motion of the dry air above the inversion and spreading of the moist air below it, which will probably intensify the already existing inversion. This has yet to be supported by actual observations when there is fair weather over northeast India. Further, it is considered that downward motion will be gentle and extensive rather than strong and localised as contemplated by Ramaswamy.

From what has been stated above, it will be seen that the claim made by Ramaswamy that one can forecast, more than 12 hours ahead, the broad regions of thundersqualls or Nor'westers and convective dust-storms on the basis of vorticity patterns in the upper troposphere at and above 500 mb level is not justified although the same will undoubtedly cause extension or intensification of these phenomena under certain circumstances.

Poona, June 5, 1956.

B. N. Desai
Meteorological Office

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Desai, B. N., 1950: Mechanism of Nor'westers of Bengal, *Indian J. Met. & Geophys.*, **1**, pp. 74—76.
Desai, B. N., and Mal, S., 1938, Thundersqualls of Bengal, *Gerl. Beitr. z. Geophysik*, **53**, p. 285.
Ramaswamy, C., 1956: On the sub-tropical jet stream and its role in the development of large scale convection, *Tellus*, **8**, pp. 26 – 60.

Reply

Dear Sir,

Most of the points raised by B. N. Desai in the above note have already been discussed by the present writer in his paper ((Ramaswamy, 1956). However, the following very brief remarks are offered for the purpose of clarification: —

(a) Desai's note would give the impression that the author does not attach any importance to the local factors such as insolation, nocturnal valley convergence, orographic influences etc. in the development of large-scale convection. This is not so: these factors do play a part in the development. The main thesis of the author, however, is that these local factors are either a constant feature (e.g. orographic effects) or are often present during fair weather as well as severe convective weather (e.g. insolation and nocturnal valley convergence) while, in contrast, upper level *divergence* is invariably present during occasions of large-scale convection and upper level *convergence* prevails during unusual spells of fair weather. In other words, upper level divergence seems to be an 'invariable factor' in the development of large-scale convection while insolation, valley convergence etc. appear to be "local perturbing factors" of the lower troposphere which determine, when coexistent with upper divergence, the times of occurrence of violent convection. These are the reasons which have led the author to conclude that large-scale convection is 'overwhelmingly determined' by the upper divergence field (see pages 26 and 54 of the original paper).

(b) Desai's note may give the impression that the upper air systems illustrated in figures 10—14 in the paper moved away eastwards during the subsequent 24 hour-period and that the convective developments shown in these figures actually took place when the vorticity systems referred to by the author were not present. Considerations of space do not permit the author to publish in this reply the charts for the 3 subsequent days referred to. All that the author would state here is that the eastward movement of the systems on these occasions was small and was, in any case, much slower than the movements of high-level systems usually observed in middle latitudes. It may be incidentally added that it is the slow movement of upper level patterns which seems to provide adequate time for the transmission of the associated upward motion to the lower parts of the troposphere.

(c) One of the important objectives in this investigation was to find out a significant scientific criterion for forecasting large-scale convection over Indo-Pakistan. As stated by Desai himself, there would have been no scope for achieving this objective if the synoptic hour for the high level flow patterns had been chosen as suggested by him. Regarding the other reasons which led the author to choose 09/15 GMT hours of the previous day for the high level patterns, the reader is referred to the original paper (pp. 30 & 31). It will also be quite relevant to mention in this connection that an extensive chart showing the sea level pressure changes in *24 hours* ending at 03 GMT is used on a routine basis by the Indian forecasters along with the 03 GMT sea level chart for prognosticating weather and pressure systems during a period of

36 hours commencing from 03 GMT. The *mean synoptic hour* of this pressure change chart is *15 GMT of the previous day*. In view of this, the choice of the upper air charts of 12 GMT (mean hour) of the previous day, by the author, for prognosticating broad zones of the thunderstorms and duststorms during a *24 hour*-period commencing from 03 GMT appears to be justified. The author would also take this opportunity to suggest that, as a further step in this investigation, it would be interesting to attempt a prognostic high-level contour chart for the next 24 hours on the basis of the 09/15 GMT (mean hour 12 GMT) chart and determine the zones of convective activity on the basis of this chart.

(d) In explaining the schematic diagrams in figures 29(a), (b) and (c), the author has discussed the role of the dynamical processes and the local factors separately, only for the sake of simplicity in discussion. These local factors and their contribution to the actual development of large-scale convection have been more fully discussed in the later para in the same section of the paper (p. 53) and again in Section 12 (p. 56) where the effects of these local factors in helping the upper divergence field to overcome the resistance to lifting of the moist air have been specifically mentioned.

(e) The author has not stated that the downward motion induced by the dynamical processes is localised. The downward motion must obviously be extensive as it has been attributed to the effect of large-scale patterns. The word 'strong' was used by the author to qualify the downward motion only in a relative sense. The speed of the downward motion will obviously depend upon the amount of upper convergence, which, in its turn, will depend (to a first approximation) upon the amount of negative vorticity advection i.e. upon the negative value of the term

$$-V^2\left(\frac{\partial k_s}{\partial x}+k_s k_n\right)$$

(p. 51 of the original paper). No quantitative assessment of the downward or upward motion has been made by the author.

In view of the above, the author is still of opinion that the high level vorticity patterns provide a useful tool for the prognostication of the *broad areas* of development of thunderstorms and duststorms in northern India and Pakistan in the premonsoon period, more than 12 hours ahead, whenever adequate Radio-wind and Radio-sonde data are available.

Madras 31, November 9, 1956.

C. RAMASWAMY.

Regional Meteorological Centre.

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RAMASWAY, C., 1956: On the sub-tropical Jet Stream and its role in the development of large scale convention, *Tellus*, **8**, pp. 26—60.

Oxygen in Antarctic Air

Dear Sir,

In reviewing the many determinations of atmospheric composition CARPENTER (1937) and PANETH (1937) concluded that the amount of oxygen in air is always the same at open places all over the world. The values generally range from 20.93 to 20.96 volume per cent. HOCK ET AL. (1952) have since published a series of 320 determinations made during an 18 months stay at Pt. Barrow, Alaska, at 71 N. All of these fell between 20.91 and 20.96 per cent and the variation was not considered significant. In sharp contrast to this essentially constant composition of arctic air are the determinations of LOCKHART and COURT (1942) on the oxygen in ground level air in the Antarctic. Their values ranged from 20.76 to 20.48 volume per cent, a surprising amount lower than those from any other place on the earth. In addition they noted a seasonal variation, the summer values being generally lower than the winter ones. GLUCKAUF and PANETH (1946) objected to these measurements because no check analyses on normal air were carried out.

The icebreaker Atka collected a series of air samples off the Antarctic continent in 1955. When these became available[1] it seemed worthwhile to determine the oxygen in them. Empty helium cylinders were filled to 2,000 pounds pressure by a compressor on board ship. Smaller cylinders were later filled from these.

[1] Mr. Paul Humphrey of the U.S. Weather Bureau was responsible for the collection of the gas samples. They were made available to the author through the kindness of Dr. F. T. Haremann of the Argonne National Laboratory.