

Letters to the Editor

Jet Streams and Clear Air Turbulence

Dear Sir,

Dr BERGGREN (1952) has given interesting frontal analyses of three occasions of jet stream over western Europe and has put forward the hypothesis that most occurrences of clear air turbulence in the upper troposphere and lower stratosphere are associated with fronts.

Frontal analyses are necessarily subjective and influenced by the personal views of the analyst and it is, therefore, not surprising that I do not agree with some aspects of Berggren's analyses. For example, the level of the tropopause over Downham Market in Berggren's figure 6 does not agree with that published in the Upper Air Section of the British Daily Weather Report, where it is given as 260 mb; the fall of temperature between 319 and 260 mb, i.e. above Berggren's tropopause, was 7.9° C. giving a temperature gradient of -5.8°C. per kilometre, which would seem tropospheric rather than stratospheric. There are other minor points of disagreement.

Frontal analysis in the upper troposphere is difficult, as has been pointed out by MURRAY and JOHNSON (1952). Thus, though the idea of tracing the frontal zone into the stratosphere is attractive, yet because the evidence for such zones rests on one, or at most two, radiosonde ascents in each case and because of the subjective element in the analysis illustrated above, I prefer to await the promised publication of the complete analysis before accepting the stratospheric fronts as significant.

There is no doubt that clear air turbulence in the upper troposphere is sometimes associated with frontal zones. HISLOP (1951) in his table I lists four cases which were definitely so associated and there may well have been others among the number listed as "doubtful". In analysing other occurrences of turbulence I have recognized many as occurring in frontal zones (BANNON, 1952). Undoubtedly, however, turbulence may occur outside such frontal zones. A good example is the Flight VR60 of Hislop's table I where part of the turbulent layer was in a frontal zone and part above it in the strong vertical shear under a jet stream core.

Berggren's extension of the front through the

tropopause entails a definition of front in that region by strong horizontal shear. His hypothesis, that most cases of clear air turbulence are associated with a frontal zone, therefore implies with his definitions that the clear air turbulence is associated with either vertical or horizontal shear or both. This does not seem to me to be any advance on previous ideas (HARRISON, 1950; BANNON, 1951, a, b, c). "Front" is merely a name given to a boundary zone; it does not imply any increased tendency towards turbulence except as such a zone is associated with the usual turbulence-producing agencies, shear and convection.

For a proper investigation of the causes of clear air turbulence it is not sufficient, except perhaps as a demonstration of a possible forecasting tool, merely to associate the phenomenon with a loosely-defined attribute of the meteorological variables called a "front". Even if the great majority of occasions of turbulence were found to be associated with fronts, which is not the case, it would still be necessary to study the fundamental features of each occasion, viz. vertical and horizontal shear and lapse rate of temperature. As HISLOP (1951, Section 6.4) has pointed out, it is the relative scarcity of observations of clear air turbulence and the lack of adequate measures of the meteorological parameters, shear and temperature lapse rate, which are restraining progress in this field.

I should like to draw attention to a misprint in the footnote on page 50 of Berggren's paper. The dimensions of eddies causing bumps to Spitfire and Mosquito aircraft are thought to be from 15—150 metres and not 15—500 metres, as stated.

Yours faithfully,

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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. Further-

more 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 concerning 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