

I OFTEN RECOLLECT¹

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PROFESSOR MULK RAJ SAHNI MEMORIAL LECTURE
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DEPARTMENT OF GEOLOGY, LUCKNOW UNIVERSITY

I. INTRODUCTION

Mr. President, Fellows of the Palaeontological Society of India and Invited Ladies and Gentlemen.

I am much thankful to the Executive Council of the Palaeontological Society of India for doing me this honour by inviting me to deliver the first lecture in the series "Professor Mulk Raj Sahni Memorial Lecture" and thus to Inaugurate this series of lectures, as Dr. M. P. Singh, Secretary of the Society has said in his letter to me in this connection.

I understand it from the Secretary's letter that this lecture series is planned to be an annual feature to be held on March 1, which is Professor Sahni's birth day. I consider it the most appropriate way in which Palaeontological Society of India has decided to perpetuate the memory of the Founder President of the Society. And resources permitting, if I may suggest, the Society may think of instituting a Scholarship or a Medal to be awarded to a meritorious student of Geology at the M.Sc. examination of Lucknow University.

I wish them all success.

I wonder why such a great honour has been done to me by the Executive Council of the Palaeontological Society of India. As the Secretary says in his letter to me in this connection, I am chosen to deliver the Inaugural Lecture of this series because of my long association with Prof. Sahni and that long association of mine with Professor Sahni, if I may reveal it, extends over more than 45 years. During this long span of time I often had occasions to praise him for the signal services that he has rendered to the cause of Palaeontology in India by taking the lead in establishing this Society.

While I have expressed my praise to Professor Sahni for his sustained efforts in stabilising this society and nurturing it, I had, I must mention that also, a few occasions to criticise him in connection with the way of working of Executive Council of the Society. And I must

admit it very happily that he received my criticism quietly and without any protest; and with all that our friendship continued unaffected in any way.

I thankfully take this opportunity you have given me, to pay in the form of my today's lecture, my homage to my late lamented friend, Professor Mulk Raj Sahni. I hope you will take this lecture in that light.

My today's lecture will be not any thing of an academic feature, but it will be in the form of briefly narrating my recollections of some of the several discussions I had with him during his long period of our friendship. This way I believe I would be revealing his academic trend of mind and efforts at fostering the cause of Palaeontology and Stratigraphy in India. Being most of my life concerned with the Cretaceous rocks of Narmada valley and of South India, naturally in what follows more space would be taken by recollections of our discussions pertaining to these rock formations.

From Professor Raj Nath my teacher at the Benaras Hindu University, I had back in 1931, heard of Dr. Mulk Raj Sahni as a capable Palaeontologist. He had by then made valuable contributions to Palaeontology of Jurassic and Cretaceous Terebratulids of Great Britain.

Towards the middle of 1935 Dr. Sahni was transferred from the Burma Circle of the Geological Survey of India to the Calcutta office as Palaeontologist to the Geological Survey. I had then, as a research student, gone to Calcutta for consulting literature in the Library of the Geological Survey and for comparing my Bagh specimens with the Types in their collection. And that was the first occasion I met Dr. Sahni.

II. BAGH BEDS OF NARMADA VALLEY AND UPPER CRETACEOUS ROCKS OF SOUTH INDIA

He made kind inquiries about the way I had planned my work on the Bagh Beds and promised all possible help in my work. He very much appreciated my

¹This title is adapted from 'I Some Times Think', title of a collection of Reflective Essays by Stephen Paget which I had read in 1925.

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plan to cover the stratigraphy as well as palaeontological study of all the invertebrate groups of fossils in these rocks, because that would give me the full picture of those deposits and also because except Vredenburg's work (1907, 1908) on ammonoids no detailed and systematic work was done on the fauna of these rocks since the days of the first detailed field report on these rocks by P. N. Bose in 1884 where he had, however, given a list of fossils only tentatively identified by him.

On this occasion we had a very helpful discussion and he encouragingly justified my creation of a new genus *Malwirhynchia* for placement of the Bagh rhynchonellids which Bose had reported as *Rhynchonella plicatiloidea* Duncan. He offered appreciative comments also on my work on Bagh echinoids which was soon to appear in the *Proceedings of the Indian Academy of Sciences*.

Thence onwards this first contact between us developed soon into a close life long friendship, and now we are separated by his sad demise. We had during this long period of our friendship occasions to meet often in Calcutta and elsewhere, and discuss various aspects of Palaeontology and Stratigraphy of Indian rock formations. Naturally enough these discussions were more often about the Bagh Beds and their stratigraphic and faunal relationship with the Upper Cretaceous deposits of South India, which have been all these long years the main area of my interest.

For the purpose of my today's lecture I would confine my attention only to the major aspects of stratigraphy and related problems of these deposits without paying much attention to their lithological characters and the voluminous heap of palaeontological details collected by me and my colleagues on which our conclusions regarding them are based.

The stratal units (Chiplonkar & Badve, 1972; Chip-lonkar, Badve & Ghare 1977) namely the *Trace Fossil Horizon*, the *Oyster Bed* and the *Jhabotrigonia* (originally reported as *Astarte*) *Turritella Bed* coming above it, which are developed in the upper part of the Nimar Sandstone, along with its increasing calcareous nature, are found to occur over much of the extent of the Bagh Beds. They are thus positive evidence of marine deposition of the Nimar Sandstone, only the earlier part of it being fresh water (fluviatile).

At Bagh we (Chiplonkar and Badve 1976; Chip-lonkar, Ghare and Badve 1977) have collected a few small oyster shells at a horizon some feet below even the *Trace Fossil Horizon*, and we hope to be able to trace their occurrence over a wider extent. This should indicate that ingress of marine waters into the Narmada valley had taken place even prior to deposition of the *Trace Fossil Horizon*, and thus prove that the Nimar Sandstone, excepting its very lower portion, was deposited under marine conditions.

In 1972 and again in 1974, while discussing the results of work by my research students, Professor Sahni was curiously enough inclined to know if the plant bearing bed reported by Murty *et al.* (1963) from Umralli of Nimar Sandstone and support Medlicott (1875) in taking the Nimar Sandstone, at least the lower part of it, as representing or belonging to the Mahadevas of the Upper Gondwanas, and that too when even Bose (1884) had already separated it stratigraphically from the Mahadevas, and assigned to it Lower Cretaceous age.

It should be mentioned here that as the latest findings by my colleagues Badve and Nayak (1982), the Umralli Plant Bed of Murty *et al.* (1963) has unconformable relation towards the Nimar Sandstone as shown by presence of grits and conglomerate, and it has to be allied with the Than Beds of Kathiawar.

The earlier workers like Bose (1884), Mukherjee (1938) and very lately workers like Poddar (1964), Murty *et al.* (1963) and others have without bestowing any critical thought on it, have taken the entire Nimar Sandstone (except a thin calcareous layer at its top near Amlipura) as of fresh water origin and separated it from the succeeding limestone marl sequence of the Bagh Beds, as a distinct unit laid under fresh water conditions, and assigned it a Lower Cretaceous age.

I tried to impress upon him that repeated visits to the exposures of the Bagh Beds through out their extent in the Narmada basin, have yielded us abundant field and fossil evidence to show continuous depositional contact of the Nimar Sandstone with the overlying limestone sequence and thus we can not but take it as an integral part of the Bagh Beds.

In view of the wide occurrence of these above mentioned horizons in the Nimar Sandstone as also the two *Inoceramus Beds* and the two *Coralline Limestone Beds* coming higher up in the sequence Professor Sahni was inclined to call them zones and treat them as biozones against the background of the efforts of Pal (1970, 1971) towards biozonation of the Bagh Beds. But considering the quick lateral and even vertical passage into the associated sediments as stressed by us on the basis of our extensive field observations and contents, he was reconciled to our attitude to take them just as horizons with concentrations of certain species in response to suitable environmental conditions, and thus treat them as indicative of facies aspects rather than biozones with a sense of time aspect.

During our discussion Professor Sahni used to be usually inclined to go by the views of Bose (1884), Mukherjee (1938) and others, and consider Bagh Beds to correspond and be correlatable with the different members of the Upper Cretaceous formations of South India and to span over much of the Upper Cretaceous period. But voluminous evidence which we had collected by a critical

and systematic study of more than two hundred species which we on many occasions had brought to his notice, and that some of the species including some of ammonoids with younger and older affinities occurring together at different horizons within the series without any regular sequential relations he was ultimately brought round to accept our interpretation that Bagh Beds spanned over a considerably shorter period of time (Albian to Turonian) as compared to the South Indian deposits and also agreed that the Bagh Beds constituted a single palaeontological unit. He also noted with a sense of appreciation that with repeated occurrence of *Inoceramid* and *oyster* beds to which evidence from the distribution of trace fossils has been lately added (Badve and Ghare 1978), we have been able to demonstrate to him that Narmada valley was visited during the Bagh period by four marine transgressions and regressions.

Apart from the voluminous evidence as collected by us on the basis of more than 200 species, at species level which we had placed before Professor Sahni, he was amazed to see the strong faunal contrast to be found so obviously between Trichinopoly and Narmada basins in respect of faunal elements even above species level when it was pointed out (Ghare and Badve 1978) to him that even the gross faunal elements such as Nautiloids, the ammonoids like Desmoceratids, Acanthoceratids, Puzosids, Phylloceratids, *Turrilites*, *Hamites*, *Baculites* and *Anisoceras*, the bivalves Pynchodontes and Rudistes, the Terebratulids are totally unrepresented in the Bagh Beds; while these elements are varieties and numerically abundant in the Upper Cretaceous of South India; further, Gastropods and Echinoids also are far more varied and abundant in the South Indian deposits. But Placenticeratidae and Coilopoceratidae are much better represented in the Bagh Beds than in the South Indian Cretaceous. And then the percentage of European element was much higher in the Bagh Beds.

With these facts placed before him and all this without my soliciting any comments from him, Professor Sahni remarked that with so voluminous evidence which has now been put forth by you and your colleagues, he was unable to understand why these workers like Verma (1968, 1969), Sastry and Mamgain (1971), B. R. J. Rao, and others, choose still to cling to the idea of correlatability and direct faunal connection between these two basins of sedimentation. He agreed that according to what we had already pointed out (Chiplonkar and Badve, 1968), whatever species are said to be common to the two basins are too few in the whole fauna as such and must have travelled along an indirect route, most probably via the Mozambique Channel.

And with very expressive eyes, which some of us to be sure must have had occasions to note, he said well the discussions we have been having on these problems all

these days, I must say, have proved fruitful.

At this stage I asked him if he recollected Wadia's views which he had cited in his Presidential Address (Shani, 1941). He was amusingly surprised when I read it out to him from Wadia's 1926 edition of Geology of India—there Wadia observes that they differ

“As widely as it is possible for two formations of the same age to differ.”

and that this contrast

“Denotes a complete isolation of the two seas in which they were deposited by an intervening land barrier of great width, which prevented the inter-sea migrations of the animals inhabiting the two seas”.

and then I also showed him from his Presidential Address that even after referring to this observation by Wadia he had continued to cite views (by implication) of Bose (1884) Mukherjee (1938) followed by a plea to rethinking on the palaeogeography of the Cretaceous period. And to this he replied smilingly “so now we have the results of our rethinking on this problem”.

III. DISCOVERY OF GRAPTOLITES IN HIMALAYAN REGION

It was late in 1963 or may be 1964, I recollect in particular the occasion when I was talking to Professor Sahni over his Presidential Address on “Palaeogeographic Resolutions in the Indo-Burmese Region : Vindhyan to Devonian” to the Geology section of the Indian Science Congress of 1941. There he had suggested rejection of the interpretation given by Cowper Reed (1912) and La Touche (1913) that India and Burma were separated during Lower Palaeozoic times by a barrier. On the ground that the fossil material at their disposal was very scanty and inadequate for interpretation of faunal affinities towards America and Europe; also that such valuable evidence as presence of graptolites was not then recorded in Kashmir or the Himalayan region partly because of the highly susceptible nature of graptolite bearing shales to weathering and damage and to be lost in tectonically disturbed region like the Himalaya. He, however, had considered other evidence in favour of untenability of such a barrier, pleaded for a detailed survey of the Kashmir and Himalayan region and a careful search for more fossils, and had almost prophetically expressed a hope that graptolites would be found occurring there. Ultimately with a careful collection *Didymograptus* and other fragmentary graptolites were found occurring in the Ordovician rocks near Trehgan in Handwara Kashmir (Wadia 1976). Thus Professor Sahni's expectations were fully vindicated, and contrary to the interpretation given by Cowper Reed (1912) and (La Touche) (1913) a marine connection between India and Burma was proved to have existed in Lower Palaeozoic times.

I recollect this occasion well, and see before me even

today the firm and triumphant way he looked at me when we referred to the occurrence of Graptolites in Kashmir Himalayan region as reported by his colleague in 1963. He asked me if I recollected the discussion we had at Benaras on this point after he had delivered his Presidential Address. So his prediction had come true—Graptolites were found in Kashmir Himalayan region and existence of marine communication between Burma and these regions was proved.

To this I responded only by silence. I remember well that I had narrated these incidents to my post-graduate students at Benaras and Poona while referring to occurrence of Graptolites in India.

IV. THE VINDHYAN SEA—ONE OR TWO

It was in 1974 at the time of his visit to Poona; seeing in my hand a copy of his 1941 Presidential Address to the Science Congress he asked me about my views about his two geosynclines (Sahni 1941 and 1962 a, b). North Vindhyan Sea and South Vindhyan Sea, in which Vindhyan sedimentation had taken place, as against the idea of one Vindhyan sea visualised by Krishnan and Swaminath (1959) in which the Vindhyan rocks of the Central Indian region as well as their analogues like Lower Haimanthas, Jaunsars, etc. in the Lesser Himalaya were deposited. Our discussion this time was rather brief. I asked him whether his North Vindhyan sea extended westwards into the Karagwe region in Central Africa where Ahmed (1962) had found rocks comparable to the Vindhyan rocks of Central Indian region, or if it extended north-westwards to join the Tethys, because Professor Sahni had referred to Vindhyan-like rocks to occur in the Salt Range and further up in Afghanistan where he considered the group of Kalu rocks and still further the Hormuz series of Persia comparable as being to Central Indian Vindhyan. To this he very humorously replied by saying "Oh!" for that I would have to travel that side again and verify the situation.

Here, when I drew his attention to the palaeo-current studies lately carried out by several workers, as for example, by Jafar, Akhtar and Srivastava (1966) and others on the western Vindhyan rocks as exposed in Agra-Fatehpur Sikri area and on the eastern exposures such as in Mirzapur district, the indications were that the northern coast of the Central Indian Vindhyan sea was not probably near about along the present southern margin of the Gangetic plains, but that his Southern Vindhyan sea in which they were laid down deepened northwards under the Gangetic alluvium as suggested by Krishnan and Swaminath (1959) and Holland (1908) and that at some time there was a vast sea which covered the Central Indian tract as well as the Lesser Himalayan region where Vindhyan-like rocks are considered to occur. Thus at least at some stage there was probably only one vast Vin-

dhyan sea instead of two seas as envisaged by him. May be that his two seas at some stage merged together. Of course here I agreed that this would involve the question as to when this merger took place, if at all it took place. And Professor Sahni with a visibly serious face agreed that it was a much involved problem and needed more extensive work. Such a merger may mean that the Vindhyan of Central India may have been more or less contemporaneous with their analogues in the Lesser Himalayan region at some stage when there were two seas and later on with their merger the relative ages of some of the members in both these regions would be a very complex and involved problem; because they are so much separated apart from one another, and also because fossil evidence at least at present is very scanty and poor in their reliability in respect of age considerations.

V. CLASSIFICATION OF THE VINDHYANS AND THE GONDWANAS

Classification of the Vindhyan and the Gondwanas have also been subjects of our discussion; on these two subjects we could not be in agreement with one other, and chose to be tolerant of our different views.

To be brief for the purpose of the present lecture, Professor Sahni defended Auden's (1933) four fold division of the Vindhyan directly into (1) the Semri Series (2) the Kaimur Series, (3) the Rewa Series and (4) the Bhandar Series by totally rejecting the terms Lower and Upper Vindhyan, because this gave what they call a balanced division in terms of the thickness of these units, but at the same time being not unmindful that the Semri Series is predominantly a set of limestones and calcareous shales, while members of the Upper Vindhyan are very low dipping to almost horizontal, predominantly arenaceous with limestones and calcareous shales as only a subordinate feature. They considered that the Lower Vindhyan (i. e. the Semri Series) alone was in its thickness equal to each of the constituents of the Upper Vindhyan and being thus too small for the total thickness of the Upper Vindhyan it was considered an unbalanced grouping. Yet Professor Sahni like Auden admitted that this lithological set of characters was indicative of different environment.

The arenaceous nature, abundant ripple marking and wide spread red colour of all the members of the Upper Vindhyan are so characteristic of shallow littoral to intertidal and arid or sub-arid circumstances which prevailed during the deposition of these Upper Vindhyan, while, the limestones and calcareous shales as the most predominant aspect of the Semris gives them a distinct aspect as a group. Therefore, to give thickness so heavy a weightage amounts to totally ignoring the natural factors which constituted the entire environmental regime under which the Semris were deposited. It amo-

units to going by a physical factor and rejecting the very laws of nature which governed the sedimentation of Semri deposits.

Lately some micro-floral remains have been reported from some of the shales; but their determinative value is yet not sufficiently assessed. They show that organic life did exist in that far off time. But that is all about them at any rate for the present.

It may be mentioned here that a two-fold classification was proposed by Vredenburg back in 1906, with a different grouping of the members and some what different nomenclature; he also had claimed it to be a balanced classification in respect of thickness of the different members. But it received little support from any quarters.

I can understand, at least as a matter of convenience, dropping the term Semri Series (as an alternative to Lower Vindhyan). But I can not in preference to the mere physical factor of thickness ignore the natural factors which constituted the environment under which the Vindhyan rocks were deposited. I do maintain that the two fold division of the Vindhyan into Lower and Upper Vindhyan as in vogue all these years is the rational approach to the problem, and it is in keeping with the laws of nature which were operative during the Vindhyan sedimentation.

GONDWANAS

Gondwanas have been for long a subject of discussion for the age and affinities of its fossil floral wealth as well as classification of the Gondwana System. Feistmantel (1876a, b, 1880, 1882) the pioneer worker on Gondwana flora divided these strata into two groups, the Lower Gondwana and the Upper Gondwana. He, however, while giving this two fold division, had mentioned presence of *transitional* flora which revealed a mixture of affinities, with the Lower Gondwana *Glossopteris* flora gradually giving place to the Upper Gondwana *Ptilophyllum* flora. This view is held for all these years. As I gather, the material collected by Hughes from the South Rewa basin, had inadvertently got mixed up while being unpacked in Calcutta; and thus some of the fossil material collected from different beds had got mixed up; and this gave rise to the so-called *transitional* flora with mixed affinities.

But interestingly enough and I must submit it, with appreciation, that following Vredenburg (1910), Wadia (1926 and later on even 1976) in his Geology of India, gave us Middle Gondwana to consist of Maleri Series (to include Parsora, Tiaki and Denwa), Mahadeva (or Pachmarhi) Series and Panchet Series. He very appreciatively recognised that these beds are on the whole poor in floral content but have vertebrate fossils of important nature and that these rocks are lithologically characteristic of arid environment. Thus Wadia took a stand which Feistmantel, for reasons best known to him, could not do.

These points were overlooked or their importance was minimised in preference to the so called mixed flora.

But later work on the fossil flora of the South Rewa basin by Shiva Dayal Saxena (1952) made his teacher, Professor Birbal Sahni to think of the possibility of the flora of the deposits in this region having its own distinctness and thus the possibility of having a Middle Gondwana flora. To this lately Lele (1964) from the Birbal Sahni Institute of Palaeobotany, Lucknow, added more evidence and to my desire to know from Professor M. R. Sahni his attitude about that so-called mixed or *transitional* flora we now know as the *Dicroidium* flora to characterise the Middle Gondwanas, he did not appear much inclined to express his views.

Incidentally I may refer here to Dr. M. N. Bose's idea of discarding the terms Lower, Middle and Upper Gondwanas and taking them as a series of seven or eight series with floral characteristics of each of them such as Talchir, Karharbari, Barakar, etc. etc. Here I may say that I had drawn the attention of Dr. M. N. Bose (some time around 1978) that he may have some more details at his disposal as a result of recent work. But Feistmantel (1876a, b also in Fox 1931) had expressed this attitude of six or seven floral units as recognisable and also expressed that on a broader basis they could be grouped with two or Lower and Upper Gondwana floras (with a transitional group in between). It is true that Dr. M. N. Bose has in post-Feistmantel days collected more details with also perhaps some more precision and he thus is only now supporting what Feistmantel had said some ninety years back. Professor Sahni did not appear much inclined to say anything on this point; may be he was not perhaps prepared to face such a question.

So, Ladies and Gentlemen I have tried to recollect some of the discussions of academic problems which I had the opportunities to have with my late lamented friend Professor Mulk Raj Sahni, and in that form I pay my homage to him.

I thank you all for so kindly and patiently hearing to what I was narrating.

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