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SCIENTIFIC CORRESPONDENCE

Macroseismic studies

In a recent paper (*Curr. Sci.*, 1992, **62**, 24) Narula and Shome have given an account of recent earthquakes of Kinnaur, 1975; Dharamsala, 1978; Dharchula, 1980; Jammu–Kathua, 1980; Dharamsala, 1986, and report that, (i) the Main Himalayan Seismic Zone is intersected by transverse faults dividing into discrete blocks, (ii) seismic sources are parallel to the Himalayan trend and (iii) such modifications in the isoseismal trends might have been caused by the reactivation of pre-existing transverse zones of weaknesses.

Since this is a review paper, it is pointed that it is 'incomplete, no new ideas are presented and rather repeats old views without any reference. Hence, it is a borrowed idea'.

Similar views stating that:

- the Himalayan tectonic zone and the craton constituting Indian continental block is divided into intra-continental blocks conditioned by linear and transverse tectonic features, separated by transverse faults,
- transverse faults in the Himalaya might have developed due to block adjustments as a result of collision,
- transverse features cutting across the linear structural features in the Himalaya suggest influence of later collision processes and tectonic activity,
- grinding and friction during sub-block adjustment would initiate release of strain energy,
- seismicity is largely due to collision-controlled structures and segmental in character,
- re-activation of pre-existing structures *were given much earlier* (Ghosh[1], p. 39, 2nd para of the abstract, pp. 40,42,44,45; Ghosh[2], p. 98,99; and Ghosh and Jalote[3]).

The present writer[2] again pointed out that suitable environment for earthquakes is created where structural planes *intersect* or branch out in opposite direction and gave examples in the western Himalaya (Ghosh[2], page 99). In the same paper the present writer (Ghosh[2], pages 98,99) pointed out the following observations to Molnar of MIT, USA regarding western Himalaya:

- the Main Boundary Fault, the Main Central Thrust and the like parallel thrusts and faults are moderate to steep angle superficial features and are at best confined to the metamorphic basement such as Jutogh, Chail and their equivalents. Hence, they indicate involvement of the volcano-sedimentary, ortho–quartzite–carbonate layers and uppermost metamorphic crystalline basement and not the deeper layers
- the presence of large open folds with axial fractures, transverse features and a wide diffused-seismicity zone also indicate involvement of the upper crustal layers and large block type movement
- the absence of evidences regarding involvement of crust–mantle interface anywhere in the Himalaya except the Indus-Suture Zone
- the absence of intermediate and deep earthquakes and low angle (about 10 degrees or less) major structural features like that of MBF, MCT, etc. which are moderate to steep angle or such type of low-inclined seismicity zone to account for convergence and large underthrusting of the Indian shield beneath the Himalaya.

Molnar writes (personal communication to the writer in September, 87): '*I agree with the first three points and most of the fourth one* but I think they ignore other evidence, particularly fault plane solution of earthquakes in the Himalaya, that do indicate underthrusting on gently dipping planes. Specially the dips of faults at the surface need not continue at depth, throughout the world there are numerous examples of faults that dip steeply at the surface and that flatten out at depth'. The writer differed with this statement of Molnar especially with respect to the Himalayan region and drew attention to the fact that the determination of the orientation of seismic fault plane solutions has its own limitations since there is no generally acceptable method to distinguish the fault plane from the auxiliary plane.

Again the idea of realistic assessment of the source potential lying in the analysis of readjustment of the sub-blocks (a changed geotectonic environment) *was* given much earlier (Ghosh[1], p. 44) and that of re-activation of transverse features in Ghosh and Jalote[3].

It would, therefore, seem from the above statements that there is *nothing new* at all in the *review paper* by Narula and Shome. Again they have failed to comprehend several important aspects of macroseismic studies which are as follows:

- the significance of several deformational phases in the Himalaya, of which the folding and faulting on NE-SW axis are the youngest and not overprinted by any other still younger features
- the importance of inconsistencies/limitations in the interpretations of plate tectonics on continent interiors which are influenced by continental tectonics where we cannot apply the existence of any low angle base shear or detachment surface or any other similar feature at depth
- the significance of the presence of lokhan/salt bed in Dharamsala-Mandi area of western Himalaya as lubricating material in initiating movements along pre-existing feature (Ghosh[2], page 94).

1. Ghosh, D. K., *J. Eng. Geol.*, 1988a, **17**, 39–45.
2. Ghosh, D. K., Morpho-tectonic setting of the western Himalaya – an analysis towards Quaternary fluvial regime, seismicity and neotectonism, *Proc. Sem. Quat. Geol.*, Baroda, 1988b, pp. 92–106.

3. Ghosh, D. K. and Jalote, P. M., Seismotectonics of peninsular shield — a survey of Indian subcontinent (Abs), 27th International Geological Congress, Moscow, 1984, vol 5, p 77.

D. K. GHOSH

Geology Department
Lucknow University
Lucknow 226 001, India

P. L. Narula and S. K. Shome reply:

The part picked up by the discussor from the paper is from the discussion part wherein the behaviour and the patterns of the isoseismals have been sought to be explained by relating to the possible source mechanisms. The paper, as is explicit from the title, pertains to a review on the macroseismic studies and does not purport to be a critique on seismotectonic models or seismotectonic studies. However, we have attempted to explain the anomalous behaviour of the attenuation and accentuation patterns with the possible genetic relationships in consonance with the prevalent tectonic setting and, thus the allegation that 'it is incomplete, no new ideas are presented and rather repeats old views without any reference, hence it is a borrowed idea' is not warranted.

For intersection tectonics no claim has been made for it to be an original idea and in fact reference has been made to Talwani (1989). We are happy to note that the discussor had arrived at similar views from an integrated study of morphotectonic setting and Quaternary fluvial regime of the western Himalaya.

P. L. NARULA
S. K. SHOME

Geological Survey of India
Lucknow 226 001, India

RESEARCH NEWS

Materials with negative Poisson's ratio

G. S. Ranganath

It is a matter of common experience that solids when stretched in a particular direction suffer a contraction in the lateral direction. The ratio of lateral strain of contraction to longitudinal strain of extension is the Poisson's ratio, σ. It is always a constant for a given material. Most solids have a positive Poisson's ratio. Interestingly, the physics of elastic bodies does not impose any restrictions on the sign of σ. When a solid is stretched in one direction it can also expand in the perpendicular direction (see Figure 1). For isotropic materials it has a lower bound of -1 and an upper bound of $+1/2$. At $\sigma = -1$ the material has infinite rigidity modulus and at $\sigma = +1/2$ the material has infinite bulk modulus. It therefore intrigued many, including Poisson, that a large class of materials should have only a positive Poisson's ratio. Poisson himself came up with a molecular model that lead to $\sigma = +1/4$. In this model an assembly of spheres interact only along the line joining the centres with no force required for tangential displacements. This triggered an extensive search for materials with negative σ. The lowest value of σ was reported by Poisson himself in the case of cork ($\sigma \approx 0$). Poisson argued that bottle stoppers made of cork can be cylindrical in shape due to σ of cork being nearly zero. They will not laterally expand when axially compressed. On the other hand, it is nearly impossible to push in, through a bottleneck, a cylindrical stopper made of rubber since its σ is close to 0.5. Hence the stopper has to be of a tapered conical form. In the beginning, workers in this field confined themselves to looking at isotropic solids and without a single exception they all had positive σ. It was in the world of crystals that they had some success in finding negative Poisson's ratio. Here again a majority of crystals had a positive σ but a limited number of them exhibited negative σ. Single crystals of zinc and ammonium dihydrogen phosphate are examples of this rare class[1]. It must be remarked that both these are anisotropic crystals and negative σ is observed only for certain directions of axial strain. However, Love[2] mentions a single example of a cubic crystal of pyrite with $\sigma = -0.14$. He suggested that this negative value may have resulted from crystal twinning.

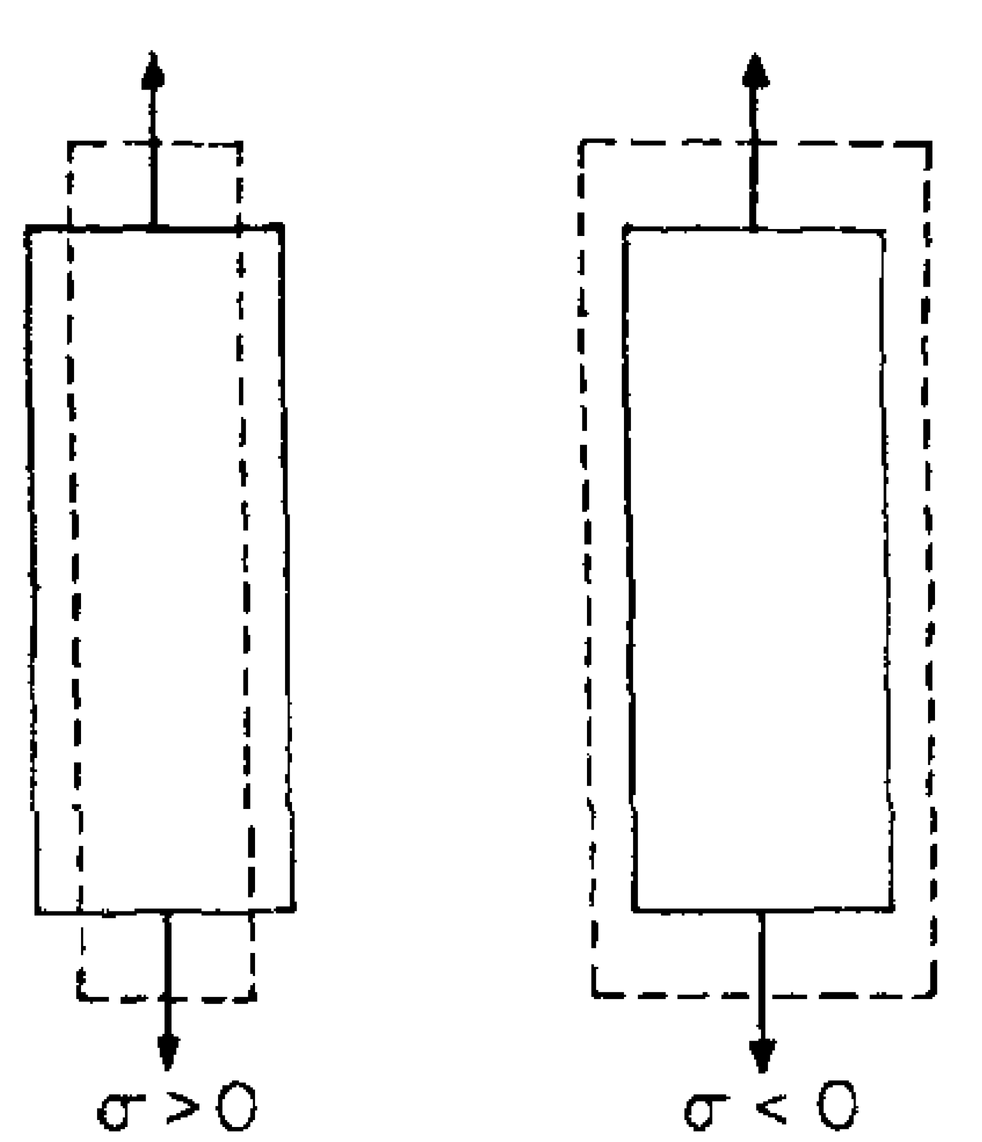

Figure 1. Effect of uniaxial tension on a strip of an isotropic material. The deformed structure is indicated by the dotted line.

It is against such a background that the recent paper in *Nature*[3] on 'Microstructure of isotropic materials with negative Poisson's ratio' by Rothenburg *et al.* assumes some significance. The authors have undertaken an *engineering* analysis of the problem. They were inspired in this exercise by the work of Lakes[4] on foams. Lakes produced, by a special process, foams of negative Poisson's ratio from conventional low-density open-cell polymer foams, which in their natural state have positive σ like any other isotropic material. Lakes subjected this normal foam to a triaxial compression, i.e. equal compression in three perpendicular directions and heated the foam to a temperature slightly above its softening temperature. The mold was then cooled to room temperature. This foam had undergone a compression by a factor of 1.4 to 4. Lakes found such compressed foams to exhibit negative Poisson's ratio. He got consistent results with foams made of different polymers and having different cell sizes. Interestingly reticulated metal