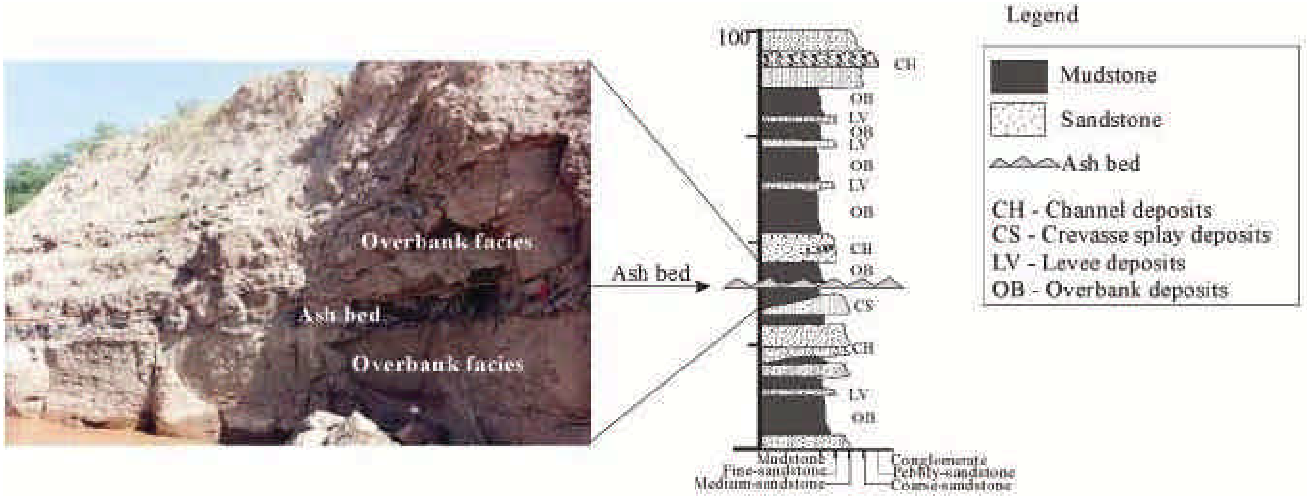

Figure 1. Occurrence of ash bed within the overbank deposits (see Kumaravel *et al.*[18] for details).

mentology[9], biostratigraphy[3,4,6] and magnetic stratigraphy in surrounding area[13–17].

All these points are explained in the paper[18] and yet we have not declared the section as type section, but only suggested to 'consider' it as type section.

The other point raised by Gupta and Kochhar regarding the 'reliability of the zircon age dating method' is out of context of our paper.

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Earthquake at Koyna

This has reference to the correspondence, 'An earthquake of $M \sim 5.0$ may occur at Koyna' by Harsh Gupta *et al.*[1]. At the outset I would like to observe that this note is not at all based on any currently accepted theories and associated earthquake mechanism.

The subject of reservoir-induced seismicity (RIS) cropped up after the 1967 Koyna earthquake. Other similar contemporary seismic events were Kariba (South Africa), Kremesta (Greece) and Xingfengkiang (China). The subject of RIS attracted some attention during the 6th and 7th decade of the last century. In defence of RIS, two arguments were hypothesized. First, the load of the water body in the reservoir causes the rock to yield. Secondly, water trickles down and the weaker section of the rock acts as a lubricant for its movement.

Both the above reasons were found to be wrong. The effect of water load was calculated. It has been observed that if the height of the reservoir is h, then the effective stress on the underneath rock is active and effective to a depth of $2h$. Below this depth there is no effect of the stress. The height of Koyna dam is about 103 m. As such, any effect whatsoever should not be felt at a depth of more than 206 m or so. The hypocentres of the earthquakes in the Koyna region are located at a depth of 2 to 10 km depth. Therefore, the water load or change in water level, whether at slow or rapid rate, will not make any effect on the rock at a depth of a few kilometres or so. Calculations have shown that the load due to the water body on the rock comes to about 3 to 4 kg/cm^2. This value from rock mechanics and rock properties point of view is very low to cause any harm to the rock.

The second reason of water trickling down was found to be totally wrong. For Stage IV of the Koyna dam project, the reservoir was punctured at the bottom. This experiment is known as 'lake tapping experiment'. Water from the bottom was taken through tunnels about 600 m down for additional power generation. During this work, huge excavation was undertaken for construction of tunnels. No water was encountered at any stage. Nowhere was the rock found to be wet or moist. These experimental evidences from Koyna dam are enough to reject the water percolation theory.

The authors have talked and discussed about Koyna earthquake and predicted an earthquake. The presentation is grotesque and nebulous. It is a fact that there have been thousands of micro-earthquakes at Koyna during the last four decades. It needs to be noted that earthquakes of magnitude up to 4.0 on the Richter scale are mostly within the elastic limits of the rock. The extensions or reductions in length, area and volume of the rock are temporary and vary with load. On the other hand, earthquakes of magnitude 4.5–5.0 and above are accompanied by fracture, rupture, cracks or some displacement. This is the basic mechanism of earthquakes of magnitude 5.0. How is it possible that fluctuations in water level (at whatsoever rate it may be) with a variable load of about 4.0 kg/cm^2 on the rock, up to a depth of 206 m could affect the rock at a depth of 2 to 7 km? Arguments by the authors have not discussed anything about rock mechanics or physics or earthquake! The unscientific nature of the paper is amply manifested by mistakes, errors and wrong arguments.

(a) The legend below figure 2 describes the duration as 9–23 August. While the same in the text is given as 1–23 August. Which one is correct?

(b) The footnote talks of 50% probability and further says that if two earthquakes of magnitude 4.0 or more occur, then the probability is enhanced. No numerical figure has been given for enhanced probability. Enhanced probability over 0.5 could be anything in the range of 0.51 to 0.99. Probabilistic statistical analyses require at least 30 points for obtaining a fair distribution and result. The authors have taken only 19 points and performed statistical analysis.

(c) The authors have talked about Kaiser effect and acoustic emission. When a solid body is subjected to load, the molecules are pushed and the molecular distance is reduced. In this molecular level movement, some energy transfer takes place and it could be detected. This could be heard with suitable instruments. This has nothing to do with the actual cracking or fracturing of rock. The reference to Kaiser effect in the text talks about 'monotonically increasing stress'. On the other hand, the authors have discussed about 'fluctuating load' and 'faster rate of loading', etc. The acoustic emission is at micro or molecular level, while changes in level of water body in the lake are at mega level.

It is difficult to understand why the authors have sandwiched their communication in one repeated sentence. The sentence, 'till December . . . may be considered as false alarm' appears both at the beginning and also at the end. This indicates escapism or pessimism. In case of failure or success, they should reserve their comments till January 2006.

Even if an earthquake of magnitude 5.0 occurs at Koyna by or before December 2005, it will be solely due to natural tectonic or seismological process and not due to changing water levels. The 'bathing of rocks' under Koyna reservoir will not be a cause.

1. Harsh Gupta *et al.*, *Curr. Sci.*, 2005, **89**, 747–748.

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Response:

We find that the comments by Arun Bapat (AB) on our article[1] are scientifically incorrect and reflect lack of understanding. AB should read papers on this topic, cited in this note, as well as papers and chapters that were published in several seismological journals and books during the past 20–30 years. In his comments, AB has not cited any reference for his argument against the topic and work, which are well accepted worldwide and published in highly reputed journals and books. He is challenging some basic, well-established concepts of seismology in general and the phenomenon of earthquake triggering in particular. Our response to the comments by AB (which span seven paragraphs) is as follows:

Carder[2] was the first to associate the triggered earthquake activity with the filling up of Lake Mead. Later, many more similar cases were reported (see table 1 in Gupta[3–11]).

The subject of triggered seismicity has now been investigated thoroughly and applied extensively to understand (i) the effect of earthquake stresses in triggering aftershocks or nearby earthquakes[12–15], (ii) tidal triggering of earthquakes[16,17], (iii) earthquake triggering by fluid injection/extraction[18] and (iv) earthquake triggering by artificial reservoirs[3,5–11]. It has been pointed out that these processes do not cause the earthquakes, but only act as a trigger. The prerequisite for these processes is the requirement that the causative fault is critically stressed. Under this condition, slight modification in the stress state leads to failure. Failure is governed by Coulomb–Mohr criterion, according to which it occurs when shear stress exceeds the effective frictional stress (i.e. $t \geq m(s - p)$). Another recent finding with regard to earthquake triggering is that no lower threshold exists for stresses to trigger an earthquake[19]. Thus a small change in stress state can trigger an earthquake. With this background, we now discuss the case of earthquake triggering by reservoirs. Rather than using empirical formulae, changes in stress and pore pressure due to a 3D reservoir and its operation have been computed using realistic earth model, i.e. using poroelastic earth[6,10,11,20–22]. It has been shown that the effect of load is significant even at a depth of 10 km (for Koyna)[21,22]. Increase in pore pressure occurs due to two main reasons, namely diffusion and compression. For the development of pore pressure, it is not necessary to have a physical connection between the reservoir and seismogenic fault (i.e. no flow boundary condition)[6,11].

Reservoir-triggered earthquakes began to occur in Koyna region soon after the initial impoundment of the Shivajisagar lake in 1961. Subsequently, filling of the Warna reservoir started in 1985. Until today, 19 earthquakes of magnitude ≥ 5, including the 1967 earthquake of M 6.3 have occurred. Sixteen of these earthquakes are associated with the loading phase of the reservoirs. If one considers the energy release due to larger Koyna–Warna earthquakes, one observes that most of the energy release (larger earthquakes) occurred in one third of the year (September–December). The period of enhanced seismic activity starts about 12 weeks after the start of filling in June, and continues for about 16 weeks after the reservoir is full. These observations are in agreement with the time for pore-pressure diffusion from the reservoir to hypocentral locations with hydraulic diffusivities of 10^4–10^5 cm^2/s, a value found at Koyna–Warna seismic zone and other RTS sites in the world[23,24]. Thus AB's comments in the first four paragraphs are not valid.

AB has apparently not understood the mechanism of earthquake occurrence. Slip on a plane is essential for all earthquakes, irrespective of their magnitude (with the exception of a few deep-focus earthquakes, which may occur due to phase transition, etc.). AB's statement 'It needs to be noted that earthquakes of magnitude up to 4.0 on the Richter scale are mostly within the elastic limits of the rock. The extensions or reductions in length, area and volume of the rock are temporary and vary with load. On the other hand, earthquakes of magnitude 4.5–5.0 and above are accompanied by fracture, rupture, cracks, or some or the other displacement. This is the basic mechanism of earthquakes of magnitude 5.0', is totally absurd.

In response to comment (a) concerning errors, we appreciate AB for pointing out the typographic error in the article. The duration '9–23 August 2005' is correct.

In response to comment (b), it may be noted that Gupta and Iyer[25] provided a simple statistics of the events, which suggests that there is a 50% probability of occurrence of an $M \geq 5$ earthquake, if two earthquakes of $M \geq 4$ are closely spaced in time. We suggested enhanced probability of occurrence of earthquake of $M \geq 5$ at Koyna due to the fact that besides meeting the above requirement, the reservoirs have witnessed higher rate of filling and higher level of water compared to that in the past four years.

In comment (c), AB's remarks are not clear. In any case, Kaiser effect has amply been described in the text of our article. For further discussion on it and its application to reservoir triggering of earthquakes, we suggest that AB should read Simpson and Negmatullaev[26] and Gupta *et al.*[27]. He may also like to see the report by Talwani *et al.*[28] on Koyna earthquakes.

It is well known that earthquake forecasting is difficult. We are of the view that Koyna–Warna is the most suitable site for the study of precursors leading to possible forecast[3]. Our forecast is driven by a knowledge base and observations. In the past, earthquakes of $M \geq 5$ in Koyna–Warna region triggered by loading of reservoirs have occurred in the time span extending to the end of December.

With reference to the last comment of AB, we would like to once again emphasize that reservoirs only trigger earthquakes.

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