

to cite them in the guise of relevancy to the journal. What is more is that the prospective author, in order to increase the chances of acceptance of his/her article, cites more articles from the journal he/she is submitting it to.

On the other hand, the relationship is quite complex when considering scientists, science and IF. The initial idea of Eugene Garfield to identify which journals mattered the most to scientists, is much distorted now. In fact, IF is playing a crucial role in employment, promotion and awarding of grants. The first objective of the researcher would be to publish in a high IF journal, by which, irrespective of the quality of material published or citation of his/her work, the researcher gains a quantum advantage¹. Increasing awareness about journal IF and its use in academic evaluation is already changing scientists' publication behaviour towards publishing in journals with maximum impact, often at the expense of specialist journals that might actually be more appropriate vehicles for the research in question². The second is that a researcher tends to cite his/her work more to gain citation advantages and if a subsequent self-citation gets into the same journal, the editor is also pleased. The consequence is that the priority of publication becomes more important than the quality of research itself. The scientist, according to his/her ability can get into a high IF journal without actually making a significant contribution to science.

In essence, it should be borne in mind that citation does not automatically mean

that a work is of high quality. A work may be highly cited because many other authors are refuting the research findings it contains. Basically, IF is a measure of average citation impact, not individual citation impact, so an IF cannot be used to measure the performance of an individual.

On the contrary, not all research work is published and cited in the citation indices: conference proceedings, for example, are often poorly covered. Hence peers should exercise extreme caution in ranking scientists on the basis of IF alone. Now looking at how science itself is affected by the fuzzy maths of IF, we see that high impact journals call for papers to be topical, and to present important science. Hence scientists are changing the kinds of questions they investigate to accommodate these journals, an attitude that in a way takes science off course. This can lead to results of key experiments being published in such a way as to optimize the sum of the IFs rather than the effectiveness and value of scientific content³. There is a clear bias in favour of English language material on citation indices and it seems that the medium of publication can skew the path of science⁴.

In India, National Academy of Agricultural Sciences (NAAS), New Delhi has come out with ratings for different journals which are being accepted by research organizations such as the Indian Council of Agricultural Research, New Delhi. This effort is commendable in that it is a beginning and Indian journals that do not find a place in ISI have been rated too. Another advantage is that the assessment

of scientific research on the basis of the impact of individual publications/journals will auger well for quality of research instead of a game of numbers – the more you publish, the better it is. But caution is again required as the list is not exhaustive and many reputed journals do not find place in the rating list, which we assume would be dynamic, as lists are announced for individual years. Again, in contrast to ISI, where a clear-cut formula exists for calculation of IFs, the basis of NAAS rating is unclear. Citation analysis and journal IF can be a worthwhile criterion for evaluating publication records of individual scientists or research units with some amount of flexibility to suit the field of research. Although at the international level, the ISI Thompson is working to make it as flawless as possible, the question is whether science has an alternative to evaluate itself and the people who are shouldering it.

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Volcanic ash beds

Kumaravel *et al.*¹ have suggested that the Ghaggar section be considered as a standard stratigraphic section for the Pinjor Formation (near Chandigarh) of the Siwalik Group. They have used the reported occurrence of volcanic ash beds from Ghaggar river section as the benchmark and, further based on fission track zircon age (2.14 ± 0.5 Ma) from the reported ash bed, the Gauss–Matuyama polarity reversal and lower limit for the Pinjor Formation have been constrained. Our comments pertain to the following points: (i) validity of ash

beds; (ii) reliability of zircon age, and (iii) Not citing inconvenient published work.

(i) Based on mineralogical, petrological and geochemical studies, the volcanic nature of the so-called ash beds has been negated^{2–6}. This is due to the following reasons:

(a) Absence of glass shards, pumice fragments and high quartz in these rocks.

(b) The zircon separates from the ash beds are clear, zoned with elongation ra-

tios (1.41 to 9.48). These features are suggestive of magmatic origin. Some are reworked also. They do not show volcanic characters.

(c) The biotites are of metamorphic source.

(d) Heavy minerals such as epidote, rutile, topaz, kyanite, magnetite, ilmenite and hematite, separated from these rocks are reworked.

(e) Paucity of trace elements such as Ni, Cu, Cr and Co in these rocks indicates that these reworked sediments were

probably derived from acidic source and not from basic rocks.

(ii) Gupta *et al.*⁵ have shown that zircons in these ash beds were derived from different sources such as granites and pegmatites. That is the reason why different researchers have obtained different ages on them. Mehta *et al.*⁷ obtained FT age (2.14 ± 0.5 Ma) on random zircon samples from the ash beds. They also observed variable amount of detritus material in the samples they studied. As such, this age cannot be used for correlation purpose or for calculating rate of sedimentation as proposed by the authors¹.

(iii) A number of research papers have been published by Gupta and his co-workers (cited above) dealing with various aspects of the so-called ash beds, and their detailed work has negated the volcanogenic origin. A critical appraisal of the ash beds in NW Himalaya was also published⁶. None of the papers published by us is cited by Kumaravel *et al.*¹, since our work does not support their model.

In view of the aforesaid comments, the ash bed cannot be used as a marker horizon for constraining Plio–Pleistocene boundary. The Ghaggar river section should not be considered as a standard section for the Pinjor Formation.

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Response:

We thank Gupta and Kochhar for their critical comments on our paper which presents the magnetostratigraphy for the Ghaggar section, where the reported ash beds^{1,2} coincide with the Gauss–Matuyama (GM) polarity reversal. The study area has been assigned as the Pinjor ‘type area’ and the Plio–Pleistocene age is unequivocally reported in the previous literature^{3–7}. The aim of our study was to constrain these ages using magnetostratigraphy. In this context, the ideal magnetic polarity pattern itself independently justifies the GM pattern of the Geomagnetic Polarity Time Scale⁸. The occurrence of volcanic ash that was independently dated by Mehta *et al.*² within the same section can form the benchmark to further confirm the ages reported in this paper.

The main point raised by Gupta and Kochhar namely ‘validity of the ash bed’ is not in the context of the paper, as we used the available published data of Mehta *et al.*². It is not yet proven in the literature that the bed does not agree with the published date of (2.14 ± 0.5), to restrict its use as a benchmark in our paper, although the validity of the bed as volcanic ash has been questioned frequently by Gupta and Kochhar in their publications in an in-house journal, *Indian Mineralogist* and in a correspondence to *Journal of the Geological Society of India*.

The comment thus does not relate to the context of the paper but to a different issue of the identification of the ash bed. We have used the published fission track date that has not been negated by the correspondents; neither has a new age been provided by anyone. However, in the following we explain our understanding of the whole issue. From the field occurrences of the ash bed as under horizontally bedded overbank lacustrine deposits (Figure 1) and detailed available literature mentioned therein, we are convinced with the volcanic ash characters rather than a large magmatic rock fragment. We present the following facts in this context:

1. The Pinjor sedimentation took place by piedmont drainage^{9,10} and there is no occurrence of magmatic rock in its source area/catchment or hinterland.

2. If it is not an ash bed and a large magmatic fragment, then its occurrence as overbank deposit and bentonite within

needs clarification. Considering the depositional settings, environment and energy conditions, how can a large magmatic body or zircons with high specific gravity (4.8) occur on floodplains?

3. If it is source-derived material in the sedimentation process, then why is it anomalously restricted to single stratigraphic level in this section as well as the distant equivalent Siwalik sections (reported elsewhere), where the source area composition is entirely different?

4. Gupta and co-workers also note that zircons are euhedral. How can it be explained by any other mechanism, as the zircon becomes rounded during transportation and is a common observation in the rest of the Siwalik section occurrences?

5. Why is high concentration of zircon confined particularly at the small stratigraphic thickness (<1 m) and not elsewhere?

6. Moreover, the occurrence of similar kind of ash invariably with the equivalent age is widely reported from other parts of the Siwalik Group^{11–14}. Had it been a magmatic rock fragment, then how did it occur in such a wide geographical extent in a single stratigraphic level at such younger age of the Himalayan orogeny?

7. Magmatic hinterland source in the ‘salt and pepper’ sandstone of Siwalik is widely reported in the Middle Siwaliks but with very low concentration of zircon (<0.5%) and good roundedness of the fragment. Such sandstone does not occur in the Ghaggar section.

8. The volcanic ash as shown in Figure 1 and described in the published literature occurs in confirmation with stratigraphy/bedding. The depositional environment is clearly low energy to ponding conditions, denying any large magmatic rock occurrence that needs tremendous energy to be deposited over floodplains.

9. We observed definite glass shards in our samples collected from the locality. These samples are available with the authors.

Gupta and co-workers have not explained the depositional environment and settings for the occurrence of magmatic body in any of their publications in addition to the above points. It will be quite informative to us if these points are explained by them.

Secondly, we proposed this section as type area because of the following convenience and availability of information: Well-established rodent stratigraphy⁷, sedi-

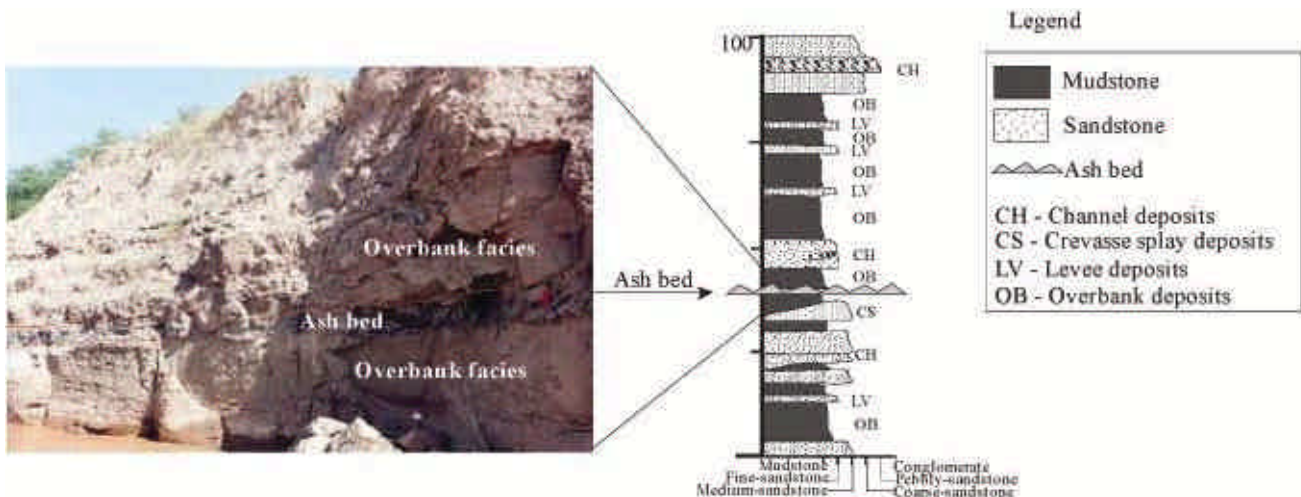


Figure 1. Occurrence of ash bed within the overbank deposits (see Kumaravel *et al.*¹⁸ for details).

mentology⁹, biostratigraphy^{3,4,6} and magnetic stratigraphy in surrounding area¹³⁻¹⁷.

All these points are explained in the paper¹⁸ 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 Xing-

fengkang (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