DISCUSSION

Comments by V. J. Gupta

1

(Comment on 'Indian Palaeontology under a Cloud' Editorial, published in the Journal of the Geological Society of India, Vol. 34, No. 6, 1989, pp. 561-563.)

The 'Editorial' published under the above-mentioned head by the 'Editor' is biased and partisan in view of the following:

The 'Editor' has overlooked the fact that the paper on 'Devonian Ostracodes from the Lower Spiti' was published jointly by S. B. Bhatia, S. P. Jain and V. J. Gupta. I happen to be the third author in the paper, whereas S. B. Bhatia is the first author in this publication. The specimens forming the basis of this paper were collected jointly by members of the field party and no collection was made by me alone. The following statement in the 'Introduction' part of the paper (Bhatia *et al.* 1982) confirms my statement: '. . . . The material for the paper was collected by the authors during their 1966 expedition to the lower Spiti valley. No detailed mapping of the area could be carried out as it lies close to the India-China international border. A generalized geological map cannot be given for obvious reasons'.

The above-mentioned paper was orally presented by S. B. Bhatia at the 'International Symposium on Himalayan Geology' organized by the Geological Survey of India in New Delhi in 1976. The proceedings of the symposium were published as late as in 1982. This leaves a span of 16 years from the date of collection of the material and the publication of the paper. The learned first author of the paper had enough time to withdraw his name from the publication. Bhatia did not disown the publication or the material till 1989 (23 years after the collection), when the present fossil controversy started. To evade responsibility as the first author in the paper he has published a note in *Nature* (Bhatia, 1989) referred to in the 'Editorial'. Bhatia probably could not realise his mistake in disbelieving the 'genuineness of his fossil collections' and 'unwittingly' became a partner in the so called deception committed by me as observed in the 'Editorial'. I have full faith in the merit of Prof. Bhatia who had been my teacher as well. He has been internationally recognised as an expert on ostracodes but he *nevertheless failed to observe the so-called 'discrepancies'* alleged to have been perpetrated by the third author of the paper.

Talent and his associates have always tried to cook-up, twist, recycle and distort facts with the sole intent of maligning me for reasons best known to them. Unfortunately, our honoured 'Editor' has fallen prey to their conspiracy and not followed the norms of referring the articles to me for comments before publication, since the papers were exclusively based on allegations made against me. I may very well appreciate the situation under which he might have desired to do so rather than following the normal practice of scientific journalism. I leave it to the esteemed subscribers of this reputed journal and the geoscientific community at large to justify the intentions of these writings on their own.

2

(Comment on the paper 'Himalayan Palaeontologie Data base polluted by recycling and other anomalies' by J. A. Talent and others published in the Journal of the Geological Society of India, Vol. 34, No. 6, 1989, pp. 575-586).

I am constrained to point out that the allegations made by Talent *et al.* (1989) are themselves nothing but recycling of their own points made in the paper/articles/ reports published in different journals/newspapers/magazines, etc. during the last about a year. Any unbiased reader of all the papers published by Talent and his co-authors can very well observe that the allegations made by them are exclusively based on literature and all these seem to be self-justifying value judgements. To what extent such writings about scientific publications are worthy of consideration is a thing which must be judged by the community at large itself. It is one thing to allege of impropriety, misjudgement, recycling, that is allied to politics and journalism etc. It is very different to provide detailed scientific work. The former can be worked up easily overnight. For the latter, one has to be dedicated and competent to arrive at tangible conclusions.

There is little demonstration of any scientific basis about the bizarreness as referred to by Talent *et al.* (1989).

The Zanskar valley of Ladakh and other areas have been opened up by the Govt. of India to boost up 'Tourism'. It is not easy for us even to-day to buy topographic sheets of these areas easily from the Survey of India. Many of the foreign geoscientists have exploited this liberal pro-tourist attitude of the Govt. of India to conduct geological research. My observation is substantiated by the maps and other geological accounts published by some of the foreign geoscientists in the scientific journals after their return from the so-called trekking trips in the Himalayas. A detailed study of these publications (including maps) will clearly reveal that many of the scientists visiting these regions of the Himalaya have digressed from the permissible routes meant for trekking, to the adjoining prohibited areas in spite of restrictions/instructions from the Govt. of India. As an Indian, it is not possible for me to take such liberties and to go against the 'Law of the Land'.

To comment on some of the *trivial* issues that are initiated beyond belief by my critics :

The closure of Chharap valley to foreign visitors and its difficulty of accessibility until 1978 seems to have nothing to do with the real point of criticism. Talent and his colleagues have not visited even other areas about which sweeping statements have been made without any basis or justification.

I do not much understand the remarks of Talent *et al.* (1989) on the specimens recorded as *Spinocyrtia* by Gupta (1987). How can these authors be so confident about these specimens being juvenile forms of *Euryspirifer tonkionsis* (Mansuy) without their having even looked at them.

The conodont fauna recorded from the Lower Carboniferous rocks exposed in different parts of Himalaya (Ladakh, Lahul and Spiti) came from the localities from where they have been reported. The specimens recorded from different sections are available for verification. The difference in the identification of some of the closely identical/similar specimens collected from different regions (Ladakh and Spiti)

referred to in two different publications within a span of 10 years is due to the availability of additional literature and better understanding of the subject. The sample No. Bu 5009 from the Guryul Ravine section of Kashmir was collected by Dr. K. J. Budurov himself. I did not even accompany Dr. Budurov for fieldwork to Kashmir. The sample under reference was also macerated by Dr. K. J. Budurov himself. However, the identification of conodonts recorded and finalisation of the manuscript was done jointly by us during my visit to Sophia (Bulgaria).

I did visit Aberystwyth in 1967. I revisited University College of Wales, Aberystwyth in August, 1985. No mention of the coral slides as referred to by Talent *et al.* (1989) was made by anyone at the Institute to me during my visit in 1985.

I visited Japan under INSA-JSPS exchange of scientists programme in October, 1989. I had been in Hokkaido University, Sapporo from 6th to 13th October, 1989 and met Prof. M. Kato on 7th October, 1989. We had detailed discussion on our joint paper which was to be sent for publication. We even discussed my having published a short note regarding the occurrence of the corals under discussion to have priority. The paper jointly with Prof. Kato was submitted for publication only after my visit to Sapporo and detailed discussion with him. This can be verified from the date of submission of the manuscript by the 'Editorlal office' of the journal *(Journ. Fac. Sci., Hokkaido University)* and its subsequent publication.

The fossil localities of corals described by Gupta (1985) seems to have been distorted. It is clearly mentioned in the paper that the fossils described were collected from near the village Tanze (34°08′30″, 77°13′15″)*. The reference to the occurrence of siliceous limestone near Surichun La was made in context to the conodont and brachiopod assemblage described by Gupta and Kachroo (1977). However, reference to the record of corals of Visean age was intended to be from the limestone exposed near Tanze which incidentally is not very far from Surichun La. I do admit poor drafting of the paragraph in the paper under reference.

As already clarified (Gupta, 1990) the plates containing Fossil 1–3 accompanying the paper on the Lower Carboniferous corals from Baralacha Ban area got interchanged with that of corals from the Luneak Formation exposed near Tanze, Ladakh. I would have detected this lapse on my part if proofs of the paper had been sent to me before publication.

I do not follow the points raised by Talent *et al.* (1989) regarding reports of Pleistocene vertebrate fauna from Nepal. It is a well-known fact that the lacustrine deposits constituting upper part of the Karewa Formation in Kashmir have yielded diverse vertebrate fauna corresponding to the Pinjor Formation of Siwalik Group. The occurrence of similar vertebrate fauna in neighbouring Nepal is not unexpected (Gupta, 1975, 1988).

Talent *et al.* (1989) have intentionally evaded reference to the publication by West and Munthe (1981) wherein vertebrate fauna from Gidhniya village in western Nepal was first recorded and illustrated. My paper (Gupta, 1983) referred to by Talent *et al.* (1989) appeared two years after the publicatlon of paper by West and Munthe (1981). Reference to this paper has also been made by West *et al.* (1988) and Jens Munthe happens to be one of the authors in this paper also. Conroy *et al.* (1985) have summarised the various records of fossils. Since the publication of these papers Cornvinus (1985) has recorded vertebrate fauna corresponding to

* (Tanze does not fall on latitude 34°08′30″ – another instance of the Care-free attitude in giving correct locality information—*Ed.*)

Pinjor Formation from a number of localities on the newly constructed road from the Terai into Rapti Deokhuri valley of Nepal. In the recent past occurrence of vertebrate fossils has also been recorded from a number of localities by Dongol (1985) along the lower reaches of the Bagmati River, downstream of Chobar Gorge.

I had no idea of my being one of the authors in the paper by West *et al.* (1988) till I received the proceedings volume. This is in view of the fact that the manuscript of the paper was not sent to me at any stage during its preparation, finalisation or publication. I did not participate in the symposium held in Hongkong where this paper seems to have been presented.

3

(Comment on the paper "The Stratigraphy of the Kinnaur Tethyan Basin—A reappraisal" by U. K. Bassi published in the Journal of the Geological Society of India, Vol. 34, No. 6, 1989, pp. 587-596.)

The stratigraphic positions assigned by Bassi (1989b) to different stratigraphic units appear to be arbitrary, without supporting evidence. The ages assigned to these units are open to modification/revision on the basis of reliable data which may be made available by subsequent workers. The assemblage of various fossils referred to in the paper are not characteristic ones from a stratigraphic point of view and hence do not support the conclusions drawn by Bassi. It is surprising that Bassi (1989b) has not referred to some of his own publications recording fossils from Tidong valley, and their stratigraphic implications. Some of these papers have been referred to at the end of the paper under the title but no mention of some of these is made in text of the paper. To cite some of these papers it may be mentioned that the reference in the text of the paper on his find of fossils from the Muth Formation (Bassi, 1988) and their stratigraphic implication is a glaring omission. In addition, Bhargava and Bassi (1986, 1987) recorded varied fauna from different horizons of the Manchap Formation of type locality which has been divided by them into four litho-units. The fauna recorded from this formation includes rich variety of brachiopods, gastropods, cephalopods, bivalves, corals, algae, stromatoporids, bryozoans, foraminifers, pteropods, ostracodes, crinoids, sponge spicules, trace fossils and plant fossils *(Psilophyton princeps)*. A detailed discussion on the stratigraphic implications of this fauna might have served a useful purpose for the subsequent workers. Chopra *et al.* (1980) have also recorded the occurrence of *Salopina*, orthids, etc. from the Muth Quartzite succession of Khimokul La Section. According to the authors (which includes Bassi and two other geoscientists of the Geological Survey of India) the Muth Quartzite at this locality is overlain by a thin band of haematitic quartzite. In addition, Bassi *et al.* (1983) had recorded the occurrence of *Orthis* sp. and *Rhynchonella* sp. from Khimokul La section and 12 km SE of Charang. Occurrence of *Heliolites* and burrow marks have been recorded by these authors from the Jogigchen area.

It is surprising that Bassi (1989) does not discuss details regarding the stratigraphic positions of the Gechang and Gungri Members of the Kuling Formation. He does not refer to the paper by Chopra *et al* (1980), wherein he himself is one of the authors.

It may be pointed out that *Eurydesma* is essentially of Lower Permian (Asselian) age, whereas *Lamnimargus Himalayensis* is fairly widespread throughout the Himalayas and is considered to represent the upper part of Chhidruan (Hachiskian) substage of the Punjabian Stage. *Cyclolobus* is believed to be of Djulfian age.

The association of *Spiriferella rajah* and *Eurydesma* is unusual. *Spiriferella rajah* is generally believed to be of Punjabian stage. However, in parts of Nepal, occurrence of *Spiriferella rajah* has been recorded from beds which have been correlated with the Dorashamian Stage (Waterhouse, 1978).

The statement of Bassi (1989 b) that I changed the lithocolumn supplied by him for our paper on *Neogondolella regale* is not correct. The lithocolumn was finalised by Bassi and Ahluwalia before being handed over to me. This lithocolumn was submitted for publication unchanged.

The manuscript of the paper on '*Neogondolella regale* from the Tidong valley—India' was first sent for publication in the Journal, Geological Society of India alongwith the stratigraphic column under reference. This paper (J/3038) was duly accepted for publication vide letter dated 21-10-1988 addressed to me by the editor. The original stratigraphic column and plate submitted with this paper are still lying with the 'Editorial office' of the Geological Society of India. This paper under reference was withdrawn from the Geological Society of India by me as 'Editor' asked me to '—arrange to furnish better photographs'. As it was not possible for us to arrange for better photographs with the available photographic facilities at Chandigarh (SEM photographs had already been sent) we were left with no alternative but to withdraw the paper and to publish it in Bull. Indian Geol. Assoc. The reference to the above paper has been made intentionally to enable the Editor to verify himself from the available records that no change in stratigraphic column was made by me as alleged by Bassi (1989b). A look at the stratigraphic column published in Chopra *et al.* (1982) and Bassi (1988) will substantiate my point of view that the allegations regarding stratigraphic column published in the paper by Bassi *et al.* (1988) are unfounded and made perhaps to blame me for the error made by another.

The statement by Bassi (1989b) that the report of Carnian conodont from Khimokul La can't be true as no rocks younger than Lower Triassic exist at this pass on Indo-Tibet border is in contradiction to some of his own publications. In one of his papers (Chopra *et al.* 1982) published in 1982 with Suresh Chopra (from G.S.I.) as the first author and himself as the second author (amongst others) the following statement has been made on p. 286 of the paper which runs contrary to his own writings in *Nature* (Bassi, 1989 b, p. 286): '. . . . In addition to the Anisian conodonts and other microfossils, the upper units of Triassic succession of the Tidong valley has also yielded representatives of *Paragondolella polygnathiforms* of Carnian age . . . . '.

It may be reasserted that Carnian and younger sequence is exposed in the Khimokul La section of Kinnaur region. Bassi (1989 b) does not provide any supporting evidence for the authenticity with which he has given statement for the non-existence of rocks younger than Lower Triassic in this region.

In contradiction to the claims made earlier, Bassi (1989 b) stated that no rocks younger than Ladinic occur in the Khimokul La section. Which of the contradicting statements published by the same person within a span of three months should be relied upon? This bespeaks of the malafide intentions of Bassi. In fact, his allegations are basically directed against his own papers.

. For the information of the geoscientific community I may mention that I visited the area under reference in 1974 long before Bassi or his colleagues started field-work there.

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Reply by S. B. Bhatia

Certain clarifications are necessary regarding Gupta's comments on Devonian ostracodes published above and also those published elsewhere (Gupta, 1990). These clarifications, I hope, will put the record straight in so far as the Devonian ostracodes from Spiti are concerned :

1. I stand by my statement (Bhatia, 1989) that the sample containing Devonian ostracodes was provided to me by Gupta in 1972, prior to my departure for U.K. Neither Jain nor I had processed any of the samples collected during the 1966 expedition to Spiti. *It is Gupta's words against mine.* It is he who stands indicted before the scientific community, not I.

2. The statement 'The material for the present paper was collected by the authors . . . . . ' (Bhatia *et al.* 1982) on the basis of which Gupta (1990; and *this volume*) wants to put me in the dock, however, certainly needs a clarification. In the late sixties and early seventies, when we (Gupta, Jain and myself) started working on ostracodes from Spiti (on collections made during 1965 and 1966 expeditions), we had an unwritten understanding that all papers on ostracodes will be published by us jointly without indicating the precise extent of involvement or division of labour between the three of us. It was on the basis of this tacit understanding, and on the premise (in good faith) that the sample provided to me by Gupta in 1972 had genuinely been processed by him personally from material collected during our 1966 expedition, that the above quoted statement was included in our paper and Jain's name figured as a co-author, although he (Jain) was not responsible for collecting any material from Kurig, Po and other localities during the last leg of the 1966 expedition. Jain, however, was responsible for finalizing the part dealing with systematics (the identifications had been done by me earlier at the British Museum, Natural History in 1972), and Gupta was responsible for drafting and finalising parts dealing with introduction, geological setting and bibliography. I have documentary evidence of Gupta's deep involvement in the preparation of our paper between 1972 and 1975 and its finalization in early 1976 for presentation later in the year at the Himalayan Geology Seminar at Delhi. I mention these details because during this period (1975 to be precise), Gupta was busy