

551.515.2 : 551.515.8 (54 : 267)

## A REPLY TO

"SOME ASPECTS OF TROPICAL METEOROLOGY WITH SPECIAL REFERENCE TO THE INDIAN WEATHER—AN EXAMINATION OF MALURKAR'S CONCEPTS REGARDING THEM"

In Vol. 2 No. 2 (pp. 113—120) of the Indian Journal of Meteorology and Geophysics, the above examination has been made of my points of view published in the book "Forecasting Weather in and near India (1945)" and the subsequent series of notes in Current Science in 1947 and 1948 on the basis of (i) the normal charts for the Indian area published by India Meteorological Department, (ii) the charts prepared by Inst. Cdr. Pandal and (iii) other published papers.

A reader going through the published papers<sup>1-17</sup> and the memoir<sup>18</sup> would readily find satisfactory answers for all the scientific doubts and questions raised by the critic.

It is suggested that concepts like the passage of low pressure waves or areas across the S. Peninsula of India causing the western disturbances to become more active, the low pressure cells in China in connection with the break in the S. W. monsoon, the orientation of high pressure areas in the S.W. Indian Ocean, the role of eastern air in Indian Weather which had to be ultimately traced to the N. Pacific high pressure area and the like to which I drew *explicit* attention were well known. It is no argument to say that they could be found by any one looking into synoptic or climatic charts. They can be so found only after pointed attention has been drawn to them. In addition the critic has missed the limitations of some of the concepts given by me.

In section 3(i)\* it is stated "It has been recognised by the majority of Indian Meteorologists since long that the southwest monsoon is caused by the transport of air across the equator from the southern hemisphere" quoting Simpson (1921). I have said in

para 2 of my paper "Recent advances in Tropical Meteorology" that "upto 1925, the idea of S.W. Monsoon was that it was due to the penetration of the *S.E. Trades* into the northern hemisphere. Blanford, Hemraj, Simpson, Doraiswamy Iyer and Normand took this view" (vide para 2†) and in para 3 of the same paper that "in his contribution to the Indian Science Congress in 1938, Normand kept on to his old view though explicit mention is lacking". Much as one might have wished otherwise, the fifteen years after 1925 brought in new doubts. While reference has been made to some earlier papers of Ramanathan the critic has omitted any reference to the work of Wagner,<sup>21</sup> of later work of Ramanathan and Ramakrishnan<sup>22,23</sup> and of Sur<sup>24</sup> where discussion of the circulation of the lower troposphere was made with the available upper air observations which "have increased abundantly enough to enable one to attempt with some prospect of success to understand the mechanics of the S.W. monsoon"<sup>21</sup> (translated).

The above papers by Ramanathan and Ramakrishnan<sup>22, 23</sup> were results of exhaustive work and have been referred to extensively. It has to be assumed that authors would have taken account of earlier work on a similar subject by senior author which alone has been quoted by Desai. The results deduced by the above authors, *viz.*, Wagner, Ramanathan and Ramakrishnan, and Sur and the obvious implications of their diagrams cannot be ignored as ones of belief. In the first memoir by Ramanathan and Ramakrishnan referred to above in Figs. 13 to 15 which show normal winds and stream lines for July at 1, 2 and 3 km, the direction of stream lines even at south peninsula latitudes is from the west with hardly any southerly component. In the second memoir on the general circulation of the atmosphere the same authors say that for the month of July "Upper air conditions have been fairly exhaustively discussed in a previous paper". No space was devoted to the circulation in the southern hemisphere either by these authors or by Sur. There was no other

\*The italicised numbers refer to sections in Desai's paper in Ind. J. Met. Geophys., Vol. 2, No. 2 pp. 113-120 (1951)

† The name of Elliot has been dropped out intentionally

serious discussion on the subject after them. Either a fresh method of approach or a use of fresh data was necessary to take the S.W. monsoon back to the southern hemisphere. As it happens in other branches of science, when switching back to an earlier concept some more definiteness has to creep in in the statements. The switch back to an earlier concept at this stage is not a pure restatement of earlier ideas.

The equator acts as a barrier against the free flow of winds across it. Even the theoretical work on the subject is still exploratory. Once the basic concept was placed in doubt, arguments of high pressure areas away from the equator, by themselves are insufficient to prove the transport of air across the equator. If upper air data from E. Africa and S.E. Asia had been used as they arrived, the equally untenable result that the air crosses the equator daily in the Indian longitudes might have been obtained. It was, therefore, necessary to trace, however sketchily it might be, the progress of the S.W. monsoon across the equator and to give reasons why air did not cross the equator daily. It does not help any one to dismiss the value of equatorial observations on any score. An intensive study near about the boundaries leads often to advances in science.

The value of island observatories in preference to moving ships has been stressed by me<sup>3</sup> (page 25). The climatic isobaric feature in the Indian Ocean and the reason why the streamlines expected from that picture had to be given up are explained in the same publication (page 26). The pressure conditions in the neighbourhood when the S.E. Trades cross the equator have been explicitly given, I believe, for the first time. None of the statements in 3 (iv) of Desai's paper is either a contradiction or an additional explanation of my statements.

Regarding 3 (v), the development of Transitional air (*Tr*) has been set out several times. I have stated under heading *Em* that "it starts from one of the high pressure cells in the southern hemisphere as dry continental air (*Tc*). In its westward travel, it gradually picks up moisture and additions

of dry air from the south. Later due to its moisture content, it can be recognised as giving rise to shallow low pressure areas. The air at this stage corresponds to the Far-Eastern Transitional or Mixed air (*Tr*) described later... When just about to cross and certainly after crossing the equator that air would have accumulated a large amount of energy and the temperature inversion disappears... The air at this stage is *Em*<sup>12</sup> (*vide* para 15). In the next para under *Tr*, it was stated by me that it "undergoes all stages in the transformation from *Tc* to *Em* in its travel from one of the high pressure cells upto its crossing the equator to the other side". Under the chapter on Medium-Long Range Forecasting<sup>3</sup> (pages 118-120) the ideas are similar. On page 120, I have said that "during the interval the air which is dry continental (*Tc*) would gradually become *Tr*\* and on crossing the equator Equatorial Maritime". Among the examples given in the papers referred to, some of the westward moving pulses caused clouding and showers at Cocos Island while others gave only a wind shift showing that sometimes but not always the air at Cocos Island may even be moist or maritime. The statements above and a careful reading of the other papers would show that Desai's criticism that "it is difficult to see how low pressure areas or their secondaries will travel all the way from near S. America to the Indian longitudes, a distance of about 15,000 miles without being modified or filling up" misses the main issue. How a set of actions is transmitted on progressively from the northern side of the high pressure area in S. America along the belt of the S.E. Trades upto the time the air crosses into the N. Indian Ocean is what has been described. The centres of action which I have pointed out would not have arisen if the stream was expected to travel unmodified. While there may be discrete anticyclonic cells in the southern hemisphere, one cannot ignore the fact that these are in a marked high pressure belt and that hence one cannot expect air from the south side of the anticyclonic cells to be the main monsoon stream (*i.e.* the path cannot be from the roaring forties to the

\* In the original this was misprinted as *Tm*

Indian monsoon low). The question of my being contradicted by or the critic finding 'support' of either Pandal<sup>20</sup> or Hogan<sup>25</sup> does not arise.

In 4 (ii), Desai assumes that the feeding of easterly waves or low pressure areas across the S. Indian Peninsula to the western disturbances of N.W. India was a common knowledge. I first put it in my notes<sup>1</sup> in 1943 and stressed it in the book<sup>3</sup>. As I felt an apparent contradiction between the statements on pages 109-110 and page 111 of the book, the problem was made more categorical in my note on Wet and Dry Western Disturbances<sup>7</sup> and in the notes on thunderstorms<sup>15-17</sup>. In the Technical Note on Western Disturbances or winter depressions by Mull and Desai<sup>19</sup>, the synoptic charts have not even observations plotted in the extreme south of India across which these low pressure waves usually travel. In the descriptive portion also there is nothing categorical about the origin of any air mass from the Trades.

In 1943-44, the concept of using low pressure waves from the east during the S.W. monsoon leading ultimately to the idea of the Far Eastern Transitional air (*Tr*) was not published and hardly thought of. Roy and Roy<sup>27</sup> introduced the concept of desiccated and deflected monsoon air due to orography of N.E. India. Ramanathan and Ramakrishnan<sup>22-23</sup>, and Sur<sup>24</sup> talked of old monsoon air. Ramanathan and Ramakrishnan stated "When near the Burma coast, the old monsoon air is often mixed with air from Upper Burma and Siam, especially at levels above 2 km. The latter may be expected to be similar in properties to old monsoon air coming as it does from the region of rainfall of the Chinese monsoon". Sur found that the equivalent potential temperature of the old monsoon air was higher than that of the fresh monsoon air but still designated it as 'deflected branch of the southwest monsoon'. The equivalent potential temperature would not change appreciably by deflection and desiccation. The recognition of this air as a distinct one with a distinct ultimate origin in the Pacific Ocean highs is not, therefore, a restatement of a well

known concept in Indian Meteorology, while the footnote under *I* seems to give an impression that this air mass was very well recognised.

As regards 2 (iv) of how there could or would be a feed of *Tc* (Tropical Continental air) to a depression to the east of another already existing depression is apparently answered by the counter-question of how there could or would be a feed of *Tr* or easterly air to a tropical depression west of another one or even perhaps by the problem of how more than one extra-tropical depression can form on the same extended front. The diagram of the tropical cyclonic storm has taken account of many possible doubts that might arise.

Regarding 4 about the interaction of weather from one hemisphere to the other, the answers are simple.

The feed of the N.E. trades to the south of the equator in the Indian Ocean strengthens the southern monsoon there. The feed of the N.E. Trades to the western disturbances over India activates them. It cannot be illogical to connect the feeding or otherwise of the N.E. Trades to one or other phenomenon: Western Disturbances over India or to the strengthening of the S. Indian monsoon. The feed to and the stimulation of one must inevitably contra-indicate the feed to and the stimulation to the other. With this trend of argument and with a reasonable amount of evidence, one should be allowed to draw the conclusion that the Western Disturbances over India would not be very active when, in a small longitude interval from the above, a southern monsoon depression exists or is forming in not too southerly a latitude. Considering the magnitude of zonal circulations, the distances involved are not unreasonable. The connecting up of the Western Disturbances and southern depressions should be done successively as in the memoir<sup>18</sup> and not over the whole season. In one particular year it is possible to explain how, the rainfall due to Western Disturbances may be abundant even though southern depressions existed *over the whole season*.

Similar arguments apply in the case of two tropical depressions on either side of the

equator. Daily comparisons on two sides of the equator have been given in the memoir<sup>18</sup>. The restriction of small longitudinal interval prevents lumping of unrelated phenomena.

Desai is willing to accept under 3 (iii) the relation between the highs in the Mozambique and the subsequent monsoon depressions in northern summer though the order of distances are equally large as in the cases under objection here. The high pressure area over the Mozambique leads to drier weather there and the monsoon depressions lead to more precipitation over portions of India. This result is logically the counterpart of the statement by me that the weather over a good portion of India would be dry with a tropical depression just south of India in the S. Indian Ocean.

Under 2 and 5 questioning whether it is the role of *Em* or of *Tr* for certain events, in para 19,<sup>12</sup> it has been pointed out that the air existing in certain areas in the periods in between the discontinuous incursions, it might lead to differing origins of the Indian monsoon. The N.E. Trades which have almost approached the equator and which have later had for some reason a northward motion would be at some distance away from the equator indistinguishable from the Equatorial Maritime air from the other side of the equator. By using data from only one side of the equator, it can easily be seen that there could be a mix up between the *Em* and the deflected Trades. The only solution would be to discuss with common data from both sides of the hemisphere to decide whether a particular event is the result of incursion of *Em* from the other side of the equator or due to deflected Trades from the same side of the equator. As such the existing literature does not contradict me.

The use of tropical radiosonde data with reference to Desai's 6 (ii) had already been explained<sup>13</sup>. "When the data are used along with synoptic charts, their utility cannot be challenged. The incursion of fresh air masses can be checked up alternately

from surface and upper air data. When all the T- $\phi$  grams are studied, the shape of the curves indicates the incursion of fresh air masses. The fall or rise in temperature at higher levels, if shown in a sequence at a number of stations far away from one another or if shown simultaneously at nearby stations can be relied upon and should not be ignored". The radiosonde data used by me and Pisharoty extended over both space and time. The westward travel of rain belts and of low pressure areas from S. China to N.E. India when the field is westerly is quite common. The reasons for the above must be similar to that of movement of air masses. A possible answer was given in para 6.<sup>14</sup>

As regards the mean position of the I.T.F. in various months (2 particularly ii and iii), there is hardly any information about the scatter or the standard deviation in the mean values. Variation of monsoon trough from its mean position even over India in June is considerable. To rule out *daily synoptic possibilities* of whether air could cross the equator due to more southerly *mean position* of the I.T.F. seems a doubtful procedure.

Regarding 6 (i) read with 3 (iii) and 7 (2), the conclusions will have to be based on the available meagre data of island stations listed in 6 in addition to any stray reports for a long time to come whether one works on low or high pressure areas. Data from many of these islands and the radiosonde data from S. China were not available before the war and with the closing of the war they are no longer available.

I have shown in a few cases that the criticism of my points of view has not been quite just. Space prevents a more detailed exposition and justification of all the points raised.

Colaba Observatory,  
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April 23, 1951.

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For references please see next page

[This controversy will not be pursued further in this journal —Editor]

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