

## DISCUSSION

### BIOSTRATIGRAPHIC STUDIES AND CORRELATION OF THE MIDDLE CAMBRIAN SUCCESSIONS OF NORTHWESTERN KASHMIR HIMALAYA

by S K. Parcha Jour Geol. Soc. India, v 65, 2005, pp 183-196.

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In his recent paper my friend and close collaborator Dr S K Parcha (2005, p 185) provides a revised biostratigraphy for the middle Cambrian faunal succession of NW Kashmir that contrasts in important ways from the one that Peter Jell and I proposed some years ago (Jell and Hughes, 1997, Hughes, 1997) The purpose of this note is to clarify the key differences in our interpretations and discuss their implications I welcome this opportunity because scientific debate is essential to the development of biostratigraphic clarity

Dr Parcha's Kashmiri stratigraphy supports a view of the differences between the Cambrian geology of Kashmir and Spiti that has a long pedigree in the literature Reed (1934) considered coeval trilobite faunas from these areas to be highly distinctive, and suggested profound palaeogeographic differences between the two regions Shah (1993) has preferred a palaeoenvironmental explanation for the difference, a view echoed by Parcha (p 191) who attributes the Kashmir faunas to an inner detrital belt setting and the Spiti faunas to an outer detrital belt Jell and Hughes (1997) provided an alternative explanation for most of the difference between the Kashmir and Spiti faunas those from Spiti largely predate those from Kashmir, and this is the primary reason why they are different Sedimentological investigations by a team including Dr Paul Myrow, Dr Parcha, myself and others conclusively reject the idea that the middle Cambrian rocks of Zaskar and Spiti represent an outer detrital belt setting (Myrow et al *in press*) Our joint taxonomic work in progress on extensive new collections from Spiti and Zaskar will allow us to assess the Jell and Hughes (1997) age estimates for the Parahio Valley section In this regard it is encouraging that the Cambrian Stratigraphic Subcommittee is considering the first occurrence of *Oryctocephalus indicus* as a datum for formal definition of the base of a middle Cambrian Series (Geyer, 2005)

Dr Parcha and I agree that the Pohru Valley contains trilobites that are diagnostic of late early Cambrian

(*Redlichia takoensis*), latest middle Cambrian (*Damesella shergoldi*), and early late Cambrian (*Cyclorenzella* sp, *Monkaspis* cf *M serrata* and *Blackwelderia* sp) ages The difference between our interpretations concerns the stratigraphic range encompassed by the older middle Cambrian trilobites known from this region Jell and I viewed the assemblages that contain the distinctive trilobites *Tonkinella breviceps*, *Baliella lantenoisi*, *Hundwarella memor*, *Iranoleesia butes*, and *Shahaspis himalayensis* to represent only the medial stage of the north Chinese stadial system for the middle Cambrian, namely the Hsuchuangian Stage Indeed, I think it likely that they represent only the upper part of the Hsuchuangian Stage On the other hand, Dr Parcha suggests that these collections encompass all three stages of the middle Cambrian (i.e. the Maochuangian, Hsuchuangian, and Changian Stages of the north Chinese scheme)

The view presented by Dr Parcha is appealing in that it suggests relatively continuous sampling of trilobites from the middle Cambrian of NW Kashmir, and Fig 2 of his paper suggests that the rock thickness between occurrences of *Redlichia takoensis* and the first appearance datum of *Tonkinella breviceps*, is less than the thickness of the range of *Tonkinella* itself at the Putshai-Kandi section, although actual measurements were not given Similarly, the stratigraphic interval between the last appearance of *Shahaspis himalayensis* and the first appearance of late middle Cambrian trilobite *Damesella shergoldi* also appears to be relatively thin However, biostratigraphical practice requires that we estimate time via taxon identification, not according to height in a section and it is for this reason that biostratigraphy is effective in the recognition of disconformities Hence the key issue in judging what stages are represented within this interval is the identification of age-diagnostic taxa, not the stratigraphic position in the section where specimens were collected relative to other taxa

In our 1997 monograph Jell and I acknowledged that the biostratigraphical placement of the Kashmiri faunas in this interval was incompletely resolved but we argued that all taxa sufficiently well preserved to permit identification are of Hsuchuangian age The reasoning was based on their

taxonomic affinities with biostratigraphically well-constrained forms known from elsewhere, principally from China. Our work involved substantial taxonomic revision of faunas previously described from Kashmir, and Dr Parcha accepts most of these reassignments. Since he and I agree that some portion of these faunas is Hsuehuangian, the critical difference between our viewpoints is the basis for the new Maochuangian and Changian age determinations. Dr Parcha suggests that his *Solenopfeura-Tonkinella* Zone fauna is Maochuangian. I disagree with this estimate for the following reasons: (1) *Tonkinella breviceps* does not occur in the earliest middle Cambrian in other regions of the globe, (2) none of the other taxa listed as occurring in this zone in Parcha's Table 2 are diagnostic of the Maochuangian Stage, and (3) *Tonkinella breviceps* occurs in the mid-middle Cambrian in other regions - as one of the most derived oryctocephalids its relatively late stratigraphic appearance is well known. Dr Parcha acknowledges this by including the top of the range of Kashmiri *Tonkinella* within the Hsuehuangian Stage. Accordingly, I see no evidence suggesting that the lowest parts of the trilobite-bearing Nutunus Formation in the Pohru Valley are Maochuangian in age. Similarly, Jell and I made the case that relatives of *Shahaspis* among the Wuanidae and Inouyidae, and the co-occurrence of *Latilorenzella* sp. strongly argue for a pre-Changian age for this assemblage. Dr Parcha does not refute this suggestion but illustrates a specimen (pl 1 fig P) identified as *Diplagnostus* that he suggests "marks the boundary between the Middle and Late Cambrian" (p 190). *Diplagnostus* is known to occur quite near the base of the Changian (Sun, 1989) (i.e. well within the Middle Cambrian as conventionally defined) but the object in the plate is not clear and apparently lacks the preglabellar medial furrow that is diagnostic of *Diplagnostus*. I consider the evidence of age provided by *Shahaspis himalayensis* and *Latilorenzella* sp. to remain preferable. Accordingly, I reject the idea that the beds containing these taxa are Changian and prefer an Hsuehuangian assignment.

This difference in opinion has significant implications for the understanding of correlations, stratigraphic evolution, and palaeoenvironments along the Tethyan Himalayan margin. Dr Parcha (p 185) notes that temporal resolution within this interval is "rather vague" and that the "different assemblage zones overlap each other". There are also some inconsistencies between the text, figures, and table in the definition of these zones in Dr Parcha's paper. For example, on p 189 the text states that the appearance of *Hundwarella* marks the beginning of the second (i.e. *Tonkinella-Hundwarella* Zone, but Table 2 lists *Hundwarella memor*

as occurring in the first (i.e. *Solenopfeura-Tonkinella*) zone. In Table 2 *Bailiella* is listed to occur only in the *Hundwarella-Bailiella* zone where it is assigned a Hsuehuangian age. However, in Fig 1 *Bailiella* from the Takwodhapora-Khanpura section is shown as belonging to the Maochuangian Stage. Hence the paper does not appear to strengthen the case for recognition of the *Solenopfeura-Tonkinella*, *Tonkinella-Hundwarella*, and *Hundwarella-Bailiella* Zones in Kashmir. According to the Jell and Hughes view the limited biostratigraphical resolution evident within the Nutunus Formation is to be expected because the time represented is relatively short. But if the formation represents almost the entire middle Cambrian then the poor temporal resolution evident is indeed surprising. More resolution would be predicted based on well-resolved Cambrian trilobite biostratigraphies known from other regions.

While I concur with Dr Parcha's concern (p 191) that the Jell and Hughes biostratigraphical scheme from the Cambrian of the Himalaya is preliminary, I am not convinced that a return to the previous conceptions advocated by Dr Parcha would be an improvement, nor did I see new facts presented that improve resolution of the issues at stake. What is most needed now is the careful description of detailed new fossil collections made in precise stratigraphical and sedimentological context in order to test the existing schemes. I am delighted that our joint work is moving towards achieving that goal. I am also delighted to have an opportunity to discuss these differences of opinion. This is how science should progress and this debate speaks of the vitality of studies of the Cambrian biostratigraphy of the Himalaya. Finally, Dr Parcha has my highest personal and professional regard. I look forward to continued long-term collaboration and further stimulating debates on differences of scientific opinion.

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I thank my friend Dr Nigel Hughes for his valuable comments on my paper entitled "Biostratigraphic Studies and Correlation of the Middle Cambrian Successions of Northwestern Kashmir Himalaya". There is no gainsaying the fact that debate is essential for progress of scientific thought and I welcome this discussion in that spirit.

Dr Hughes has given details of where he agrees and disagrees with my interpretations. Without going into the agreements I would concentrate on the disagreements. His main disagreements seem to be the following:

- 1 Regarding the difference between the Middle Cambrian fauna of Spiti-Zaskar and Kashmir I follow the model

proposed by Shah (1993) giving a palaeoenvironmental explanation for the difference. Dr Hughes prefers the alternative explanation given by Jell and Hughes (1997) stating that Spiti fauna predates that of Kashmir.

- 2 I have tentatively correlated the Middle Cambrian faunal elements of Kashmir with the Chinese stages viz Maochuangian, Hsuehuangian and Changian indicating that they range from Late Maochuangian to Changian. Dr Hughes thinks that the entire fauna except probably that of *Damesella* Zone is Hsuehuangian.

With regard to the first, the statement that the fauna of Spiti predates that of Kashmir is not borne out even by the own observations of Jell and Hughes (1997). In their text Fig 4, bulk of the faunas from both the places has been shown to occur at a single datum line in Hsuehuangian. Only *Oryctocephalus indicus* and some associated elements have been shown to occur earlier. At this datum level while Kashmir fauna is represented by *Tonkunella-Bailiella* Assemblage, the same is totally absent in Spiti and instead bears *Oryctocephalus salteri*. Obviously the faunal variation is not due to time difference. The theory may appear an easy explanation since correlation is not possible in this situation but it is illogical to imagine that there was a make and break situation between Kashmir and Spiti whereby when fauna was getting preserved at one place, there was a blank at other place and vice versa.

As regards the second, in the absence of distinctive and common faunal elements between India and China it is not possible precisely to follow the Chinese classification. However a tentative correlation can be undertaken. There is not much weight in the arguments of Dr Hughes to telescope the entire faunal range into a single stage viz Hsuehuangian, though bulk of the fauna can be correlated

to that stage. However, it is a matter of opinion. Dr Hughes seems to suggest that I have undertaken my correlation on the basis of the thickness of the sections. I can assure him that I have done no such thing nor have I indicated anywhere about that being a factor in correlation. But the fact remains that my work and that of Professor Shah and other members of that group is based on a careful bed by bed measurement of the various sections and faunal collection at precisely indicated levels. That is why I have incorporated a summarized version of all the sections. The interpretations can vary and so can the determinations of various taxa but the data will remain unchanged. Only additional data can be incorporated.

In the absence of any suitable zonation I have followed the zonation proposed by Shah (1982) who admits that they are overlapping each other, I find this zonation unsatisfactory and it has to be taken only as a workable classification till we have a better control.

It has to be emphasized that agnostids constitute the most significant faunal elements for Cambrian zonation. Very few reports of agnostids from Indian Cambrian were known earlier and it is only in recent years that some attention has been paid to this important group of trilobites (Whittington, 1986, Shah and Parcha, 1986, Shah and Sudan, 1987, Shah et al 1995, Parcha, 2001). However, the data is still unsatisfactory and additional work needs to be done in this area. I fail to understand the argument of Dr Hughes about *Diplagnostus*. *Diplagnostus* is known to be associated with *Lejopyge laevigata* the world over that marks the upper boundary of Middle Cambrian.

I am appreciative of the interest that Dr Hughes has shown in critically evaluating my paper. I reciprocate his sentiments and assure him that I have the highest esteem for him personally and professionally. I shall always look forward for his comments and scientific observations which can be useful for the advancement of science.

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**A GEOSTATISTICAL APPROACH TO RESOURCE EVALUATION OF KALTA IRON ORE DEPOSIT, SUNDERGARH DISTRICT, ORISSA** by B.C. Sarkar and Indranil Roy. *Jour. Geol. Soc. India*, v 65(5), pp 553-561

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At the outset I must congratulate the authors of the article and the Journal of the Geological Society of India, for publishing the article in geostatistics which is the state of art technology in estimation methods

1. Case histories are a must as they alone prove the efficacy of a method and make a method/model developed by the theoreticians, acceptable by the practitioners. In fact, not many articles on the practical geostatistics and on case histories of Indian mineral deposits, are forthcoming. Hence the authors deserve praise for their attempt to model Kalta iron ore deposit geostatistically.
2. The authors have successfully fitted with three parameters, log-normal distribution fit (p 560). Then, why they could not have adopted log-normal kriging, requires to be elaborated, as the same is not clear in the text. After all, if the grades are distributed log-normally, log-normal kriging is better estimator than ordinary kriging.
3. It is stated (p 560) that Rourkela Steel Plant (RSP) estimated by polygonal and cross-sectional methods an indicated reserve of 72.13 million tonnes. How many blocks of size 50 m x 50 m x 50 m, estimated (taking a cut-off grade of 57% Fe) which were estimated as waste i.e. below 57% of Fe, have been estimated as ore i.e. the estimates are greater than 57% Fe due to kriging, by you?
4. It is stated (p 560) that “the computed marginal increase in tonnage may be due to precise overlying of blocks on each of the level plans that considers the marginal ores”. This statement is not clear and requires clarification.
5. It is stated that in Kalta there are many ore types. By what precise method of calculation, the average specific gravity of 4.21 has been arrived at?
6. It is stated (p 560) that “the ore inventory and associated grade tonnage relations can be further refined by using a map of variation in specific gravity, which itself can be considered as a regional variable”. Normally, each ore is assigned a particular specific gravity, as the variation of specific gravity in the same ore type is more or less constant. Alternatively, even if variation is there, it is negligible. Whereas the grade of the variables like Fe vary even in the same ore types. Therefore, the density variation can be considered more as a mosaic rather than a regionalized variable in a strict sense.
7. The authors have drawn grade tonnage curves (Fig 6, p 560) based on the kriged estimates of blocks of size 50 m x 50 m x 10 m. There are two types of curves namely (i) cut-off grade (%Fe) vs tonnage of ore and (ii) cut-off grade (%Fe) vs average grade (%Fe) in the diagram. The second diagram is also called grade-mean curve. The authors have drawn it for one particular bench (715 mRL). For drawing curve of local recoverable reserves, disjunctive kriging (DK), which is also a non-linear geostatistical method is better suited. The technique of DK can be used both for kriging block by block and also calculate the recoverable reserves bench by bench. Murthy (1989) applied DK technique for an Indian iron ore deposit. I presume that the authors are aware of this.
8. It is very well known that each block estimate is