

## Geoscientific Investigations of the Seabed for Paleoclimatic Inferences : Thrust Area for Priority Research in the Nineties

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Whenever time comes to decide thrust areas in Earth Sciences our attention get focused to important but traditional geological fields like Archean granulite, Proterozoic sedimentary basins, Deccan volcanic provinces etc. What about Marine Geology, which is rather young branch of Earth Sciences. Oceans are known as store houses of food, minerals including oil and gas, energy etc. They also possess uninterrupted record of climatic changes. Therefore, marine geosciences need attention and patronage.

Indian Ocean (Area :  $74.917 \times 10^6 \text{ km}^2$ , volume  $291.945 \times 10^6 \text{ km}^3$  Average depth : 3897 m) along with its two arms i.e. the Arabian Sea (Area  $7.456 \times 10^6 \text{ km}^2$ ) and Bay of Bengal (Area :  $2.1712 \times 10^6 \text{ km}^2$ ), is 3rd largest of the world oceans. This region, till the sixties was one of the least studied of the world oceans. The Indian ocean is almost entirely bordered by the developing countries and to this date the work on Marine Geosciences has been largely carried out by the developed countries from outside the region. However, the International Indian Ocean Expedition in the sixties, along with the further work by the National Institute of Oceanography considerably enhanced our knowledge of the Ocean, including its mineral resources.

India has a coastline of about 6,000 kilometres. Recently declared 200 nautical miles limit for EEZ (Exclusive Economic Zone) has provided an area of about 2 million sq. kilometres for exclusive exploitation to India. It means no regular activity can take place by any other country in this area except using it as innocent passage for trade and non military purpose.

This exclusive right offers unique opportunity along with a challenge to Indian geoscientists to explore this area as early as possible. The task includes the search, development and utilization of mineral resources and also the study of seabed sediments for paleoclimatic inferences which in turn will be useful for understanding monsoonal variations. These studies have a vast potential for improving the living standard of our people and economic growth of our country.

In view of the limited manpower, especially expertise, equipment and research vessels, geoscientific studies in the region may have to be phased. The phases could be exploration of the inner-shelf, outer-shelf, & the slope and the deep sea. Otherwise, phases could be subject-wise like mineral resources, sedimentological, geochemical, micropaleontological etc. The first phasing which is depth-wise is more practical. Due to Geopolitical reasons and economic potential, deep sea regions of the Indian Ocean has already been studied separately under the 'polymetallic nodules project'.

Now, a beginning could be made for an integrated study of the inner shelf to understand paleoclimate and peleo sea-level changes. These two applied aspects require an understanding of all the branches of Earth Sciences like Geophysics, Geochemistry, Micropaleontology, Sedimentology etc.

By now, it is widely accepted that sea level will go up in near future as a result of thermal expansion of sea water and melting of ice of polar region due to 'greenhouse effect'. To assess the net impact of greenhouse effect we require records of paleo sea-level changes for the last few thousand years and on much finer resolution of time. For this, we have to examine the sediments from the shallow seas where rates of sedimentation are high.

Study of paleoclimates, particularly the paleomonsoons is very important as it helps in predicting future behaviour of climate. Geological data from shallow near shore areas where sedimentation rates are high, gives a higher time resolution for a period covering the last few thousand years.

The above two aspects can be covered by studying series of core samples from the shallow water all along the Indian coast. Initially, a conceptual frame work/working hypothesis should be formulated and tested in some model areas. Concept proved worthwhile will be applied in other areas too. The results thus arrived at will be very useful to planners as it can project (i) how much sea level will fluctuate in near future and what will be the extent of inundation of coastal plains (ii) how monsoonal precipitation will vary in coming years, at least on decadal basis, in each sectors along the coast.

One such pilot study has been undertaken at N.I.O. Based on foraminiferal variations in a short core off Karwar at a depth of 23 m, it is inferred that monsoonal precipitation had varied in the past and there is a cyclicity of about 77 years in concentration of drought years. The inference is supported by average rainfall data of last 100 years which show the trace of this 77 years cycle. This cycle is very close to the Gleissberg cycle of the sun.

These observations can be further supported by other studies of Earth Sciences like oxygen isotope studies. Such facilities are limited to few places in our country. Similarly, a need for  $C^{14}$  dating is felt when a long core has been analysed to enhance the length of data. Is it possible to create few centre where such data can

be obtained on priority basis on payment?

It may be emphasised that other than the above two topics, geoscientific investigations of the shallow region holds the key to many other problems like development of port and harbours, exploitation of placer deposits, monitoring of marine pollution, monitoring the seabed for stabilities of offshore structures like pipeline for oil and gas and so on. Therefore, it is suggested that geoscientific investigation of the seabed specially with reference to paleoclimates may be selected as a thrust area of Earth Science for priority research in the nineties.

### COMMENTS ON THE PROPOSAL BY ASHOK SAHNI

The proposal presented by Rajiv Nigam for determining the cyclicity of past drought and palaeomonsoonal cycles is well intentioned, basically sound in its avowed objectives and topical for a country like ours. However, good intentions are not necessary good enough for scientific research and there are many pitfalls along the chartered route to success. Nigam, in a pilot study off the Karwar coast, has tried to demonstrate the inverse correlation between percentage abundance of *Clavortalia annectens*, a common enough benthic foraminifer in core samples in relation to freshwater discharge through rivers and streams. Conversely, lower frequency of the species indicates higher discharge, presumably because of monsoonal influx. He deduces that there is a cyclicity of about 77 years in the monsoon pattern over a span of about 450 years. Nigam in search for a cause for such a periodicity, ascribes it to the Gleissbag Cycle of the sun which is approximately of the same duration.

I have the following comments on the scheme with a view to restructuring investigations on the two major components of the proposal.

- 1) The palaeobiological bias
- 2) The sedimentological bias.

Percentage abundance of *Clavortalia annectens* may not be monocausal phenomenon related only to drought periods. Several questions on the lines listed below have to be answered. Do we really know the life cycle of *C. annectens*? Could biological factors promote the periodic annual increase of populations? Is there any relationship to upwelling and corresponding "blooms" noticed in marine organisms? What factors control competitive effectiveness of *C. annectens* in comparison to associated shelf benthic communities?

Sedimentological studies in a way, are more basic to Nigam's proposal as they can provide a biased data set for the palaeobiological inputs. Nigam has stressed the efficacy of studying shelf sedimentation, as according to him, these are regions of high sedimentation and should provide more accurate records of annual layering in a ky scale. Several aspects concerning this premise come to mind.

a) High sedimentation rates do not necessarily indicate a continuous record of deposition. High erosion rates are also known for such regions. In fact monsoon climate itself controls near shore sedimentation both directly and indirectly (Sahni, 1982). Duing (1970) has reported monsoon generated wave action that can disturb sediments upto a depth of 400 m off the western coast of India. After monsoon storms, currents thus formed can move river-derived material to greater depths and can cause mixing of deposits higher in nutrient content. Schott et al. (1970, p. 175) suggest that monsoon activity is represented by rhythmic laminations along the upper continental slope while according to Siebold (1972), actual layering has not been observed in the shelves. Annual repopulation of the foraminifer *Ammonia beccarii* has been demonstrated near Patcham Island, Kutch, as a consequence of monsoonal flooding (Glennie and Evans 1976). This case study needs to be further examined in the context of the current proposal.

The above observations are not meant to detract from the intrinsic merit of the proposal. On the other hand, they are designed to strengthen the project to meet its objectives.

1. Reliance should not be placed only on a single species, but efforts should be made to recognize an indexing assemblage. In the same manner as the marine benthic foraminifers have been used to record draught cycles, corresponding freshwater taxa in the same core samples should be studied to document increased river discharge.
2. The pilot study should be extended to river systems which are well characterized in terms of fluvial dynamics, run off, ponding and vegetational cover parameters.
3. Stations measuring cyclical phenomenon of the above nature should be established for continuous monitoring.
4. This proposal must be undertaken in collaboration with a specialist sedimentologist and with the full support of an isotope application laboratory for dating the critical levels.

If the final proposal considers some of the points outlined, it may well achieve a breakthrough in palaeomonsoonal analysis.

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