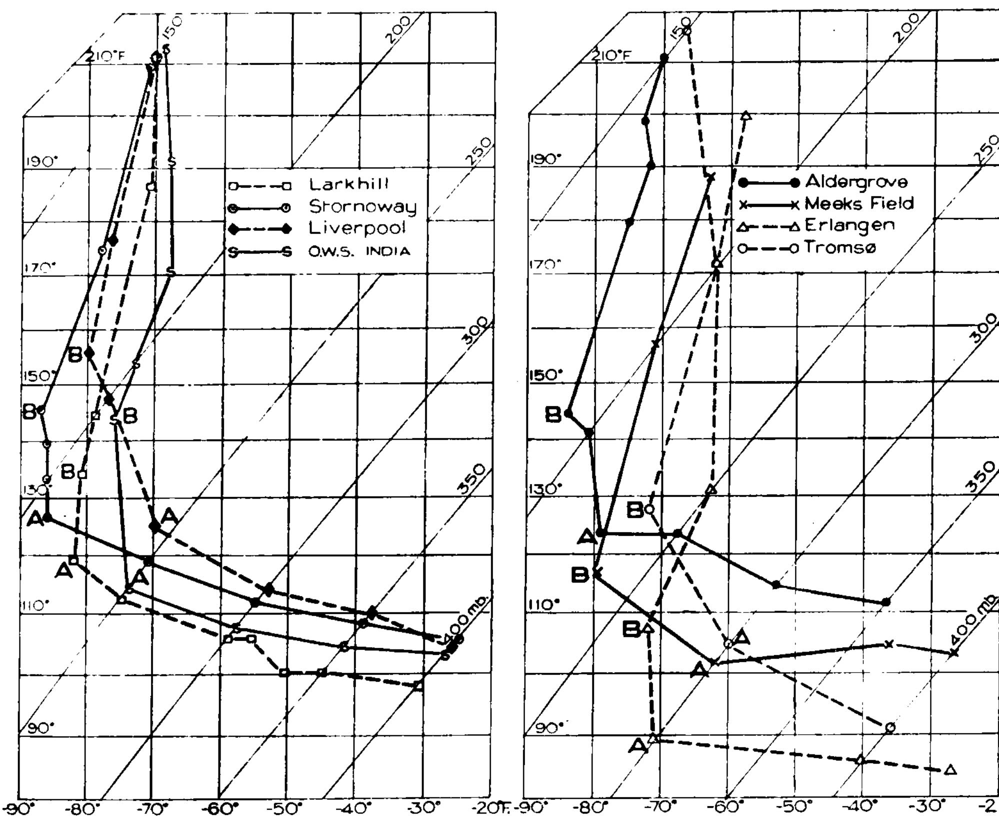

FIG. 2—TEPHIGRAMS OF THE UPPER PARTS OF ASCENTS MADE AT 1500 G.M.T., JANUARY 19, 1950

From the ascents in the left-hand tephigram in Fig. 2, it is not unreasonable to associate the lower of the singularities (marked A) as belonging to one tropopause surface and the upper singularities (marked B) as belonging to a second tropopause surface. The ascents shown in the right-hand tephigram, however, were made at stations which are much farther apart than those whose ascents appear in the left-hand one, and, while each ascent does contain two singular points, it is not clear from inspection of this tephigram that all the

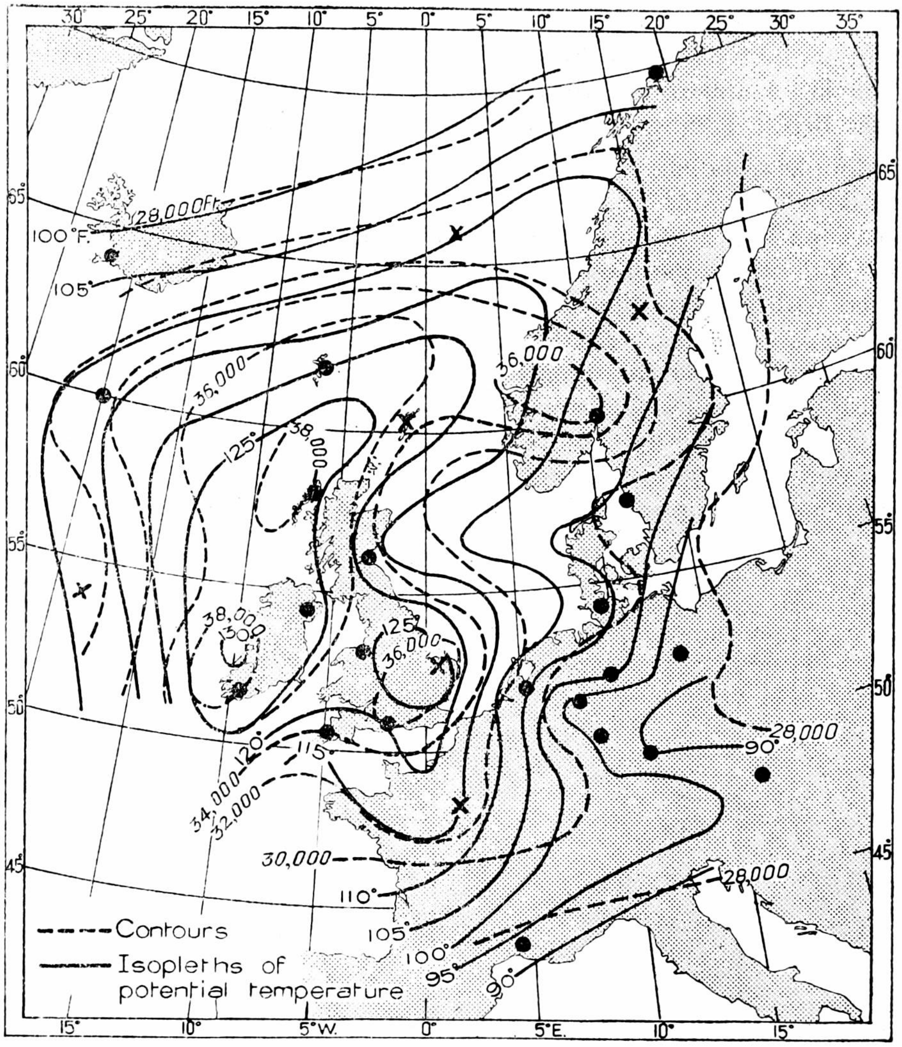

FIG. 3—ISOPLETHS OF POTENTIAL TEMPERATURE AND CONTOURS OF THE LOWER TROPOPAUSE

● Stations showing the double tropopause clearly.

X Stations where ascents failed to reach both tropopauses or with three possible tropopauses.

points A can reasonably be taken to be on one tropopause surface and all the points B to lie on a second distinct tropopause surface. Such an analysis, however, becomes more plausible if the space distributions of height, potential temperature and temperature at the two singularities are considered. This has been done in two ways.

First, isopleths were drawn of the heights and potential temperatures of each singularity. These are reproduced in Figs. 3 and 4 which show, in addition, the location of those stations whose ascents have been examined. Ascents from stations located by a dot showed the double change of lapse clearly and unambiguously. Of the ascents from stations marked as a cross, those from the ship at station METRO (66°N., 2°E.) and Östersund-Frösön (63°N., 14°E.) reached only the lower tropopause. The ascent from the weather ship station INDIA (53°N., 18°W.) did not reach tropopause levels at 1500 G.M.T., but a double change of lapse showed clearly at 0900 G.M.T. and 2100 G.M.T. Ascents

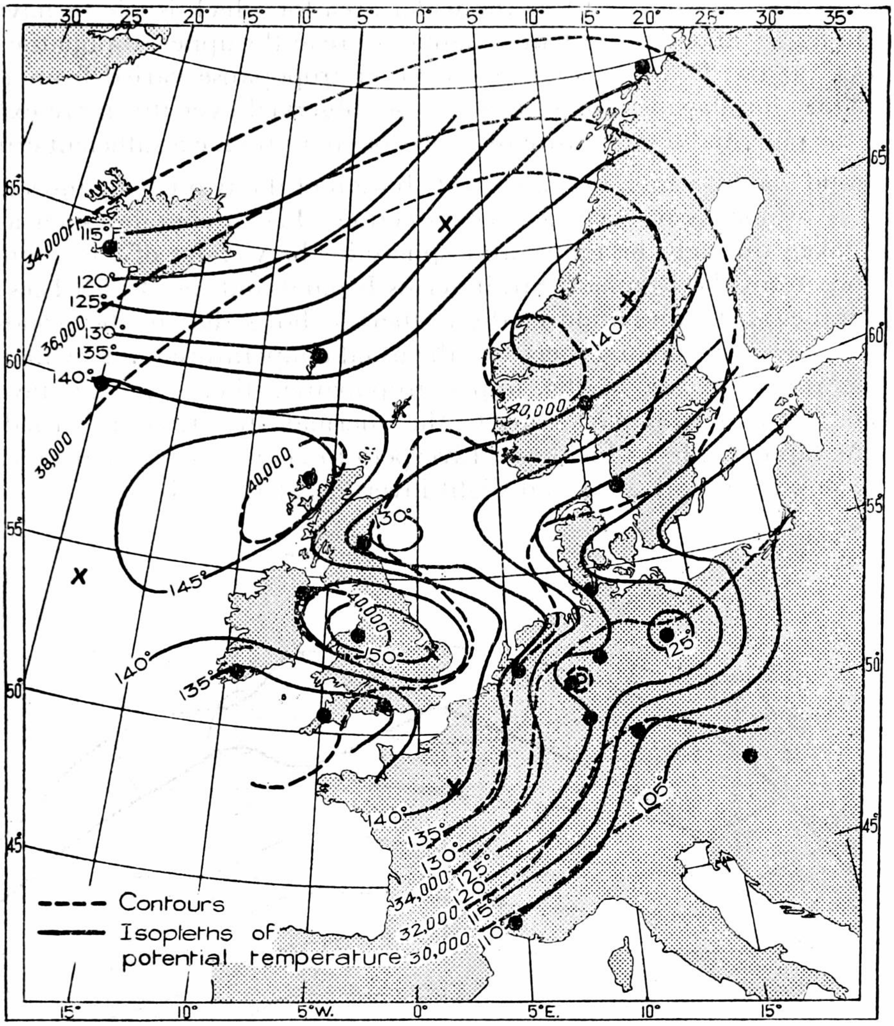

FIG. 4—ISOPLETHS OF POTENTIAL TEMPERATURE AND CONTOURS OF THE UPPER TROPOPAUSE

● Stations showing the double tropopause clearly.

X Stations where ascents failed to reach both tropopauses or with three possible tropopauses.

from Downham Market (53°N., 0°E.), Lerwick (60°N., 1°W.), Trappes (49°N., 2°E.) and Jever (54°N., 8°E.) all contained three points which might be considered as singularities. For each of these last ascents, two of the singularities were selected as being most likely to correspond to the widespread tropopauses, consideration being given to the general character of the ascent in relation to features of ascents from neighbouring stations.

Comparing Figs. 3 and 4 with Fig. 1 it is seen that the heights and potential temperatures at each tropopause were greatest in the central region of the upper anticyclone and decreased towards its periphery. The potential temperatures at each tropopause were reasonably uniform only over limited regions. For stations in the British Isles, for example, the potential temperatures reported at the upper tropopause varied from 154° to 133°F. and at the lower tropopause from 132° to 115°F. It is therefore just possible on this occasion to use potential temperature as a means of deciding to which layer a tropopause observed over the British Isles belonged. Over the wider area for which isopleths have been drawn in Figs. 3 and 4, the potential temperature at the upper tropopause varied from 154° to 103°F. whilst that at the lower tropopause varied from 132° to 90°F. Thus, for classification of tropopauses observed over this more extensive region, the potential temperature by itself does not provide a sufficient criterion.

A second way in which the space distribution of the two tropopauses may be studied is by means of a vertical cross-section. Fig. 5 contains such a cross-section taken through the upper atmosphere in the vicinity of the tropopause. It extends from the Denmark Strait across Iceland and the British Isles to the Mediterranean. Examination of the isotherms shows that in the troposphere below the lower tropopause there was the usual temperature lapse, whilst in the lower stratosphere above the upper tropopause, there was an inversion. Between the two tropopause sheets the thermal stratification varied from moderate lapse at Meeks Field and Liverpool to near isothermal at Stornoway and the ship at station INDIA and slight inversion at Larkhill.

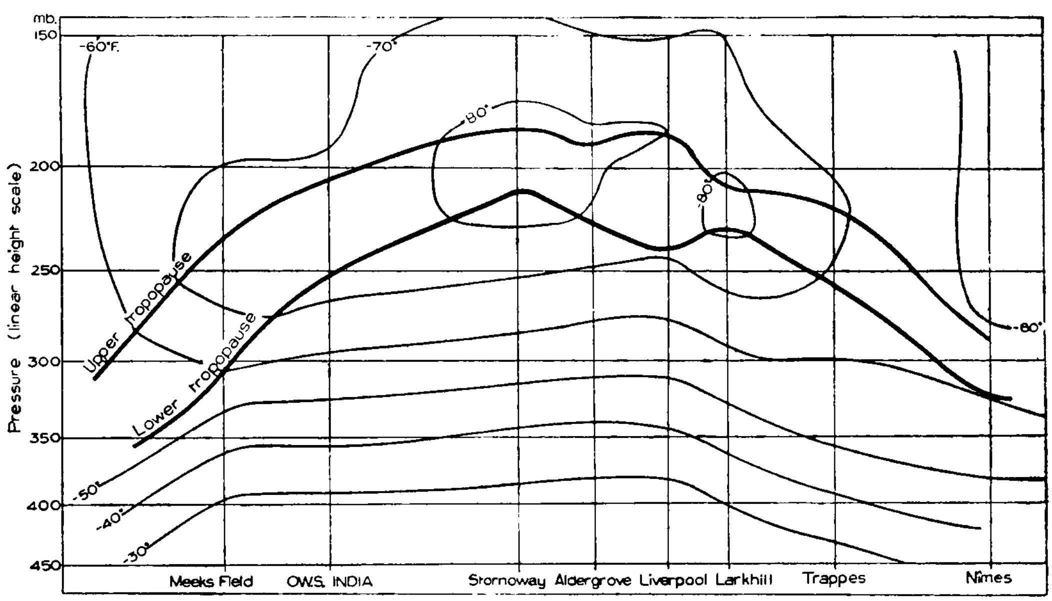

FIG. 5—CROSS-SECTION FROM THE DENMARK STRAITS TO THE MEDITERRANEAN THROUGH THE UPPER TROPOSPHERE AND LOWER STRATOSPHERE

It is interesting to apply to the ascents used in the construction of the cross-section a definition of the tropopause which does not recognize the existence of multiple singularities. Such a definition is given in the introduction to the *Daily Aerological Record*[2]. Three types of tropopause are defined, namely

Type I. The tropopause is indicated by a definite inversion.

Type II. There is no inversion, but a sharp discontinuity, the lapse rate above the discontinuity being 1°F./1,000 ft. or less.

Type III. There is neither an inversion nor a discontinuity, but the lapse rate falls gradually to a value less than 1°F./1,000 ft. The point where the lapse rate drops to the value is taken as the tropopause.

Using this definition we are obliged to take as the tropopause the lower singularities at Larkhill (Type I), Stornoway (Type II) and the ship at station INDIA (Type II), and the upper singularities at Meeks Field (Type I), Aldergrove

READING THE BAROMETER

Also to be seen are the Gold slide, an oil-damped barograph and a cloud card. The Gold slide is used for reducing the pressure as read to sea level and at the same time allowing for index error, latitude and temperature.

READING THE SCREEN THERMOMETERS

It will be seen that the screen is only temporarily secured in its present position, since it should always be hung on the windward side of the ship.

OBSERVATIONS ABOARD A "SELECTED" SHIP

(see p. 91)

[To face p. 80

OBSERVING THE WIND DIRECTION

The direction of movement of wave ripples or spume streaks is sighted over the gyro compass, thus obtaining the wind direction free from the necessity of any correction for ship's course and speed.

COPYING THE CODED MESSAGE ON TO THE SIGNAL PAD

OBSERVATIONS ABOARD
(see

READING THE SEA TEMPERATURE

The special bucket (Meteorological Office pattern, Mk III) is trailed through the water and the temperature of the resulting "catch" is measured as seen in the photograph.

TRANSMITTING THE MESSAGE

A "SELECTED" SHIP
p. 91)

Reproduced by courtesy of the British Ship Adoption Society

Reproduced by courtesy of James Hall Ltd., and the British Ship Adoption Society

SCHOOL PARTIES VISITING THE O.W.S. *Weather Watcher* IN JUNE 1952 AND MAY 1953

The *Weather Watcher* has been adopted by the Eastbank Secondary School, Glasgow. The lower photograph shows Capt. Elston presenting a silver trophy to the Headmaster of the school.

(Type I) and Liverpool (Type I). A definition which is convenient for statistical purposes, is not necessarily satisfactory from the point of view of aerological analysis. The problem of revising the definition of the tropopause to meet this and other difficulties is under consideration.

REFERENCES

1. BJERKNES, J., and PALMÉN, E.; Investigations of selected European cyclones by means of serial ascents, Case 4: February 15–17, 1935. *Geofys. Publ., Oslo*, **12**, No. 2, 1937.
2. London, Meteorological Office. Introduction to the *Daily Aerological Record.* Published quarterly.

LENGTH OF A FROST-FREE PERIOD

By L. P. SMITH, B.A.

In agricultural meteorology, questions are sometimes asked with regard to the average length of the frost-free period in a given area. The difficulty about such requests is that minimum air temperatures (at screen height) are of limited validity unless taken on the actual site, and that the simple long-term average gives a very incomplete picture.

As an example, the dates of first and last screen frost (minimum 32°F. or below) were extracted for Rothamsted for the years 1921–50. The date of the last frost varied from March 23 to May 29 with April 29 as the average. Similarly the first autumn frost lay between October 11 and November 15, with the average at October 26. By subtraction, the length of the frost-free period in any year ranged from 148 to 230 days, average length 180 days.

At Rothamsted during this period, the variation in the date of last frost in spring was greater than the variation in the date of the first frost in autumn. It is not therefore surprising that the longest frost-free periods occurred in 1943 (230 days) and 1942 (221 days) when the dates of the last spring frost were abnormally early, namely on March 23 and March 29 respectively. Conversely, with a very late spring frost, such as May 29 in 1927, the frost-free period was the shortest in the 30 yr. (148 days). The next shortest was in 1941 (150 days) when the similar date was May 16.

An average by itself is thus of limited use and we need a simple way of expressing the variation about the average. On the evidence available, the shortest period in 30 yr. was 148 days, implying that the odds were 29–1 against a period of 148 days or less. The next shortest was 150, with odds 14–1 against for a period of this length or less. The longest but one was 221 days, with odds 0·034–1 against (or 29–1 on) a period of this length or less; this is perhaps better stated as odds of 29–1 against a period longer than 221 days.

If these odds are plotted against the length of period, a logarithmic curve is obtained (see Fig. 1), and the equation of the closest fit is

$$\text{length} = (178{\cdot}6 - 24{\cdot}2 \log C) \text{ days}$$

where C is the odds against unity of a frost-free period of the given length or less. From this it can immediately be seen that there is an even chance of at least 178·6 days, a 10–1 against chance of 154·4 days or less, and a 10–1 against chance of more than 202·8 days. Table I shows the observed and calculated values. It is perhaps unfair to compare these figures with those recorded in the nearby frost hollow at Rickmansworth, which were listed by E. L. Hawke[1]. Nevertheless, if we do so, some interesting points emerge. The comparable data are given in Table II.

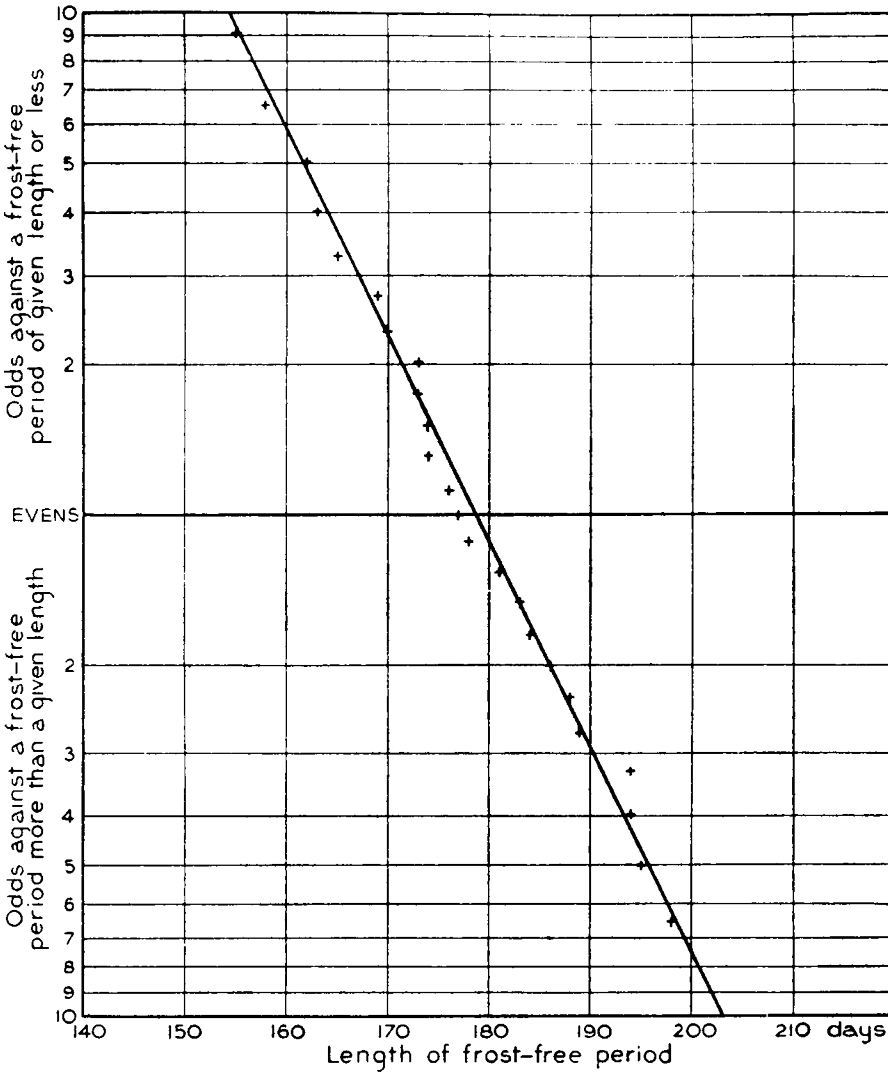

FIG. I

TABLE I—MAXIMUM LENGTH OF FROST-FREE PERIOD IN THE LEAST FAVOURABLE YEARS IN 10

	Observed	Calculated	Error
		days	
1 year in 10	155	155·5	0·5
2 years in 10...	163	164·0	1·0
3 years in 10...	170	169·7	0·3
4 years in 10...	174	174·3	0·3
5 years in 10...	177	178·6	1·6
6 years in 10...	183	182·9	0·1
7 years in 10...	188	187·5	0·5
8 years in 10...	194	193·2	0·8
9 years in 10...	200	201·7	1·7

Not only is the length of frost-free period much shorter at Rickmansworth, it is also more variable; the comparable length-odds equation is

$$\text{length} = (97{\cdot}3 - 31{\cdot}7 \log C) \text{ days.}$$

There is no correlation between the last two columns of figures in the above

TABLE II

	Date of last frost Rothamsted	Date of last frost Rickmansworth	Date of first frost Rothamsted	Date of first frost Rickmansworth	Length of frost-free period Rothamsted	Length of frost-free period Rickmansworth
1930	Apr. 22	May 9	Oct. 27	Oct. 10	188	153
1931	Apr. 22	May 21	Oct. 22	Sept. 7	183	108
1932	May 8	June 10	Oct. 29	Sept. 26	174	107
1933	Apr. 23	May 27	Oct. 18	Oct. 13	178	138
1934	Apr. 10	May 25	Oct. 30	Aug. 26	202	92
1935	May 19	June 9	Oct. 21	Aug. 28	155	79
1936	Apr. 23	June 4	Oct. 29	Sept. 29	189	116
1937	Apr. 27	June 30	Oct. 18	Sept. 16	174	77
1938	May 8	June 11	Oct. 24	Aug. 21	169	70
1939	Apr. 28	July 2	Oct. 26	Sept. 28	181	87
1940	Apr. 18	May 21	Oct. 29	Aug. 24	194	94
1941	May 16	May 21	Oct. 13	Sept. 17	150	118
1942	Mar. 29	June 26	Nov. 5	Sept. 16	221	81
Mean	Apr. 29	June 5	Oct. 25	Sept. 15	181	101

table, as the coefficient is approximately zero. In 1942, when Rothamsted had one of its longest frost-free periods (221 days) Rickmansworth had one of its shortest (81 days). Indeed, it would have been surprising if any uniform pattern of relationship had been found between the two sets of figures.

It is suggested that the following conclusions must be drawn:—

(1) A meteorological average must be supported by some indication of the scale of variation about that average.

(2) A meteorological average is only valid for the site on which the observations were taken; any extrapolation must be done with extreme care, especially when dealing with parameters involving threshold values.

(3) The representation of frost statistics by isopleths on small-scale maps is of limited practical use.

REFERENCE

1. HAWKE, E. L.; Thermal characteristics of a Hertfordshire frost-hollow. *Quart. J. R. met. Soc., London*, **70**, 1944, p. 23.

METEOROLOGICAL OFFICE DISCUSSION

Frontal analysis in the higher troposphere and the lower stratosphere

The discussion on Monday, December 21, 1953, held at the Royal Society of Arts, was opened by Mr. C. L. Hawson who based his statement on the paper:—

BERGGREN, R.; On frontal analysis in the higher troposphere and the lower stratosphere. *Ark. Geofys., Stockholm*, **2**, 1953, p. 13.

Mr. Hawson said that Berggren's paper dealt with the problem of how to combine in a vertical cross-section, the extended front, the two or more tropopauses usually seen on vertical soundings, and the discontinuities observed in the stratosphere.

From observations in November 1949 Berggren constructs idealized soundings representing tropical and polar air. The temperature difference between these two air masses increases with height to about 400 mb. Above the tropopause the polar air is isothermal, and at about 250 mb. both air masses have the same temperature. Above this level the polar air is warmer than the tropical air and the temperature difference increases to about 130 mb.

Berggren also observes that a decided northward movement of tropical air is often connected with strong winds in the higher troposphere, but that the wind speed usually decreases rapidly above about 9 Km. He therefore believes it may be reasonably assumed that, as seen in a vertical cross-section, the tropical air invades the polar air as a wedge with its axis at about 9 Km. Further, Berggren considers that the air immediately above the tropopause has in many cases the same geographical origin and the same life history as the air in the higher troposphere. The tropopause, he concludes, ought not to be regarded as a boundary between two different air masses, but as a phenomenon localized within one air mass.

From these hypotheses, Berggren puts forward a frontal model in which both tropical and polar air are regarded as high-reaching air masses separated by the polar front, which can generally be assumed to reach well up into the stratosphere. In those cases where the polar front is a warm front in the troposphere, it acts as a cold front in the stratosphere, and *vice versa*. At the level of zero temperature difference, the front is vertical, and the horizontal wind shear through the front attains its maximum value. A reasonable value for this shear seems to be about 40–50 m./sec./100 Km.

Berggren analyses specific fronts and shows them to be not inconsistent with his model, making particular reference to the following points:—

(i) distribution of wind in the layer between 400 and 200 mb., especially the horizontal shear at about the level of vanishing temperature difference between tropical and polar air;

(ii) distribution of temperature between 500 and 100 mb.;

(iii) vertical stability on both sides of the front between 400 and 200 mb.;

(iv) change in time of wind and temperature in the layer between 500 and 100 mb. when the polar front passes a station.

To illustrate Berggren's model Mr. Hawson presented Berggren's detailed analysis of the Atlantic warm front of November 7–8, 1949 with the aid of cross-sections and showed Berggren's model for a warm-front occlusion. He particularly emphasized the scale of frontal zones, 70–150-Km. wide in Berggren's example, and quoted J. S. Sawyer's suggestion of an average 130 miles (200 Km.) with wide variations.

In addition to the discussion of particular examples Berggren constructs mean soundings, typical of each 100 Km. either side of a front at 500 Km. He uses these soundings to produce a mean vertical cross-section through a warm front, and also to obtain idealized soundings of tropical and polar air. Using these idealized soundings, he constructs an idealized frontal model with uniformly sloping fronts which he discusses in relation to real cases. The following points emerge:—

(1) The maximum temperature difference between idealized polar and tropical air, found at about 400 mb., should give the jet-stream centre above the point where the central part of the frontal layer intersects the level, if all the baroclinity is concentrated to the front. A displacement to a lower layer is usually found, brought about by the distribution of baroclinity and sometimes by the wind distribution in the lower layers.

(2) A dip in the lower tropopause of the tropical air towards the front is often observed. This causes a rather great horizontal temperature gradient, which, having the opposite direction to the one in the troposphere, entails a rapid decrease of the wind speed above the tropopause level. At the lower tropopause of the polar air, the opposite phenomenon may occur, and a wind increase ensues.

(3) The position of the stratospheric part of the front changes considerably with respect to the position of the front at 500 mb. Berggren does not suggest that his model is applicable to all cross-sections but he thinks his special solution is rather common. He points out that every part of a high-reaching front is subject to frontolytic processes. This is especially the case near the surface of the earth and in the stratosphere. In the first case the destruction of the front is brought about by the exchange of heat between the air and the surface of the earth and in the latter case by the vertical motions. Although only an indirect reference to the problem is made in his main paper, Berggren in his preliminary paper suggests that clear-air turbulence might be associated with the frontal zones of his model, and claims that his model may provide a possible way of attacking the prognostic problem.

Mr. Hawson, in giving his own opinion of Berggren's paper, said that studying the paper left him with the impression that the examples had been carefully selected, but that even so the observations had been stretched to their limits. In the detailed example for which Berggren's analysis had been given earlier, he showed that some of Berggren's tropopauses do not accord with current British practice; and that the routine British tropopause analysis placed the tropopause of the tropical air coincident with the tropical boundary of Berggren's stratospheric front in the only two places where this front was directly observed.

Nevertheless he believed that as a working hypothesis for an experienced meteorologist Berggren's model is an aid to clear visualization of the atmosphere, whether the boundary zones be called tropopauses, fronts, or sloping baroclinic zones. Broad divisions of the kind described by Berggren do exist, and the possibility of the occasional occurrence of clear-cut surfaces of a frontal nature in the lower stratosphere cannot be ruled out. Far more frequently, however, the frontal surface is not clear cut even in the higher troposphere, and detailed frontal analysis becomes too subjective to be profitable to the forecaster.

Observations of the Meteorological Research Flight can throw light on the detailed frontal structures, and although no flights have so far been undertaken specifically to explore stratospheric fronts, recent papers of the Meteorological Research Committee by J. S. Sawyer and R. Murray, giving analyses of the observations of meteorological research flights in the vicinity of fronts and jet streams, make illuminating reading. These authors do not carry frontal analysis into the stratosphere, and indeed consider their detailed observations in many cases do not justify carrying the frontal analysis into the higher troposphere.

Mr. Bannon said he had read Berggren's preliminary paper* with interest. He had been struck by the fact that, in the examples given, no two simultaneous ascents pass completely through the stratospheric part of Berggren's front. The same could be said of the examples shown this afternoon. The observational

*BERGGREN, R.; The distribution of temperature and wind connected with active tropical air in the higher troposphere and some remarks concerning clear air turbulence at high altitude. *Tellus, Stockholm*, **4**, 1952, p. 43.

evidence for Berggren's analysis he thought was thin. Could the opener say if Berggren gave any better examples? Turning to the question of Berggren's tropopause analysis Mr. Bannon said that when the preliminary paper was published he wrote* pointing out that the tropopause analysis did not accord with the definitions in use in the British Meteorological Office. Berggren agreed this was so, but said the limiting lapse rate of 2°C./Km. in current use is quite arbitrary and carries no great weight. In conclusion Mr. Bannon said there was no doubt that a good deal of clear-air turbulence occurs in the regions indicated by Berggren, but a great deal also occurs elsewhere. In his view it was wrong to attempt to tie the turbulence to the Berggren front, and the prognostic problem was mainly one of forecasting upper wind shears. Mr. Hawson, in reply, said that from the point of view of observations the best example of a cross-section in Berggren's papers is that of the warm front over the British Isles at 0300 on November 9, 1949, given in the preliminary paper.

Mr. Veryard asked if Berggren gave his definition of the tropopause. Mr. Hawson replied that he did not know of any special definition proposed by Berggren; none was given in the paper.

Mr. V. R. Coles said that on the slide showing the surface analysis at 0000 G.M.T. on November 8 he noticed a frontal system to the north-east of Iceland much nearer to Keflavik than the Atlantic warm front for which the detailed analysis was given. Would not the other frontal system affect Berggren's upper air analysis? He was also unhappy about the magnitude of the discrepancy between the winds as observed and as adopted by Berggren at 200 and 170 mb. at 0300 on November 8 at ocean weather station J. In reply, Mr. Hawson said that Berggren does not give a surface analysis for 0000 on November 8, only one for 0300 on the 9th which serves for other cross-sections as well. This is not the most convenient for the example under review. He had therefore prepared the slide shown from the 0000 chart for November 8, 1949, published in the *Daily Weather Report.* As Mr. Coles so rightly noticed, this contains the additional frontal system to the north-east of Iceland which might well complicate the analysis. This frontal system is a very old one, however, with an inverted warm sector. Berggren does not mention it; it might not even appear on Berggren's surface charts, and it was soon dropped from the British analysis. There is no sign of this frontal system on the upper air soundings, although there is evidence on the Keflavik sounding for an arctic front which does not appear on the British analysis. With reference to the winds at station J, the errors attributed to the winds at 200 and 170 mb. by Berggren are about three times the probable error of British ocean-weather-ship wind observations. An individual observation can show such a large error, especially if the observation is made at long range, as in this case, and the sea is rough. The discrepancy is certainly disturbingly large, but not impossible on instrumental grounds.

Mr. Peters wondered whether the frequency and accuracy of upper air observations were sufficiently high to justify conclusions being drawn about the finer structure of troposphere and stratosphere. He thought it important that the principle of continuity in tropopause analysis should be recognized and studied, and suggested that, in the daily routine of upper air analysis, tropopauses determined by assistant staff should be checked by upper air analysts for

*BANNON, J. K. and BERGGREN, R.; Jet streams and clear air turbulence. *Tellus, Stockholm,* **4,** 1952, p. 385.

consistency with the evolution of the general situation. He considered that many of the doubtful assessments of tropopause depending on small changes in lapse rate would thereby be eliminated, whilst some, that now result from an uncritical application of the existing definitions and have little or no physical significance, would be suppressed. The problem of devising new definitions of the tropopause had been under study in the Meteorological Office for several years past, and it was now being examined by the World Meteorological Organization. He hoped a satisfactory solution would not be unduly delayed.

Mr. Matthewman said he had drawn a number of cross-sections through fronts while at Dunstable. He thought the general pattern of the isotherms around a well marked warm front were often very similar to those displayed by Berggren, but there was insufficient evidence to say that the pattern was exactly like Berggren's around and above the tropopauses. There was evidence of well marked fronts in the upper troposphere on occasions. Meteorological research flights suggested a frequency of about one such front a month in the vicinity of the British Isles. Another model analysis he had found useful, was one in which the boundaries of the frontal zone were extended to link with the tropopauses of the two air masses. The model looked rather like a waterspout.

Mr. Sawyer said he thought that analyses such as Berggren's were useful for fixing ideas, even if only as a basis for discussion. The small-scale fluctuations of temperature found in the atmosphere made objective analysis difficult, since these were easily capable of masking the large-scale structure. Small-scale temperature changes of the order 5°F. in 6–10 miles had been observed by the Meteorological Research Flight on several occasions. In some cases the irregularities of the temperature profile, observed by research flights through fronts, were such that an analysis into a definite frontal zone containing a strong temperature gradient separating two more or less homogeneous air masses represented a considerable idealization of the observed curve, and could not be carried out objectively. In other cases the analysis in terms of a frontal zone appeared simple and natural. He was unconvinced by Berggren's analysis.

Mr. Gold said he first wished to thank the Director for his kind invitation for guests to attend Meteorological Office Discussions, a privilege which is very much appreciated. On behalf of the guests he extended sincere Christmas and New Year good wishes to the Director and members of the Meteorological Office. Mr. Gold recalled that he had been "in" at the birth of fronts and said he was now beginning to wonder if he were present at their death. He thought in tropospheric analysis it is essential to consider the synoptic history, and that rigid definitions of the tropopause are inappropriate in research work. The British tropopause definitions were drawn up 40 yr. ago, when upper air observations were as far as a month apart and relatively little was known of the upper atmosphere. The term tropopause was introduced to describe something they found in their early observations. If modern observations show that such a phenomenon does not always exist, it is wrong to insist on labelling a tropopause on each ascent. He suggested that the humidity observations should be used to assist in the identification of the tropopause. Turning to Berggren's paper, he asked if the winds were in agreement with the temperature gradients, and if not, which should be believed. Further, he was unconvinced by Berggren's statement that the trajectories of the air immediately above and below the tropopause are often similar. Wherever the tropopause slopes, there is a more or less rapid change of wind from the upper troposphere to the lower stratosphere. Air masses in these layers which today are in juxtaposition, will have been yesterday 500–1,000 miles apart. Mr. Hawson, in his reply, said that as temperature falls the lag of the humidity unit on the British radio-sonde becomes greater and the observations decline rapidly in value. The decline is such that when the temperature falls below a certain value (−40°F.) reporting of the humidity observations is discontinued. Humidity observations therefore usually fail to provide a routine basis for tropopause analysis. With the exception of the winds at 200 and 170 mb. at station J already mentioned, the winds and temperature gradients in Berggren's analysis are in good agreement. He wished to emphasize that Berggren himself drew attention to the discrepancy. The general question of conflict between observed winds and observed temperature or contour gradients is a broad one, depending upon the distribution of the observing stations, the type of the instruments used and the altitude. For British land stations the probable error of a wind observation is about 5 kt. and changes little with height unless extreme range is reached. British ocean-weather-ship wind observations have probable errors two or three times greater. For British radio-sonde contour-height observations, the random probable error increases with altitude and is about ± 50 ft. at 300 mb. For stations spaced as in Great Britain and a westerly wind the corresponding probable error of the geostrophic wind between two adjacent stations is between about 20 and 30 kt. At 100 mb. the corresponding probable errors are about twice as great. The problem of the trajectories of air above and below the tropopause is a difficult one to settle. He had examined a few examples of track analysis available in the research section at Dunstable and found some support for Berggren's claim. The closer one approached the tropopause the easier it was to believe the claim. He fully agreed with Berggren's hypothesis that the tropopause should not be regarded as a boundary between two different air masses but as a phenomenon localized within one air mass.

Dr. Farquharson referred to the fact that a number of years ago he had used PAMPA records in a study of fronts, and had been disappointed to find how rarely a front could be traced on upper air records above about 10,000 ft. Mr. Matthewman when working in the Forecast Research Division on fronts had given the impression that he also found it hard to find fronts above about 10,000 ft. He (Dr. Farquharson) was not convinced by Berggren's case for fronts in the stratosphere.

Mr. Gold said he was still unconvinced on the question of similar trajectories above and below the tropopause. If the trajectories are similar, how does the change from damp to dry air across the tropopause arise. The frost point falls with increasing height in the troposphere mainly because the air gets colder; that cause does not operate in the stratosphere. The plausible and probable explanation of the low values of the frost point in the stratosphere is that the air has come from lower latitudes (or in the polar night from polar regions) where correspondingly low temperatures have occurred. Some of this very dry (absolutely) air could naturally get mixed with the layer around the tropopause, and so account for the frost point in these layers being below the air temperature. Without such interchange the air at the tropopause would necessarily be saturated. The alternative suggestion of dissociation of the water vapour seems less likely. Mr. Hawson, in his reply, said that he believed the atmosphere to be generally baroclinic; by the action of pre-existing pressure systems and geography the baroclinity is given character and, in particular, sloping zones of relatively strong baroclinity are produced. These give rise to strong upper wind fields of considerable character, which produce major convergence and divergence and associated vertical motions within the atmosphere, largely in accordance with the theories of Sutcliffe and his collaborators. In their turn these modify the pressure systems and baroclinity to renew the cycle, the vertical motions reacting with the atmosphere to determine the weather. If divergence is concentrated at the tropopause level, vertical motion takes place upwards and downwards to this level. The air above the level is warmed and becomes dry (i.e. the relative humidity becomes low). The air below the level is cooled, becomes damp and approaches the saturated adiabatic lapse rate. If the divergence level does not coincide with the tropopause level a similar effect occurs and in addition the character and height of the tropopause is modified. The tropopause is dragged towards the divergence level and in time a new tropopause is formed at this level. Mr. Hawson said he did not claim this to be an authoritative account of what happens but simply a statement of what he personally believed. It was offered as a possible explanation to meet Mr. Gold's point.

Dr. Sutcliffe referred to the Canadian adoption of a system of synoptic analysis with three distinct frontal surfaces more or less continuous. He said one's reactions depended on one's point of view. It was possible to define a variety of models and then attempt to explain the behaviour of the atmosphere in terms of any chosen model. As time went on and unexplained features were discovered various adjustments had to be made to the original ideas or the model rejected and a new one developed. On the whole he thought that models too often attempted to "straight-jacket" the atmosphere. He recognized the necessity for objective definitions of the tropopause for some statistical work which must deal with the raw data. These definitions would not always satisfy the synoptic analyst's tests of continuity in space and time.

Dr. Farquharson said that senior forecasters in the Forecasting Division had envisaged with horror the possibility of certain suggestions as to fronts in the upper air being adopted by the World Meteorological Organization. Problems in analysis abounded, but these were best dealt with by giving considerable freedom to the scientists who were the senior forecasters at Dunstable. He would like to make it clear that denigration of frontal analysis in the high atmosphere was in no sense a depreciation of the Bjerknes school of thought whose ideas, in his view, constituted the greatest contribution to practical forecasting of the present century.

Dr. Frith said that the observations made by the Meteorological Research Flight showed that it was the relative humidity which fell sharply across the tropopause. As a rule absolute humidity simply went on falling more or less steadily with increasing altitude. These observations favoured the idea that the stratosphere and the troposphere either side of the tropopause are one air mass. Mr. Hawson said he was glad to echo the pleas which had been made for continuity in analysis but must sound a note of warning. Air masses are not static things, but living dynamic entities constantly changing character by the processes of subsidence and convection, as well as advection and radiation. He recalled the case of a front which passed through Dunstable as a well marked cold front, yet, having traversed the North Sea, approached southern Scandinavia as a warm front. The air to the west of the front apparently first subsided rapidly to become the warmer air, and then picked up moisture from the North Sea.

Mr. Davies suggested that Berggren might find similar trajectories immediately above and below his tropopauses because he placed them within one air mass, according to the British analysis. Mr. Hawson replied he did not think this likely. Although he had pointed out examples where Berggren's tropopause analysis differed from the routine British analysis there were plenty of cases where the two were in agreement.

Mr. Harley said that the fact of briefing those who fly through the atmosphere compels the forecaster, while using simplified ideas such as fronts, to keep close to the realities of atmospheric behaviour. He has to try to maintain a scientific attitude, remembering how much these working ideas are but hypotheses, and to struggle to form a clear conception of atmospheric circulations in three dimensions, which is essential to real forecasting. Mr. Harley supported Mr. Hawson in his declaration of his beliefs in this respect.

Mr. Lumb said that at Preston they had been forecasting the height of the tropopause for some time, in connexion with their high-level wind forecasts. They found the "waterspout" model described by Mr. Matthewman of value.

The Director, in closing the meeting, said that he had been most interested by the discussion. He was quite used, in mathematics, to problems which had been dealt with in different ways, but the answer always was the same. This did not seem to hold for upper air analysis, which had a large subjective element. However, even if this discussion had shown the weakness or failure of a model, we should not worry unduly. Physics progresses by proposing hypotheses, finding where they fail and constructing new hypotheses. As Jeffreys said: "It is the exception which improves the rule".

LETTER TO THE EDITOR

Rate of rise of pilot balloons

In your issue of October 1953 Mr. F. H. Ludlam discusses some experiments he has carried out in the Albert Hall on the rate of ascent of pilot balloons. Amongst others, four 20-gm. balloons were inflated to give free lifts varying between 62 and 91 gm. and on release were all found to ascend at the same rate, within the observational error, of 150 m./min. This result appears to me remarkable, and from it Mr. Ludlam concludes that small variations of lift such as may be produced by intermittent sunshine or a slow leak will have a negligible effect on the rate of ascent of balloons. He adds that "this effect was discovered by J. S. Dines in his original work on the rate of ascent of pilot balloons". The reference is to a paper of mine in the *Quarterly Journal of the Royal Meteorological Society* of 1913, but I do not think that this can fairly be deduced from my paper. On the contrary, I found that the change in the rate of ascent of a balloon inflated to increasing diameters agreed closely with that expected from the assumptions that the resistance to rising varies as the square of the velocity and directly as the cross-sectional area. From these assumptions it is easy to calculate that any increase in free lift due to expansion of the gas by heating or decrease due to a slow leak will lead to a corresponding increase or decrease in the rate of ascent.

The behaviour of balloons is known to be erratic, and the rate of rising through the height available in the Albert Hall may well differ between one ascent and another so that a considerable series of measurements is desirable to give a reliable mean. Is it possible that Mr. Ludlam trusted to results obtained from too few ascents and that a longer series would have shown that the balloons with the larger lift did in fact rise with the greater velocity? The only other explanation which I can think of is that with these particular balloons (20 gm.) rising at this particular rate (150 m./min.) some factor comes in, such as a change in the instability of the wake, which over a limited range prevents an increase of free lift leading to an increase in the rate of ascent. My own experiments did not suggest any discontinuity of this kind, but it should be pointed out that I did not use balloons of this particular size.

J. S. DINES

The Yews, Hermitage, Newbury, Berks., November 13, 1953

[I agree with Mr. Dines that the results of our tests are remarkable, and would say also that they are consistent and reliable even though few in number. It appeared to us that the shape of the balloons when inflated was important. Increased inflation can distort a balloon considerably from a spherical shape; it then ascends with its major axis roughly horizontal and the characteristics of the wake may change. It is certainly implied that formulae for the rate of ascent can be applied only to balloons of particular construction, inflated into a particular shape.—F. H. LUDLAM]

NOTES AND NEWS

Napier Shaw Centenary

The centenary of the birth of Sir William Napier Shaw falls on March 4, 1954.

One of Shaw's actions as Director of the Meteorological Office was the establishment in 1905 of the Monday discussions. One may imagine with what great interest he would have attended the discusssion of February 15, 1954, on the work on the numerical prediction of the pressure distribution which is being carried out in the laboratory at the Central Forecasting Office which bears his name.

The discussion of January 18, 1954 on the services provided by the Office to agriculture would have been equally interesting to him, for Shaw did much to encourage agriculturists to use meteorological knowledge and meteorologists to make the observations agriculturists need and arrange them in the most suitable way.

Observations aboard "selected" ships

The series of photographs in the centre of this magazine, beginning with those facing p. 80 shows how the observations are made aboard ship. The photographs were taken on board M.V. *Ruahine* with the assistance of Mr. J. Cosker, Third Officer, and Mr. R. Baker, Third Radio Officer, and by permission of Capt. Youngs, the Marine Superintendent of the New Zealand Shipping Co. Ltd.

Frequency distribution of wind speeds

Summaries of meteorological observations are commonly presented in the form of frequency distributions which may, in some cases, be represented concisely by frequency curves or their Cartesian equations. Brooks and Carruthers[1] fit a Pearson curve of Type I to observations of Beaufort wind force at Boscombe Down, 1932–38, and they quote Sherlock as having successfully fitted a Pearson Type III curve to frequencies of wind speed in America[2]. According to Jacobs, however, the frequency distributions of wind forces in the British Isles are expressible by a law of exponential form[3]. If n_v is the percentage of observations which exceed the wind speed v the relation found by Jacobs is

$$n_v = e^{a-bv^2}$$

or

$$\log_e n_v = a - bv^2 .$$

At any particular place b is constant for all directions of wind and is equal to the slope of the straight line obtained by plotting the natural logarithm of the mean values of n_v (i.e. for all directions) as ordinate against the quantity v^2 as abscissa. The relation expressed by the equation given above holds good for the frequencies of wind force in each cardinal direction owing to the constancy of b, and hence it is clear, when putting v equal to zero, that e^a is the total percentage of winds of all speeds from any given direction. The quantity a will therefore vary according to the direction of wind which is under consideration. In his memorandum, however, Jacobs does not give the experimental evidence on which his assertion of the constancy of the coefficient b for all directions is based.

The equation given by Jacobs has been found to be of great value in estimating the usability of airfields as limited by the force of the wind, especially at places where wind statistics are only available in the form of separate summaries of wind force and of wind direction. Crossley has shown (in an unpublished manuscript) that this equation corresponds to a frequency curve of the form

$$P(v) = 2b\, v\, e^{-bv^2} ,$$

and he deduces from it that the mean value of the wind speed v is equal to $\sqrt{(\pi/b)}/2$. The average wind speed is thus constant for all directions. We may make the further deduction that the variance of the wind speed is equal to $(1 - \pi/4)/b$ or approximately $1/5b$.

It would appear, however, that Jacobs's law is not of universal validity. In studying the wind observations at Kabete Observatory, Nairobi, for the 10 yr. 1934–43, the writer found that the wind forces observed at 1500 local time conformed to the linear relationship between $\log_e n_v$ and v^2 required by the law, but that those observed at 0900 did not. The proportion of light winds observed at Kabete at 0900 was much higher than at 1500, and there were also about

TABLE I—WIND STATISTICS AT KABETE OBSERVATORY, 1934–43

		0900		1500	
v	v^2	n_v	$\log_{10} n_v$	n_v	$\log_{10} n_v$
m.p.h.		%		%	
0·5	0·25	88·3	1·94547	100	2·00000
3·5	12·25	64·3	1·80821	97·3	1·98811
7·5	56·25	25·0	1·39794	78·2	1·89321
12·5	156·25	3·4	0·53403	34·3	1·53466
18·5	342·25	0·4	$\bar{1}$·61909	7·7	0·88930
24·5	600·25	...	...	0·6	$\bar{1}$·76567

Calms: 0900, 11·7 per cent.; 1500, zero.

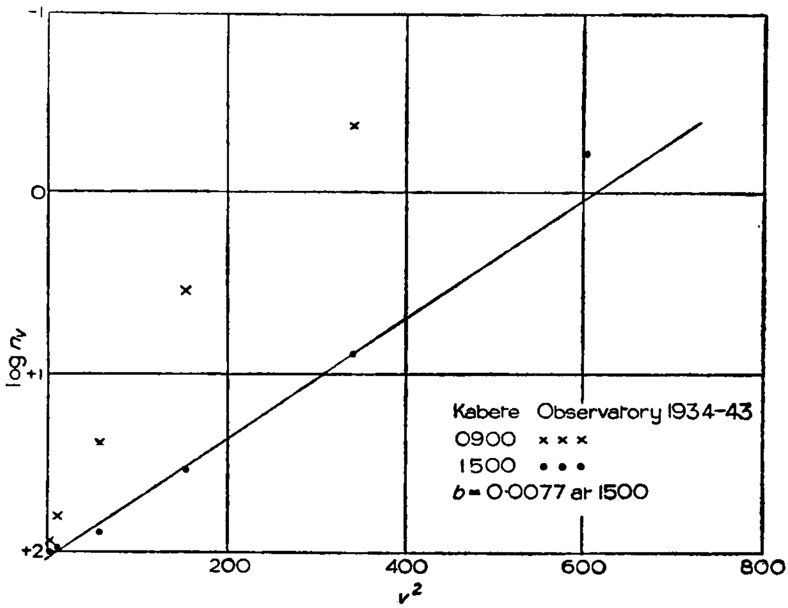

FIG. 1—WIND STATISTICS FOR KABETE OBSERVATORY, 1934–43

12 per cent. of calms at 0900, whereas no calms were recorded at all at 1500. The observations at Kabete therefore suggest that the law formulated by Jacobs is only valid for wind forces observed in conditions of turbulent motion and that laminar motion in the surface layers of the atmosphere apparently vitiates it.

J. WADSWORTH

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2. BROOKES, C. E. P. and CARRUTHERS, N.; Handbook of statistical methods in meteorology. London, 1953.
3. JACOBS, L.; The planning of runway layouts from the point of view of weather. Unpublished; copy in Meteorological Office Library, London, 1944.

METEOROLOGICAL OFFICE NEWS

Retirements.—*Mr. R. A. Watson*, Principal Scientific Officer, retired on January 10, 1954. He joined the Office in 1919; during his 35 years' service he worked at several aviation outstations, at Eskdalemuir Observatory, at Edinburgh and at Headquarters where he was Head of the Branch concerned with meteorological training in the Royal Air Force and with the meteorological requirements of the War Office and Ministry of Supply. At the time of his retirement he was Superintendent of the Meteorological Office, Edinburgh. Mr. Watson was seconded as Director of the Royal Alfred Observatory, Mauritius during 1927–30. Mr. Watson has accepted a temporary appointment in the Meteorological Office.

Dr. A. W. Lee, Senior Scientific Officer, retired on January 31, 1954, because of ill-health. He joined the Office in 1923 and served at Lerwick, Kew and Eskdalemuir Observatories until 1939. During the Second World War he served at Headquarters, Bomber Command and as Senior Meteorological Officer, No. 12 Group of the Royal Air Force. From 1946 to 1948 he served at Headquarters. During the past five years Dr. Lee worked at Kew Observatory.

Mr. W. A. Toms, Senior Experimental Officer, retired on December 31, 1953, on medical grounds. He joined the Office in 1918 when he was attached to the Observatory at Falmouth. The whole of his career in the Meteorological Office was spent at outstations and included two tours of duty overseas.

Academic successes.—Information has reached us that the following members of the staff have been successful in recent examinations; we offer them our congratulations.

Intermediate B.Sc.: pure and applied mathematics, geography, J. N. Brand, M. F. Lee.

General Certificate of Education (Advanced Level): pure and applied mathematics, physics, J. A. Gregson; pure mathematics, physics, D. C. Davis, W. H. Mills; pure mathematics, A. Lambley; physics, J. H. F. Childs.

University Bursaries for Assistant Experimental Officers.—We congratulate Mr. P. Goldsmith on his selection for an award of a bursary, to be taken up in 1954, for full-time study at a university.

Ocean weather ships.—o.w.s. *Weather Watcher* and *Weather Explorer* spent Christmas at sea. An aircraft of R.A.F. Coastal Command from Kinloss, on a navigation exercise, dropped mail and parcels, including fresh milk! The aircraft was able to drop the containers close to the ships, which was perhaps fortunate, as owing to heavy swell it was not possible to lower any boats. Seasonal greetings were exchanged between the aircraft and the ships. On Christmas Day, after the evening meal, impromptu concerts were held in which all hands joined.

Sports activities.—The Ariel Club Wintle Cup for the annual Billiards Handicap has been won this year by Mr. R. A. Ogden of M.O.8. This trophy has not been won by a member of the Meteorological Office staff for nearly 30 years when it was held by Mr. W. G. Davies, who is now at Watnall. Mr. Ogden also reached the semi-finals of both the Snooker Handicap and the Snooker Doubles Handicap.

WEATHER OF JANUARY 1954

Mean pressure was above normal over the eastern United States, the North Atlantic and north-west Europe but below normal over central Europe and the Mediterranean. The greatest excess of mean pressure above normal occurred in the region just west of Ireland and reached 10 mb. in places. The highest value of mean pressure, 1026 mb., was in the region between the Azores and Portugal while the lowest mean pressure, 999 mb., was recorded by the ocean weather ship between Greenland and Iceland. Mean pressure over the Mediterranean varied between 2 and 6 mb. below normal, the lowest mean pressure, 1010 mb., being in southern Italy.

Mean temperature was below normal over most of Europe, including the Mediterranean region; in south-east Europe it was as much as 12°F. below normal, but elsewhere it was between 2° and 5°F. below normal.

In the British Isles the weather was changeable; for the first ten days the general type of weather was northerly; subsequently a mild, unsettled westerly type prevailed for the most part until the 20th. South-easterly winds set in on the 22nd and from the 24th onwards it was very cold; in the last few days the wind was north-easterly.

On the 1st a large anticyclone was centred over south-west Ireland; this moved westward while a ridge to the north of it moving east resulted in rather cold N.–NE. winds over most of Great Britain. Another outbreak of northerly winds on the 6th caused an occlusion to break through the ridge, and wet snow fell over much of England and Wales and wintry showers in Scotland and Northern Ireland. Frost followed and frozen snow and ice affected many areas for 48 hours; on the 8th, Topcliffe in Yorkshire had a screen minimum temperature of 9°F. and Houghall, County Durham, one of 8°F. Later on the 8th milder air came round the anticyclone centred off south-west Ireland, with a general thaw, and a rather mild north-west current lasted until the 11th, with scattered slight rain or showers. An unsettled, mostly mild, south-westerly type of weather, with rain at times set in on the 12th culminating in the widespread gale of the 15th, which was most severe in the north, with gusts of 84 kt. and 79 kt. at Renfrew and Flamborough Head, respectively. During the gale, trees were blown down, buildings suffered considerable damage and many people were injured and a few killed. The westerly wind, of long fetch over the Atlantic, was unusually mild and temperature rose to 55°F. or above at most places in England and Wales on the 15th; 57·7°F. at Kew Observatory was the highest in January since records started there in 1871. On the 17th to 18th a ridge of high pressure moved east across the country giving widespread frost, but during the 18th mild south-westerly winds returned and heavy rain fell in the north-west (3·35 in. at Ardgour, Argyllshire). Between the 20th and 22nd there was a major change of type. On the 20th temperature reached 55–57°F. in many places in the south but there was an anticyclone to the north-east of Iceland and colder air was encroaching over the north-east districts of the British Isles. During its advance there was heavy rainfall and some flooding in south Scotland and north England (3·40 in. at Stonyhurst, Lancashire, 3·11 in. at Bolton Waterworks, Lancashire and 2·64 in. at Stocks Reservoir, Yorkshire on the 20th). By the 22nd the anticyclone had moved to Scandinavia, and on the 23rd its central pressure reached 1048 mb. and it was the dominant feature for the remainder of the month. Frost occurred over much of England on the 24th and from this time onwards the weather was severe. A trough over Ireland and west Scotland gave heavy rain locally in these areas on the 24th (2·50 in. at Gruline, Isle of Mull). The trough developed into a depression over Ireland on the 25th, which, moving south-east, gave considerable snow locally over most western districts of Great Britain and also in southern England excluding Kent and the London area (snow lay to a depth of 1 ft. at Bwlchgwyn, Denbighshire on the 26th). Snow fell during the last few days in most areas, mainly in small amounts. Temperature remained continuously at 32°F. or below at some low-level stations from the 25th to the 28th inclusive, for example at Watnall in Nottinghamshire, while at Bwlchgwyn (1,267 ft. above M.S.L.) air frost began at 1300 on the 24th and continued for the rest of the month. On the 28th South Farnborough registered a screen minimum temperature of 10°F. and at Boscombe Down the maximum on the same day was only 23°F.

The general character of the weather is shown by the following provisional figures :—

	AIR TEMPERATURE			RAINFALL		SUNSHINE
	Highest	Lowest	Difference from average daily mean	Percentage of average	No. of days difference from average	Percentage of average
	°F.	°F.	°F.	%		%
England and Wales ...	59	8	−1·9	88	−3	117
Scotland	56	9	−0·1	101	−1	112
Northern Ireland ...	55	17	−0·3	93	+1	78

RAINFALL OF JANUARY 1954

Great Britain and Northern Ireland

County	Station	In.	Per cent. of Av.	County	Station	In.	Per cent. of Av.
London	Camden Square ...	1·04	56	*Glam.*	Cardiff, Penylan ...	2·66	72
Kent	Dover	2·33	109	*Pemb.*	Tenby	..	..
,,	Edenbridge, Falconhurst	1·69	67	*Radnor*	Tyrmynydd	5·83	93
Sussex	Compton, Compton Ho.	2·20	69	*Mont.*	Lake Vyrnwy	5·90	102
,,	Worthing, Beach Ho. Pk.	1·26	54	*Mer.*	Blaenau Festiniog ...	10·38	102
Hants.	Ventnor Park	1·86	71	,,	Aberdovey	4·07	105
,,	Southampton (East Pk.)	1·45	54	*Carn.*	Llandudno	2·17	90
,,	South Farnborough ...	1·43	68	*Angl.*	Llanerchymedd ...	2·85	90
Herts.	Royston, Therfield Rec.	0·81	47	*I. Man*	Douglas, Borough Cem.	4·25	127
Bucks.	Slough, Upton	0·89	47	*Wigtown*	Newton Stewart ...	4·59	111
Oxford	Oxford, Radcliffe ...	1·29	71	*Dumf.*	Dumfries, Crichton R.I.	4·01	125
N'hants.	Wellingboro' Swanspool	0·93	50	,,	Eskdalemuir Obsy. ...	5·55	103
Essex	Shoeburyness	1·33	99	*Roxb.*	Crailing...	2·36	122
,,	Dovercourt	1·67	104	*Peebles*	Stobo Castle	4·45	148
Suffolk	Lowestoft Sec. School ...	1·55	93	*Berwick*	Marchmont House ...	2·53	112
,,	Bury St. Ed., Westley H.	1·52	85	*E. Loth.*	North Berwick Res. ...	1·85	108
Norfolk	Sandringham Ho. Gdns.	1·93	99	*Midl'n.*	Edinburgh, Blackf'd. H.	2·24	127
Wilts.	Aldbourne	2·08	90	*Lanark*	Hamilton W. W., T'nhill	4·91	149
Dorset	Creech Grange... ...	1·77	54	*Ayr*	Colmonell, Knockdolian	3·77	87
,,	Beaminster, East St. ...	2·34	67	,,	Glen Afton, Ayr San. ...	6·30	124
Devon	Teignmouth, Den Gdns.	1·63	56	*Renfrew.*	Greenock, Prospect Hill	7·59	117
,,	Ilfracombe	2·39	73	*Bute*	Rothesay, Ardencraig ...	5·49	122
,,	Princetown	4·11	52	*Argyll*	Morven (Drimnin) ...	7·36	116
Cornwall	Bude, School House ...	2·24	74	,,	Poltalloch	..	..
,,	Penzance, Morrab Gdns.	3·65	96	,,	Inveraray Castle ...	9·09	111
,,	St. Austell	3·09	72	,,	Islay, Eallabus	6·37	136
,,	Scilly, Tresco Abbey ...	2·68	85	,,	Tiree	6·24	147
Somerset	Taunton	1·81	76	*Kinross*	Loch Leven Sluice ...	3·57	113
Glos.	Cirencester	1·69	67	*Fife*	Leuchars Airfield ...	1·36	75
Salop	Church Stretton ...	2·71	104	*Perth*	Loch Dhu	9·23	101
,,	Shrewsbury, Monkmore	2·33	119	,,	Crieff, Strathearn Hyd.	3·39	84
Worcs.	Malvern, Free Library...	1·65	75	,,	Pitlochry, Fincastle ...	3·71	106
Warwick	Birmingham, Edgbaston	1·08	53	*Angus*	Montrose, Sunnyside ...	1·10	55
Leics.	Thornton Reservoir ...	1·23	62	*Aberd.*	Braemar	2·29	72
Lincs.	Boston, Skirbeck ...	1·26	78	,,	Dyce, Craibstone ...	1·11	47
,,	Skegness, Marine Gdns.	1·49	86	,,	New Deer School House	1·72	74
Notts.	Mansfield, Carr Bank ...	1·75	81	*Moray*	Gordon Castle	1·14	56
Derby	Buxton, Terrace Slopes	4·64	104	*Nairn*	Nairn, Achareidh ...	1·35	75
Ches.	Bidston Observatory ...	2·48	117	*Inverness*	Loch Ness, Garthbeg ...	3·57	81
,,	Manchester, Ringway...	2·70	113	,,	Glenquoich	..	..
Lancs.	Stonyhurst College ...	5·63	132	,,	Fort William, Teviot ...	7·99	82
,,	Squires Gate	2·89	111	,,	Skye, Broadford ...	7·61	101
Yorks.	Wakefield, Clarence Pk.	1·94	101	,,	Skye, Duntuilm ...	4·87	92
,,	Hull, Pearson Park ...	1·68	93	*R. & C.*	Tain, Mayfield... ...	2·00	82
,,	Felixkirk, Mt. St. John...	2·69	135	,,	Inverbroom, Glackour...	5·82	108
,,	York Museum	1·36	77	,,	Achnashellach	7·38	81
,,	Scarborough	1·80	90	*Suth.*	Lochinver, Bank Ho. ...	3·45	81
,,	Middlesbrough... ...	2·41	151	*Caith.*	Wick Airfield	1·86	76
,,	Baldersdale, Hury Res.	3·71	111	*Shetland*	Lerwick Observatory ...	3·74	88
Norl'd.	Newcastle, Leazes Pk....	2·87	145	*Ferm.*	Crom Castle	2·31	69
,,	Bellingham, High Green	3·13	109	*Armagh*	Armagh Observatory ...	1·87	74
,,	Lilburn Tower Gdns. ...	2·92	141	*Down*	Seaforde	2·69	85
Cumb.	Geltsdale	3·04	109	*Antrim*	Aldergrove Airfield ...	2·74	100
,,	Keswick, High Hill ...	4·58	91	,,	Ballymena, Harryville...	3·82	103
,,	Ravenglass, The Grove	2·99	89	*L'derry*	Garvagh, Moneydig ...	3·74	109
Mon.	A'gavenny, Plâs Derwen	3·80	102	,,	Londonderry, Creggan	4·09	114
Glam.	Ystalyfera, Wern House	3·70	59	*Tyrone*	Omagh, Edenfel ...	3·24	92

Printed in Great Britain under the authority of Her Majesty's Stationery Office
By Geo. Gibbons Ltd., Leicester

Wt.34. K16