it is well known that scientists in the West rarely cite the work done in India. What's more upsetting, several Indian scientists also seem to follow them for several reasons, not entirely scientific. This factor naturally distorts the citation index. However, despite the weaknesses in the three indicators, overall they give a fairly good idea about the quality of a work which, in turn, reflects a scientist's worth.

In general, these criteria have served the managers quite well in assessments of scientists and their work. However, the current ISI analysis has upset many well-established notions. The analysis was requisitioned to give a new dimension to AIDS debate, specifically the question of continuing the financing of research in this disease. In the last 10 years, billions of dollars have been spent to find a cure for AIDS. With no cure in sight, questions were being asked about the quality of AIDS research and the scientists were even accused of not being critical enough to stop the 'rubbish' being churned out. The acrimonious nature of the debate prompted a closer look into the publication record of some prominent scientists working in the area. And hence the ISI's involvement.

An aggregate of 36,000 papers on AIDS published in major journals between 1981 and 1992 were analysed. These were individually sorted out for their detailed citation-record. Contributions of different countries, laboratories and scientists were also analysed. In 1981–86, 15.37 and 14.59 per cent of AIDS and general biology papers (taken as base-line data) respectively, did not receive a single citation; about 52% of papers in AIDS (and in biology) received just 4 or less citations; 38% received 5 to 49 citations; only 10% received 50 or more citations. The situation improved marginally during 1987–92 for AIDS, though not for publications in biology research. Apparently, a large chunk of papers are going uncited, implying that just too many trivial and inconsequential papers are being published.

Another point which emerged is that contributions from small countries like Belgium, Sweden, the Netherlands obtained greater number of citations on the average than major countries like the USA and the UK. For instance, out of 18,495 publications, 15.24 citations/paper were received for USA in contrast to 15.70 citations/paper for 573 papers from Belgium during the same period. This implies that science in small set-ups is closely integrated, with work in well-defined problems. Generally this is a wise strategy to follow, especially when funds are scarce.

Lastly, when ISI looked at citations of an institution or of an individual, one aspect which seemed to dominate was that the overall quantity was in no way a measure of its impact or quality. Apparently, largely scientists are 'forced' to follow the idea of 'publish or perish'.

Implications of the study

The above discussion raises some important questions. Is the large volume of scientific literature really worthless? How is it that this research is going unnoticed for such long time periods, and still getting financed? One would have expected that at some point, critical reviewing would have cried a halt to further work in these programmes.

There are no easy solutions to these problems. Stricter vigil and reviewing processes (and implementing the reviewer's comments), and a more open atmosphere can certainly improve the situation. Above all, a group leader could set a precedent by publishing only such work which is significant and impress on his/her team the need to carry out and publish only well rounded and path breaking conceptual ideas. In this discussion, one point which needs to be remembered and which is also highlighted by the analysts is that a scientist's contributions need not be measured only in terms of his scientific work and publications; invaluable and not-too visible contributions may also be there in the nature of teaching, or training students and staff, or building a group. Many of such scientists are known to publish their work only after many years and these publications are known to be important. It is hoped that this discussion will initiate a debate in India, too, which, in turn, can lead to phasing out many redundant projects and encourage good work.

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SCIENTIFIC CORRESPONDENCE

Enigma of the negative $\delta^{18}O$ pulse at LGM

With reference to S. K. Gupta and P. Sharma's paper 'Enigma of the negative $\delta^{18}O$ pulse at the last glacial maximum in the Arabian Sea' (*Curr. Sci.*, 1993, 64, 107–110): Downcore records[1] of $\delta^{18}O$ in the shells of planktonic foraminifera in a sediment core collected from the southeastern Arabian Sea (SK 20–185; lat. 10° N, long. 71° 50′ E; water depth 2523 m) show negative excursions of up to 1‰ during or close to the last glacial maximum (LGM). The cause of the observed excursion that lasted for approximately 4,000 years has been the subject of some debate. Sarkar *et al.*[1] proposed that it might arise from an intensification of the northeast (NE) monsoon during the LGM: this was assumed to have led to an enhanced transport of fresher waters from the Bay of Bengal to the eastern Arabian Sea. However, Krishnamurthy[2] suggested that the observed excursion could be caused by local changes in the seasonal temperature distribution brought about by a weak southwest (SW) monsoon during the LGM. Now another explanation for the shift in $\delta^{18}O$ has been offered by Gupta and Sharma[3]. They suggest that the lighter oxygen in foraminiferal tests might be the consequence of an increased meltwater discharge from the Himalayas during ~ 22–18 kyr BP. These authors cite a variety of palaeoclimatic evidence for a synchronous warmer phase in the Indo-Tibetan region in support of their hypothesis.

However, like the first explanation of the results[1], the most recent re-interpretation[3] is also based on the premise that a *more intense NE monsoon would lead to* a stronger northeast monsoon coastal current (NEMCC) that presently carries a large volume of warmer and fresher waters of Bay of Bengal origin to the southeastern Arabian Sea during November–January[4, 5]. This simplistic presumption is supported neither by observations nor by models.

The coastal circulation off the west coast of India during the NE monsoon is anomalous in that the NEMCC flows against the wind: the winds during this season (and indeed throughout the year) are generally equatorward while the current flows poleward[5]. The only other eastern boundary current known to oppose the prevailing local winds is the Leeuwin Current off western Australia. The strong influence of winds over these currents is apparent from observations[5] as well as models[6]: a stronger wind may retard the opposing flow, and may even change its direction. Using the climatological averages off western India, an evaluation of contributions of the longshore pressure gradient and wind to the near-surface momentum balance[5] indicates that the effect of the longshore pressure gradient may exceed that of the opposing wind during the NE monsoon, but not during the SW monsoon season. What generates the longshore pressure gradient is not entirely clear, but the longshore density gradient is regarded as the most likely forcing mechanism[5–7]. As the density gradient is mostly due to the occurrence of high-salinity waters in the northeastern Arabian Sea and low-salinity waters carried westward by the NE monsoon current south of Sri Lanka, the NEMCC may be considered to support itself.

Therefore, temporal changes in intensity of the NEMCC cannot be linked simply to variability of the NE monsoon. Instead, these will be determined by relative changes in the two opposing factors: longshore winds and thermohaline gradients. Some insights into the glacial–interglacial changes in these factors could be gained from an examination of the sedimentary record.

It is generally agreed that an intensification of the NE monsoon probably led to stronger equatorward winds at the LGM (e.g. ref. 8). Judging from the planktonic $\delta^{18}O$ distribution during the LGM (figure 3 in ref. 9), however, it would appear that the longshore density gradient off the west coast of India at that time might not have been very different from that observed today. Thus, the stronger winds in *conjunction with similar longshore density* gradient actually could have led to a *weakening*, if not a reversal, of the NEMCC off western India. Significantly, this is well reflected in the spatial distribution of $\delta^{18}O$ in planktonic foraminifera investigated through the analysis of numerous sediment cores[9]. The Holocene (core-top) distribution closely mimics salinity distribution in that tongues of isotopically lighter oxygen extend northward along the Indian coast. In sharp contrast to this, the distribution during the LGM was quite uniform zonally. Duplessy[9] ascribed it to the weakening of the SW monsoon and the associated decrease in freshwater discharge from the Indian subcontinent. However, it may be noted that the occurrence of lower salinities off the Indian coast is only in part due to the freshwater runoff and an excess of precipitation over evaporation; advection of the low salinity waters (about $7\times10^6 m^3 s^{-1}$ – ref. 5) by the NEMCC is perhaps a more important mechanism as evident from the salinity distribution during the two monsoons[10]. Therefore, the absence of a pronounced east-west gradient in $\delta^{18}O$ seems to provide firm evidence for the absence of the NEMCC during the LGM.

Perhaps the greatest drawback with the hypotheses that invoke an import of the isotopic signal from the Bay of Bengal is that, as noted previously[3], a similar negative spike in $\delta^{18}O$ has not been observed in the Bay of Bengal where the signal should be the strongest. It is not as if there are no isotopic records of planktonic foraminifera with adequate vertical resolution available from this region. One such record from ref. 10 is reproduced in Figure 1. It may be argued that the sampling interval (10 cm) in MD 77181 is larger than in SK 20–185 (2 cm). However, this may be expected to be compensated by a higher sedimentation rate (by a factor of three). Given the duration of the excursion in SK 20–185, the signal should have been observed in MD 77181 as well. Its absence strongly argues against the Bay of Bengal origin of the spike. On the other hand, a similar isotopic shift has also been observed in another core

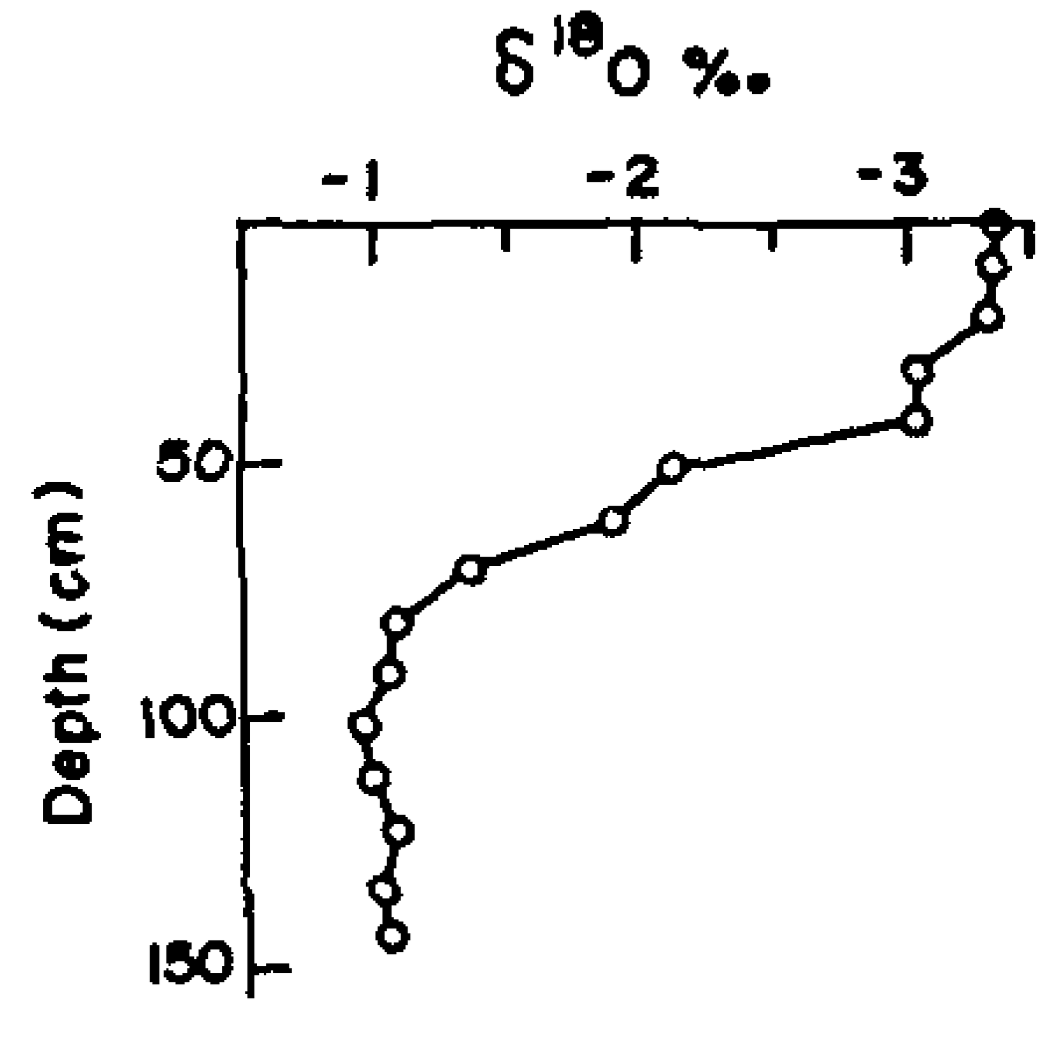

Figure 1. Downcore changes in $\delta^{18}O$ (‰ versus PDB) in *G. ruber* in the core MD 77181 (17° 24′ N 90° 29′ E; water depth 2,271 m (modified from ref. 10).

raised from the upwelling zone off Oman (CD 17–30; lat. 19°56′ N, long. 60°39′ E; water depth 3850 m). This has been attributed to a weakening of the upwelling regime and the associated increase in the sea surface temperature during the LGM (ref. 11). A smaller magnitude of the $\delta^{18}O$ excursion (maximum 0.73‰) in CD 17–30 has been taken to preclude a similar formative mechanism for the feature seen in SK 20–185 (ref. 3). However, it must be noted that the somewhat coarser vertical resolution may in part account for the observed differences in the amplitude of the $\delta^{18}O$ spikes in the two cores. In view of the inferred absence of the NEMCC during the LGM and the absence of a spike in the Bay of Bengal we conclude that the isotopic signal could not have been imported from the Bay. Instead, the similarity of the CD 17–30 and SK 20–185 records seem to support the view that the $\delta^{18}O$ shift at the LGM more likely reflects local thermohaline changes in the surface waters[2].‰

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S. K. Gupta and P. Sharma reply:

We thank Naqvi for commenting on our paper[1] and raising some basic issues on the enigmatic $\delta^{18}O$ pulse observed at LGM in a core from the Arabian Sea[2]. The thrust of his comments concerns lack of evidence for our postulated meltwater spike in the Bay of Bengal around LGM. He also questions the intensification of North East Monsoon Coastal Circulation (NEMCC) at LGM that is required to transport low salinity Bay of Bengal water to the core locations in the east Arabian Sea.

As already noted[1], there is no direct observational evidence as yet for the meltwater spike in the Bay of Bengal. However, evidence for a warming event during the 22–18 kyr period through several independent proxy climate indicators from widely separated areas in the Indo-Tibetan region was presented. We had suggested[1] that this event could have provided the meltwater needed to explain the observed isotopic data[2]. Based on the data from the available cores from the Bay of Bengal, including that on the core MD 77181 (ref. 3), it is not possible to discount the existence of the postulated meltwater spike in the Bay of Bengal. We are essentially looking for a spike of 1–2 kyr duration in the time interval 22–16 kyr BP. For this, we require a sampling resolution ~ 1 kyr. The isotopic data of the core MD 77181 as presented in Figure 1 of Naqvi's comments cannot be used to infer that the required meltwater spike did not exist. Firstly, no radiocarbon dates are available and, therefore, the chronology is to that extent uncertain. Secondly, even taking into account that the sedimentation rate may be three times higher than in SK–20–185 (ref. 2) the time resolution corresponding to 10 cm sampling interval of MD 77181 is more than 1.7 times that in SK–20–185. Thirdly, the isotopic data of MD 77181 are for the foraminifera *G. ruber*, which, because of its depth habitat, gives a much subdued signal compared to *G. sacculifer* as seen in Figure 1. This figure replaces Figure 1 of ref. 1 in which, by oversight, some of the ^{14}C ages were marked incorrectly. This correction does not, however, affect our[1] arguments and conclusions.

We, therefore, see that it is not unreasonable to speculate the existence of a meltwater spike in the Bay of Bengal, particularly in view of (i) available palaeoclimatological evidence for a warming event in the Indo-Tibetan region, and (ii) absence of high resolution studies on cores from Bay of Bengal. In our opinion the question of existence or otherwise of the meltwater of Indo-Tibetan origin at LGM can be resolved by a high resolution investigation of sediment cores from suitably selected locations in the Bay of Bengal as already mentioned[1]. The question of NEMCC at LGM can also be resolved from a similar study off the southern coast of Sri Lanka.

Without going into the details, we had assumed[1] that the mechanism for transport of Bay of Bengal water to the east Arabian Sea through NEMCC invoked by Sarkar *et al*[2] could account for the transport of the meltwater spike proposed by us. We note that the arguments presented by Naqvi against strengthening of the NEMCC during LGM, based on coarse resolution data of Duplessy[3], are generally valid. From these data one cannot, however, rule out the possibility of a short (1–2 kyr) duration climatic event at LGM to account for the observed $\delta^{18}O$ spike. In particular, during this period, even a weakened NEMCC could have transported significant quantity of the low salinity light water of the Bay of Bengal originating from the enhanced freshwater input of (i) either increased run off from south Indian rivers or, (ii) the increased meltwater from the Indo-Tibetan region. In the absence of high resolution (~ 1 kyr) analysis, the reported isotopic data of ocean sediment cores from Arabian Sea[3] could have possibly missed to see this evidence.

We had also noted[1] that the $\delta^{18}O$ spike in SK–20–185 could not be wholly accounted for by the mechanism proposed by Krishnamurthy[4] in view of (i) the observational data on the seasonal distribution of the foraminifera *G. sacculifer*

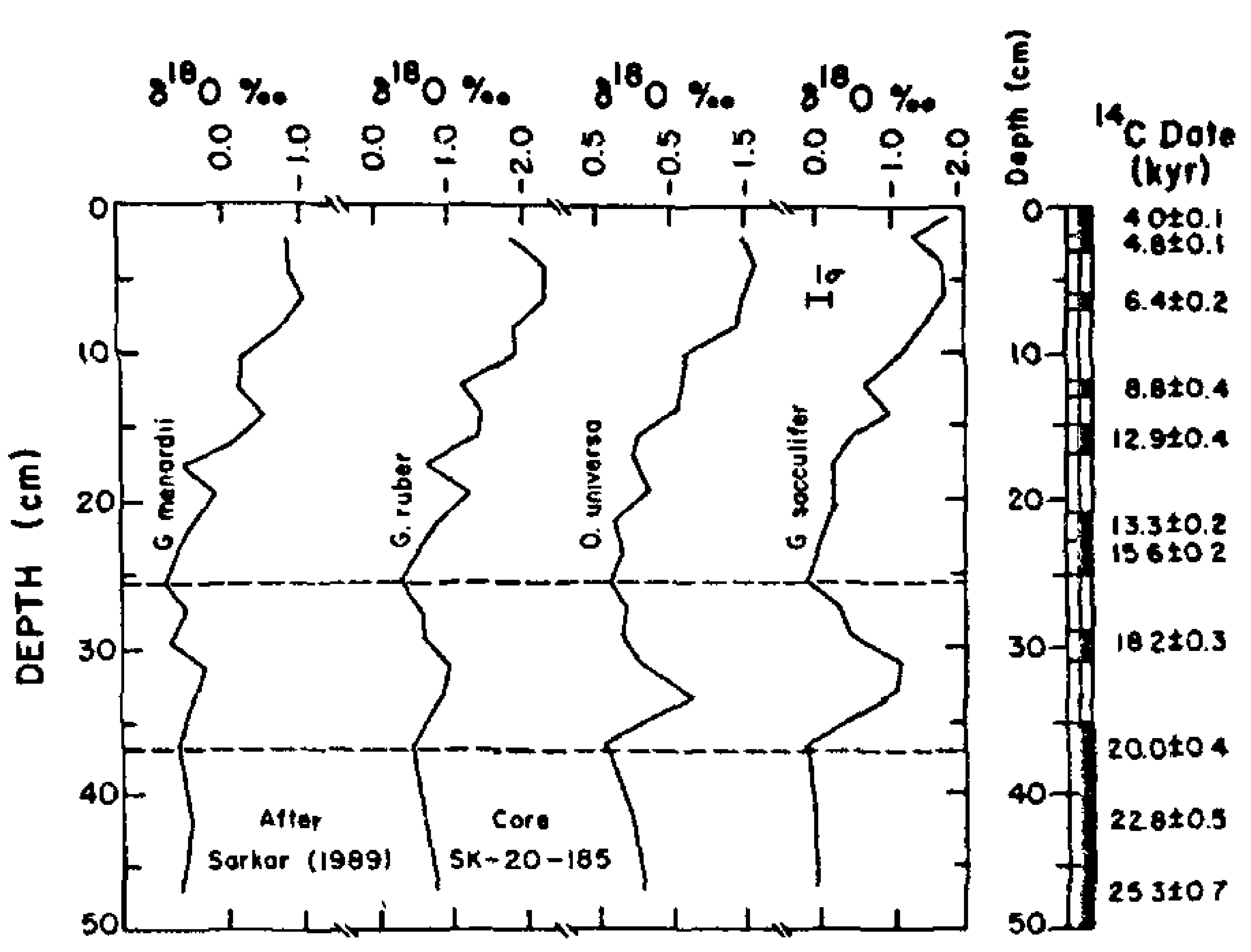

Figure 1. Oxygen isotope data of sediment core SK–20–185 from east Arabian sea, showing the enigmatic negative $\delta^{18}O$ spike of up to 1‰. This figure replaces figure 1 of ref. 2 in respect of radiocarbon dates.

and *G. ruber* from the east Arabian Sea[5] and (ii) lack of evidence[6] for a comparable magnitude of negative $\delta^{18}O$ excursion from active monsoonal upwelling region of the Arabian Sea (core CD-17-30; where the amplitude of the observed negative $\delta^{18}O$ excursion at LGM is only half of that observed in SK-20-185). Therefore, while we cannot rule out the possibility suggested by Naqvi that the enigmatic $\delta^{18}O$ shift reflects local thermohaline changes in the surface water, we believe that this may not wholly account for the observed signal.

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OPINION

Latur earthquake of 30 September 1993: implications and planning for hazard-preparedness

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The moderate earthquake (M 6.4; depth 7 km) of 30 September 1993 in the Latur area in Maharashtra (Figure 1) has once again demolished the thesis that the Peninsular India is a geodynamically stable shield. It has also strengthened my belief and postulation[1,2] that southern India did not and cannot escape the impact of northward drift of India and its collision with Asia. The resistance of the Indian plate to sliding under the Asian Plate is manifest in neotectonic movements and attendant seismicity in the Himalaya as well as in the Peninsular India. The earthquake-affected Latur belt has been exhibiting microseismicity for quite sometime – 1962, 1963, 1967, 1983, 1984 and in 1993 from 2 to 29 October. Obviously, internal strain has been progressively building up all through the time. Our inability to heed to the nature's loud signals is not only a sad commentary on our apathy, but also on the inexplicable failure of our seismological network to give timely warning of the uncommon occurrences (tremors) leading to a calamity of unimaginable proportions

Seismicity in southern India

The 1764 Mahabaleshwar and the 1843 Bellary earthquakes are among the many that have rocked southern India since time immemorial[3,4]. The seismicity is related to neotectonic movements on the NNW-SSE and E-W trending faults and fractures[5] (Figures 1 and 2). For example, the 8 February 1900 Coimbatore earthquake (M 6.0, hypocentral depth

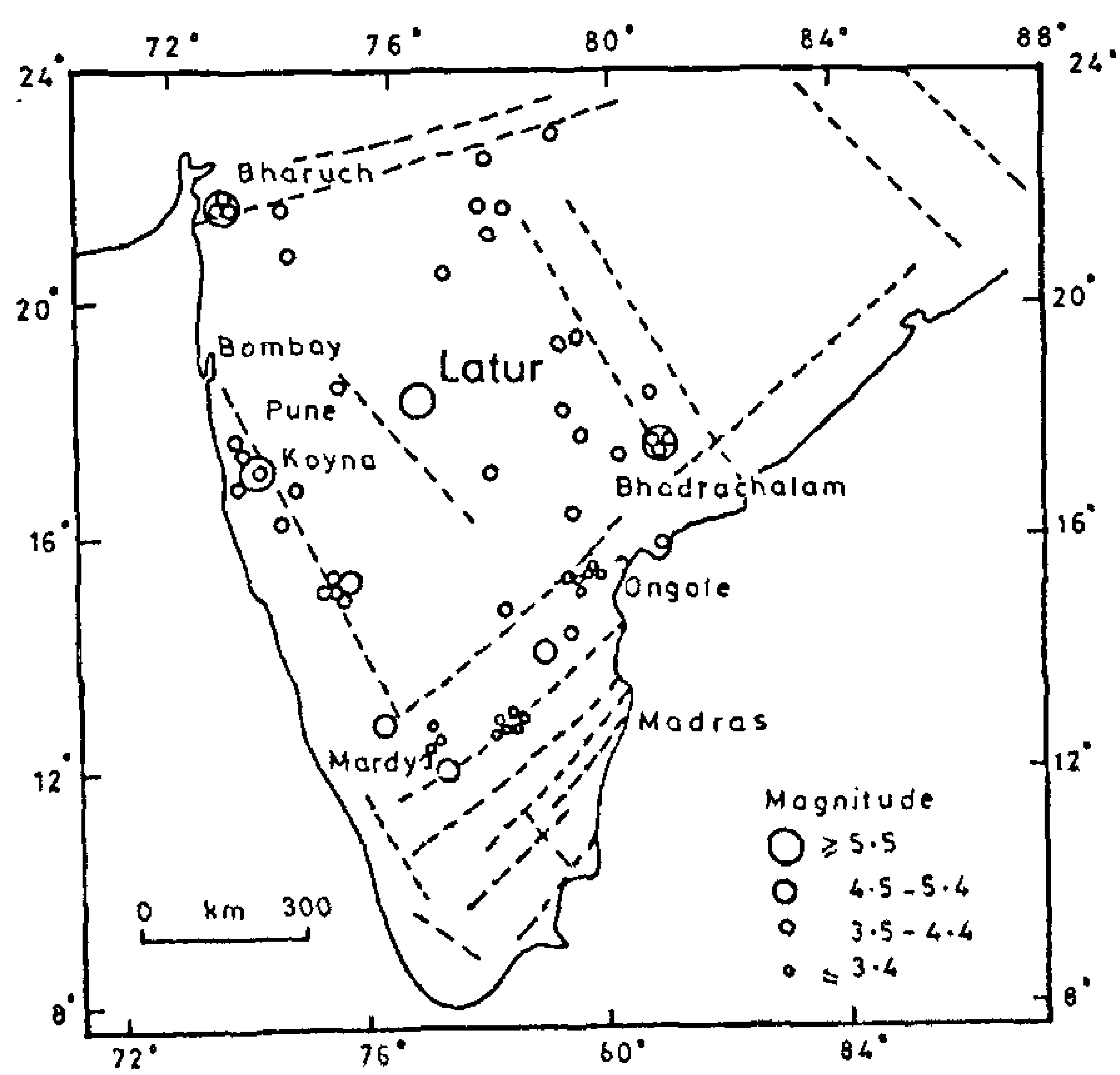

Figure 1. Seismicity in the Peninsular India is related to episodic movements on transcurrent faults (From Valdiya[5], based on the Annual Report of the NGRI, Hyderabad, 1977).