Inverted metamorphism

I have read the article 'Toward a solution for the Himalayan puzzle: Mechanism of inverted metamorphism constrained by the Siwalik sedimentary record' by Sorkhabi and Arita[1]. They have, on the basis of heavy mineral data of the Siwalik rocks, concluded that the metamorphic inversion is *apparent* and the result of differential uplift related to thrusting and shearing. They also concluded that structural disruption is post-metamorphic.

They have wrongly assumed that structural disruption is post-metamorphic because it was demonstrated by various workers that metamorphism is synkinematic with thrusting and deformation. This synkinematic inversion of the metamorphic zones poses the big puzzle.

Though inverted isograds are reported from many regions, these have not been studied critically. Specially, the dip of isograd surfaces have not been determined. Also most of the workers took isograds to be isothermals, which is entirely wrong. Bhattacharyya and Das[2,3] derived a simple equation relating dip of isograd, dip of isotherm, temperature gradient and $\Delta T/\Delta P$ of the reaction across the isograd. They measured dip of isograd at Buxa Duars and Gangtok in the Eastern Himalayas. At Buxa Duars, biotite grade rocks overlie chlorite grade rocks, dip of the isograd being 60° towards north. At Gangtok, staurolite grade rocks overlie garnet grade rocks which again overlie biotite grade rocks, dip of the isograd is 20° towards N70E. In both cases metamorphism is synkinematic. Inversion is real and not apparent. With the given value of $\Delta T/\Delta P$ of the reactions across the isograds, it was shown that at Buxa Duars with a horizontal temperature gradient of 18°C/km and at Gangtok with a inverted temperature gradient (whose value could not be determined), the metamorphic inversions were generated.

It may be noted that from the equation mentioned above, it was also derived that inverted metamorphism would occur even if the temperature gradient is downwardly directed, as it is normally the case, where isograds dip opposite the isotherm; in this case the value of the temperature gradient is most critical.

I do not think Sorkhabi and Arita are on firm grounds to explain metamorphic inversions in the Himalayas.

1. Sorkhabi, R. B. and Arita, K., *Curr. Sci.*, 1997, **72**, 862–873.
2. Bhattacharyya, D. S., *Tectonophysics*, 1981, **73**, 385–395.
3. Bhattacharyya, D. S. and Das, K. K., *J. Geol.*, 1983, **91**, 98–102.

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Response:

We thank Dr Bhattacharyya for his attention to our paper. We regret to say that he does not seem to appreciate the significance and novelty of the approach we adopted in our paper, i.e. to use the denudation record of the Himalayan metamorphic rocks in the Siwalik molasse to constrain the models proposed to explain the inverted metamorphism in the Himalaya. It is necessary to employ various approaches to tackle the puzzle of the Himalayan inverted metamorphism.

Much of Bhattacharyya's comment does not directly evaluate, refute or reinterpret the sedimentary evidence we discussed in our paper, but rather introduces the works he and his colleague have published on a numerical analysis of the Himalayan metamorphic zonation.

He points out that the inverted metamorphism in the Himalaya is 'real', not 'apparent', and criticizes us for saying that the inverted metamorphism is 'apparent'. We think that this point arises from a misreading of our paper. Of course, we agree that there is a real inversion of metamorphic zones in the Himalaya and the very purpose of our paper was to constrain models for the Himalayan inverted metamorphism. What we said was that thermal causes of inverted metamorphism would produce a 'genetic' inverted metamorphism, which is the 'primary pattern' in the rocks. On the other hand, post-metamorphic structural disruption of metamorphic zones would produce an 'apparent' inverted metamorphism. Then we argued that structural models (more probably, imbricate thrusting and distributed ductile shearing) for disruption of metamorphic zones are consistent with the sedimentary evidence in the Siwaliks because metamorphic index minerals seem to have been uplifted and eroded successively through time; i.e. the higher-grade rocks have been deposited more recently due to differential uplift and exhumation.

He draws our attention to the north-dipping of the metamorphic isograds and the piling of the biotite zone over the chlorite zone, and so on. This is one of the best established facts about the metamorphic rocks in the Main Central Thrust (MCT) Zone and the Higher Himalaya that the rock fabric shows a consistent north-dipping sense of motion in the dip direction of the MCT and its various splay thrusts and shear structures within the Higher Himalayan Crystalline Complex (HHC). This fact itself supports our notion that the apparent inversion of metamorphic zones is the result of thrusting and shearing – hence the north-dipping of the isograds. It should also be noted that imbricate thrusting and ductile shear structures within the HHC have been mapped by several workers (which we referenced in our paper), and we call on structural geologists to pay more attention to internal deformational structures in the Higher Himalaya rather than limiting the structural setup of the Himalaya to a few clear-cut thrusts such as the MCT.

Precise kinematic and temporal relations between metamorphic events and deformational structures are little constrained in the Himalaya. It is well known that the metamorphism in the HHC is a Tertiary phenomenon and a product of the India–Asia collisional tectonics. However, there is no reason to believe that the Himalayan metamorphism and the inversion of metamorphic sequence took place at the same time, and are genetically related (i.e., thermal causes for the inverted metamorphism).

While we never claimed that our paper definitely solved the Himalayan puzzle of inverted metamorphism once for all, we still consider our interpretation as valid.

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