BANNON, J. K., 1952. Weather Systems associated with some occasions of Severe Turbulence at High Altitude. *Met. Mag.*, **81**, pp. 97—102.

BERGGREN, R., 1952. The Distribution of Temperature and Wind connected with active tropical air in the High Troposphere and some remarks concerning Clear Air Turbulence at High Altitude. *Tellus*, **4**, pp. 43—53.

HARRISSON, 1950. Some characteristics of the Upper Level Low and Jet Streams. *United Air Lines Circular*, No. 34.

HISLOP, G. S., 1951. Clear Air Turbulence over Europe. *Journal of the Roy. Aero. Soc.*, **55**, pp. 185—225.

MURRAY, R. and JOHNSON, D. H., 1952: Structure of the upper Westerlies; a Study of the Wind Field in the Eastern Atlantic and Western Europe in September 1950. *Q.J. Roy. Met. Soc.*, **78**, pp. 186—199.

Reply

Dr Bannon emphasizes that it is necessary to study the fundamental features of each occasion with clear air turbulence, viz. vertical and horizontal shear and lapse rate of temperature. I agree completely with him in this respect, and that is the reason why I have mentioned this special phenomenon in my paper. As is pointed out there the distance between the stations of the present radiosonde networks is often not of the same order as that of the phenomenon one is investigating. For that reason it is—for the time being—necessary to use some sort of a model. Here the concept of front enters the picture. If we study the frontal model in detail when the available material is most favourable and make as many generalizations as are justified by the data, then we may use the frontal model to advantage in such cases where the material is sparse. A specific example may clarify what I mean.

It is generally accepted—as can also be seen in Bannon's letter—that horizontal wind shear is of great importance in many cases with clear air turbulence. In his paper BANNON (1951 a) tries to correlate the appearance of these two items and is taking the horizontal shear given by the wind-observing stations in Great Britain. I dare say that my paper (as well as many others)—and a second one which will soon appear as "band I, nr 24", of "Arkiv för Geofysik"—has made it clear that the wind distribution is anything but linear over such great distances as those ordinarily existing between radiosonde stations. We have no possibility whatsoever to get the true value of the shear by linear interpolation between stations, but—and this is one of the main points in the second part of my paper—the frontal model gives a possibility to get better values, even if not the right ones. Furthermore it is easier—for instance in a vertical cross-section—to extrapolate or interpolate the wind field with the help of a model then to do it without one. It is certainly rather dangerous to do so, but if one is well aware of this fact, the danger can be diminished.

Bannon mentions a paper by MURRAY and JOHNSON (1952) stating that frontal analysis in the upper troposphere is difficult. I certainly agree with that statement, but this gives us no reason not to use the concept of front also in this analysis. The fact that the frontal analysis may be carried through also in the upper troposphere is well established by many writers, among others by PALMÉN (1951) and GODSON (1951).

It is right that table I in Hislop's paper gives only 4 cases with clear air turbulence connected with fronts. But the table gives only 6 cases where it is definitely stated that no front was associated. For the remaining 10 cases the connection is stated as 'doubtful'. Considering the difficulty of analysing fronts in the upper troposphere one is justified to assume that a number of front-cases are hidden in this group, as Bannon also does.

It do not quite understand Bannon's statement in paragraph four. First he says that clear air turbulence in the upper troposphere is *sometimes* associated with frontal zones, and then he states that he has recognized *many* such cases as occurring in frontal zones. I certainly agree with his last statement.

As to the connection with fronts I would like to review a remark made by Farquharson in a discussion meeting held in London February 1952, on "Meteorology and the operation of jet aircraft" (see *Quart. Journ.* vol. **78** no. 337 p. 451). He states that in the forecasting of clear air turbulence the Meteorological Office at Dunstable succeeded in 22 cases out of 24 by using the observed connection between clear air turbulence and the jet stream. Is it not so, however, that talking about the displacement and the behaviour of the jet stream is equivalent in many (most?) cases with the use of the frontal model?

It was not the purpose of my paper to touch directly the question of the *causes* of clear air turbulence. I wanted primarily to discuss the treatment of the involved meteorological parameters.

As far as I can see, the difference of opinion concering the height of the tropopause in my fig. 6 is not an important question. It is quite right that the layer immediately above the tropopause in my case shows a vertical temperature gradient

of $-5.8°$ C km^{-1}, but the gradient in the layer below the tropopause is $-7.3°$ C km^{-1}. It is thus a marked difference between the layers. It must not be forgotten that the definition of the tropopause used by the Meteorological Office in the daily service is to some extent arbitrary and formal and may sometimes lead to wrong conclusions.

Finally I want to thank Dr Bannon for the interest he has shown in my paper and I am glad that a discussion has arisen out of it.

ROY BERGGREN

REFERENCES

PALMÉN, E., 1951: The Aerorology of Extratropical Cyclones. Compendium of Meteorology, *Am. Met. Soc.*, Boston, p. 599—620.

GODSON. W. L., 1951: Synoptic Properties of Frontal Surfaces, *Q. J. Roy. Met. Soc.*, **77** pp. 633—653.

Dear Sir,

The article "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" by Roy Berggren in the February 1952 issue of Tellus, has recently been drawn to my attention.

I do not wish to comment here on Mr Berggren's claim that clear air turbulence is essentially associated with the presence of a frontal system in the upper air, as I understand that this will be covered in a separate communication from Mr Bannon.

However, I would like to point out that in my view figure 10 in my paper to the Royal Aeronautical Society (mentioned on page 52 of Mr Berggren's paper) does not give the impression that the turbulent area is associated with anticyclonic wind shear. In fact, my interpretation of the figure is that, if anything, the shear is cyclonic. In any case, no mention of the character of the wind shear is mentioned in the text. The figure shown is merely illustrative of typical conditions and rather than show a chart which we might have prepared specially, I preferred to use the chart as published in the Daily Weather Report.

Yours faithfully,
G. S. HISLOP.
Ruislip, Middlesex
11 September 1952

Reply

Dr Hislop is opposing to my comments on one of his figures of the paper cited by me. I do not quite understand his objections and for this reason I shall give a short discussion of the sign of the horizontal wind shear on his figure.

The radiosonde station Liverpool (03322) gives the wind 200° 129 knots at 300 mb at a height of 29,200 feet. Station Larkhill (03743) gives 200° 115 knots and the height 29,700 feet. Isotach analyses by many authors (e.g. NEWTON, PHILLIPS, CARSON, and BRADBURY, 1951) give as a rule the jet centre quasi-parallel to the isohypses, in cases without strong accelerations. Thus, in this case, the centre of the strong winds must pass to the west of the indicated turbulent area. In any case, I consider it impossible to get cyclonic shear through this area.

The most serious objection, however, to the discussed figure is the time difference between the chart and the observation of clear air turbulance. As I pointed out in my paper the chart time is 0300 GCT, while the observation was made at 1300 GCT. Studying the change of the wind from 0300 to 1500 GCT one gets the following interesting values. At 350 mb the Liverpool wind changed from 200° 122 knots to 230° 78 knots, while the corresponding values for Larkhill were 200° 111 knots and 200° 102 knots, respectively. The Downham Market (03476) sounding gave at 1500 GCT 210° 109 knots (unfortunately the 0300 GCT sounding did not reach the 350 mb level).

This distribution of the wind makes it quite clear that the horizontal (isobaric) wind shear was cyclonic at the time of observation over the area where clear air turbulence was observed.

I cannot quite understand the reason to publish a 0300 GCT chart to illustrate the conditions at 1300 GCT, when there are soundings made at 1500 GCT. I know that the 300 mb charts for that time were not published in the Upper Air Section, but that is not a sustainable explanation for the choice.

It is quite true that nothing specific is said in Hislop's paper about the sign of the shear, but this question has been brought up in later papers. For that reason I considered it important to make these remarks, as Hislop's paper will be classic in this field and is and will be read by many persons.

ROY BERGGREN.

REFERENCE

NEWTON, C. W., PHILLIPS, N. A., CARSON, J. E. and BRADBURY, D. L., 1951: Structure of Shear Lines near the Tropopause in Summer. *Tellus*, **3**, p. 154—171.