DISCUSSION

Comment

(Comment on the paper, '**Mineralogy, Geochemistry and Genesis of Middle Riphean Phosphatic Carbonates, Tirohan Limestone (Lower Vindhyan Supergroup), Chitrakut Area, Central India**', by S. Kumar, published in the Journal of the Geological Society of India, Vol. 41, No. 2, pp. 133-143, 1993).

The author should be congratulated for the above paper. I would like to draw the attention of the learned author to the following points.

1. Title does not fully reflect the content of the paper, particularly the use of the term 'phosphatic carbonate' which should better be replaced by 'phosphatic material associated with carbonate'.
2. Nowhere has it been mentioned that the phosphatic material was freed from the associated carbonate as suggested by McClellan and Lehr (1969), McClellan (1980), Jervis (1980) and Banerjee *et al.* (1980) in their studies of the mineralogy of phosphorite.
3. In Table III, fluorapatite should be replaced by carbonate fluorapatite.
4. Method of extraction of organic carbon should have been given.
5. In Table III 'Geochemical and mineralogical data . . . . .' have not been normalised for correlation coefficient. In Table IV confidence level has not been given. It is well known that linearity of relationship, homoscedasticity, continuity of the variable and normality of distribution should be ascertained before the computation of correlation coefficient.

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Reply

I am thankful to Dr. S. K. Tripathi and H. P. Sengupta for their interest and comments on my paper. I am submitting below my reply to their comments itemwise :

1. I do not agree with the authors that 'phosphatic carbonate' should have been replaced by "phosphatic material associated with carbonate". It would have made the title too long without appreciably improving the title.
2. It is specifically mentioned that collophane was hand picked.
3. I agree with the suggestion. In Table III fluorapatite refers to carbonate fluorapatite.
4. Method for determining organic carbon content is given under the heading 'Stable Carbon Isotope'. The same material which was used for determining organic carbon isotope was also used for determining the organic carbon with the help of Carlo-Erba Elemental Analyser. For determining organic carbon isotope it is more or less a prerequisite to know the amount of organic carbon present in the samples. Preparation of samples for organic carbon isotope determination is given under the above mentioned heading.
5. Correlation coefficient given in Table IV is a spearman correlation coefficient which was computed by using a computor programme.

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Comment

(Comment on the paper, **Evolution of Southern Part of Indian East Coast Basins'**, by K. N. Prabhakar and P. L. Zutshi, published in the Journal of the Geological Society of India, Vol. 41, No. 3, pp. 215-230).

I would like to congratulate the authors on deriving a useful evolutionary model from the large amount of data now available for eastern India. They have initiated a powerful tool for hydrocarbon exploration. Of course, the model itself might have to evolve if it is to retain its vitality.

In their abstract, the authors refer to "shear tectonics associated with differential movements during the long flight of the Indian sub-plate". Later, they say that basin configuration is controlled by en-echelon horsts and grabens and that "the Bapatla ridge is affected by a translatory tectonic movement". However, the model they present comprises just four phases: rift, pull-apart, shelf-sag and continental margin tilt. This amounts to a "full cycle of evolution of the east coast basins typical of a divergent margin".

The authors may not be aware of the study by Goel *et al.* (1991). In that study, both the sedimentation and the structure of the Tatipaka Field in the Krishna-Godavari Basin and of the Nanilam Field in the Cauvery Basin were found to be controlled by strike-slip faults, sub-parallel to each basin's margin. The en-echelon Palaeocene fault system at Tatipaka, the positive flower structure at Nanilam and the anti-clockwise rotation of Sri Lanka are all diagnostic of sinistral shear, directed along the coastline. Therefore, the eastern margin of the continental sub-plate was probably transcurrent, not purely divergent, if only during Prabhakar and Zutshi's tilt phase, with post-Cretaceous tectonics and volcanics. Prabhakar and Zutshi also note that the "Pennar and Palar basins do not

appear to have undergone any major transformation beyond the rift phase". This could be a simple consequence of that coastal segment's complementary trend. On a transcurrent margin, both transtension and transpression can occur, in accordance with the local strike.

In conclusion, it follows that the 1–2D basin modelling used for divergent continental margins will not suffice for Indian coastal basins. 3D Finite Element analyses should be more appropriate for the petroleum migration and entrapment processes observed. It also follows that we need to use the Indian sub-plate's 'flight-recorder'. India's crash landing into the belly of Asia may have partially decoupled the sub-continent from her oceanic companion. The sedimentary and structural controls of some of India's most challenging oil and gas fields were set during her "long flight".

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Reply

The authors thank Westerman for his critical comments. Additional studies carried out in Cauvery basin data suggest presence of transtensional and transpressional tectonic indices.

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TOZO WILSON

In the Obituary Note on Prof. Tuzo Wilson, (Jour. Geol. Soc. India, Vol. 42, No. 2, August 1993), it was mentioned that Prof. Wilson did not come to India. Dr. M. N. Qureshy and Prof. K. V. Subbarao have since written to say that Prof. Wilson did visit India in 1975 and gave lectures at the Indian National Science Academy and Aligarh Muslim University.

Dr. Qureshy adds – "One of his most outstanding traits was his leadership quality. He enthused many a physicist and mathematician to deliberate on geophysics".