DISCUSSION

Comment (1)

Comment on the paper, **"The Sandur Schist Belt and its adjacent Plutonic Rocks: Implications for Late Archaean Crustal Evolution in Karnataka"** by Brian Chadwick, V.N. Vasudev and Nazeer Ahmed, published in Jour. Geol. Soc. India, v.47(1), 1996, pp. 37-57.

The authors should be congratulated for abandoning the earlier intracratonic model in favour of a different one. The paper presents an interesting map, which should have been published in colour. The contributions of Mukhopadhyay and Matin (1993) should have been given due importance and credit. In fact their work demonstrates the non-existence of the Sandur Syncline.

While erasing Closepet granite from the Geological maps of India, the authors should have referred to the pioneering works. On what scale the authors have mapped the area of Closepet granite? The proposal of an evolving batholith from Hungund to northern Tamil Nadu is unacceptable because a batholith is a structural term with well defined dimensions, and an event. If the idea of the evolution of a batholith is accepted, then the entire continental crust from 4 B.Y. to 2.1 B.Y. has to be considered as an evolving batholith. In addition to these comments, we request the authors to clarify the following:

1. What do the authors mean by an unstable-mixed mode basin?

2. In most cases, the bottom and top of their Formations are neither exposed nor available; then how do they define their Formations.

3. What are the differences between Vibhutigudda and Joga conglomerates as far as their tectonic setting and depositional environment are concerned. The inference that granitic basement similar to Chitradurga schist belt does not exist for Sandur schist belt is not acceptable on the basis of the similarities of the clast composition of Talya, Aimangala and other conglomerates of the Chitradurga belt and those of Sandur. The nature of conglomerates and graywackes in both these belts suggest that a pericontinental depositional environment adjoining an oceanic crust prevailed.

4. Chadwick *et al.* (page 45), now suggest that "early in its development the belt may have had a floor of oceanic crust". However, earlier on the same page they propose that the basement to the Sandur schist belt mostly comprised batholithic rocks. They repeat that (p. 45) "The basement to the Dharwar Supergroup in the west of Karnataka is continental crust comprising quartzofeldspathic orthogneisses older than c.3000 Ma and tracts of older supracrustal rocks". A few lines above they cite their 1992 paper claiming that "the group (Dharwar Supergroup) was deposited in an unstable volcanic regime characterized by irregular uplift and subsidence". Chadwick and his co-workers may make up their mind regarding what they want to presume and remove the confusion about their assumptions created by their different papers.

5. How do Chadwick *et al.* find the Sandur discontinuity? What is the implication of this discontinuity? We need unequivocal evidence in place of speculations. The eastern boundary between the Donimalai and Taluru Formations is also a thrust. The Joga cherts which have almost E-W strike and are interbedded in Taluru Formation, abut against the rocks of the Donimalai Formation having N 40°E strike. Low angle discordance between the metavolcanics of the Taluru Formation and the BIF of the Donimalai Formation in the eastern region can be seen even on Fig.2. of Chadwick *et al* (1995 and 1996).

6. An evolving batholith as the basement of an island arc and an oceanic crust do not go together at the same spot at the same time.

7. How can an island arc develop on an evolving batholith? At what depth were the magmas

for this evolving batholith produced? And at what depth did partial melting for basalts of the intra island arc having MgO from 18% to 8% took place? How do these basalts coexist with rhyolites and andesites of calc-alkaline affinities? The genetic interpretations of this paper are wide open for extremely pertinent questions in the absence of geochemical and isotopic data on the belt itself. This is further complicated when their zircon ages are also "surprisingly imprecise" (Chadwick *et al.*, 1995; Nutman *et al.*, 1996).

8. The base of the Vibhutigudda Formation according to Chadwick *et al.* "is not easily defined, because of the detachment and this detachment is not clearly exposed and its position has been inferred from its relation on the map". The angular contact between the top of Taluru and bottom of Vibhutigudda is presumed to indicate a tectonic contact. Same is the case between the so called bottom of Taluru and top of Donimalai Formation. Therefore why cannot the Taluru Formation have a tectonic contact with the Donimalai Formation also ? If the contact between their Taluru and Vibhutigudda Formation is a thrust one, how can the amphibolites to the east of the Vibhutigudda sequence, where again the younging directions are towards east, can be correlated with the amphibolites of Taluru Formation ? (No. 5 in Fig.2 of Chadwick *et al.*, 1996) The authors themselves accept that the so called western limb of the Vibhutigudda syncline is seldom present. There is no closure in this region. The wide hinge SE of Sugalammadevi Konda has been stretched by the authors too far to imagine a syncline which does not have its western arm and closure though the underlying rocks are present.

9. Chadwick *et al.* on page 43 accept that the banded ferruginous cherts of the Vibhutigudda Formation are similar to those found in the other Formations of the Sandur group. They also accept that the ferruginous cherts of Raman Mala Formation and Donimalai Formation may be of same age (page 48). If these BIFs are of same age and lithology and the contacts are poorly exposed, how do Chadwick *et al.* explain the deposition of BIFs at three different places in their stratigraphic succession in a continuous ocean, with a distance of about 20 km represented by Taluru Formation? BIFs are deposited at shallow shelf. Did three separated shelves exist in the Sandur region ?

10. The authors may read the recent work on BIFs, as the concept of hydrothermal source of BIF constituents has been provided by the REE and isotopic data.

11. What do the authors mean by "a Dharwar batholith suggest a more complex convergent system (p. 54) than the consistent northward subduction and continental collision in Newton's model"? In which part of the belt author's would like to presume their subduction zone?

12. Confining their work to the Sandur schist belt and adjoining granites alone, authors arrive at sweeping interpretations for the entire eastern Dharwar craton extending from Kushtagi to Kolar N to S and from Chitradurga to Cuddapah basin E to W. Based on the limited and "imprecise ages" available to them the authors are not justified in creating further confusion in Dharwar geology by suggesting terms like Dharwar batholith *etc.*, It is pleasing to see that Chadwick (in Chadwick *et al.*, 1992, 1995, 1996) has also started interpreting Dharwar geology in terms of plate tectonics and now even visualises the existence of an Archaean oceanic crust in Dharwar region for which geochemical evidence has been continuously marshalled by Naqvi and his team since 1973 onward and disputed by Chadwick and his coworkers through their structural interpretations. It is good, that like their evolving batholith, the Dharwar supergroup has also evolved from intracratonic basins in 1981 to "marginal or back arc volcano-sedimentary basins (p. 54)" in 1996. But, they should have given due credit to previous workers. For the last 15 years Chadwick (changing coworker) debated the work of others to support his model which now he himself has now abandoned. It has to be seen how long his island arc-evolving batholith model survives.

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Reply

Dr.Naqvi's comments are fundamentally flawed. He has failed to quote us correctly and he has accused us of confusions which are of his own making because he has not read our papers with due care and attention.

Contrary to Naqvi's first sentence, we have not abandoned our view that the Dharwar Supergroup in western Karnataka, i.e. the Chitradurga-Gadag tract and others to the west, accumulated in an intracratonic setting; the depositional and volcanic basins were ensialic as spelt out clearly in our publications since 1985. Since 1989 we have consistently referred to the oblique-slip mobile regime indicated by our analyses of basin development during accumulation of the Dharwar Supergroup in western Karnataka, and we have drawn attention to analogies with more recent plate tectonic settings reviewed for example, by Mitchell and Reading (1986). We had amassed sufficient data by the late 1980s to propose a marginal basin or incipient back-arc setting for the Dharwar Supergroup in western Karnataka which was presented at the Perth conference in 1990 (Chadwick *et al.* 1992). Our views regarding the Late Archaean foreland in the west of the Dharwar craton are entirely consistent with our more recent findings in the eastern part which resulted in our recognition of the Dharwar batholith and its intra-arc basins.

Contrary to Naqvi's assertion, we gave full credit to the important contributions by A. Matin and D. Mukhopadhyay.

Naqvi's comment on the Closepet Granite is puzzling. We have not erased any granites from any geological map of India. In the light of our recent studies, we have simply found that the term Closepet Granite is unsuitable because it is not a single granite as the name implies. Published isotopic age data and our detailed fieldwork have revealed that it comprises a plethora of different anatectic and juvenile granitic and other plutonic suites, many of which are polyphase. In answer to his question, we have mapped considerable areas of the Closepet Granite at a scale of 1:50,000. This work built on the detailed knowledge acquired by the second author during inspection of numerous quarries when he was based in Bellary and Raichur with the Department of Mines and Geology, Karnataka, 1980-1990.

With reference to the last two sentences of Naqvi's second paragraph, the Dharwar batholith has magmatic and structural coherence. Its dimensions are shown in Figure 4 of our paper. The batholith represents anatectic and juvenile, calc-alkaline crustal accretions during the Late Archaean, *i.e.* it represents an event, albeit with a time span of c.150 Ma on the grounds of published age data from widely separated parts of its outcrop. We are of the opinion that batholith accretion at convergent plate boundaries was an important feature of the growth of continental crust in the period quoted by Naqvi in the last sentense of his second paragraph.

With reference to Naqvi's numbered comments:

(1) We recommend that he reads our papers carefully.

(2) The formations in the Sandur Group are fully defined in our paper.

(3) The polymict conglomerates in the Donimalai and Vibhuti Gudda Formations probably had similar depositional and tectonic settings. Naqvi appears to imply that the presence of granitic clasts in the conglomerates of the Sandur Group indicates that the Sandur schist belt had a basement of Peninsular Gneiss (>c.3000 Ma) like that of the Dharwar Supergroup in western Karnataka. He is naive to believe that granitic clasts in the Sandur conglomerates necessarily indicate a basement older than c.3000 Ma. Naqvi gives no justification for the opinion given in his final sentence.

(4) We do not understand why Naqvi uses the word "now" in his first sentence. Our proposal is entirely consistent with an arc setting for the Sandur Group. He quotes us erroneously. We suggest that he reads our previous papers more carefully to clear up his confusions.

(5) We suggest that Naqvi pays more detailed attention to our paper. He remarks that the

boundary between the Donimalai and Taluru Formation is a thrust, but he provides no evidence. There is no mention of thrusting in his previous papers on the Sandur belt (Manikyamba *et al.* 1993; Manikyamba and Naqvi, 1995). The stratigraphy and structure of Roy and Biswas (1983) was used without comment in the former paper, but in the latter Manikyamba and Naqvi suggested that the Deogiri and Donimalai Formations of Roy and Biswas (1983) may be coeval, following Mukhopadhyay and Matin (1993). Manikyamba and Naqvi (1996) used an outcrop of deformed pillow-structured amphibolite to suggest that the Sandur schist belt is "an allochthonous remnant of accreted oceanic volcanism", but their questionable data raises serious doubts about their claim to "report evidence of horizontal compression, shearing and thrusting of volcanic rocks of oceanic origin and infer the consequent crustal shortening for the Sandur greenstone belt". Manikyamba and Naqvi (Fig.1, 1996) included a "Stratigraphy" with "younging" on their version of the map by Roy and Biswas (1983), but they made no reference to the fact that the consistent NE younging of their new stratigraphy bears more than a passing resemblance to that of Chadwick *et al.* (1995, 1996).

We are puzzled by Naqvi's remark that the cherts east and west of Joga abut against the NE-trending Donimalai Formation. This formation is dominated by NW bedding trends. We recommend that he studies our map (Figure 1) more carefully before commenting on the nature of our formation boundaries.

(6) We did not use the term oceanic crust. Is Naqvi really unaware that volcanic rocks in arcs have contemporaneous plutonic equivalents?

(7) We suggest that Naqvi consults a text book on igneous petrology for the answers to his questions. If he reads our paper carefully he will realise that his final sentence is irrelevant. We did not say that the SHRIMP zircon ages were surprisingly imprecise.

(8) Naqvi not only quotes us erroneously, but his remarks also raise questions about his awareness of the principles of structural geology, a point reinforced by Manikyamba and Naqvi (1996). Naqvi denies that there is a closure which we called the Sugalammadevi Konda syncline. If he chose to map the area, he would find the closure clearly displayed in the hills NW of Sugalammadevi Konda and in the low ground NE of Obalapuram.

(9) We have not been able to fully understand Naqvi's comment. We are aware that the iron formations in the Vibhuti Gudda Formation are lithologically similar to those elsewhere in the Sandur belt. We remarked that our thrust interpretation implies that the Raman Mala and Donimalai Formations may be of similar age. Naqvi seems to suggest that the iron formations of the Vibhuti Gudda Formation have the same age as those in the Raman Mala and Donimalai Formations. We did not say this. He may therefore answer his own question.

(10) After consultation of recent publications on the genesis of banded iron formations, we referred to our favoured view that banded iron formations were deposited in shallow marine shelf settings. We did not discuss the source of iron or silica.

(11) Naqvi omitted an important part of our sentence in his quotation. We suggest that he reads it carefully, and we recommend that he consults Newton (1990). We do not fully understand Naqvi's second question. We have laid no claims to the site of a Late Archaean subduction zone and we made no presumptions about one in the Sandur schist belt.

(12) To clear up his confusion, Naqvi should read our paper more carefully to learn that our recognition of the Dharwar batholith was not based solely on our work in the Sandur schist belt and its adjacent granites. Naqvi's claim that we have now started to interpret the Dharwar craton in terms of plate tectonics is unjustified. We have consistently endeavoured to interpret the craton in terms of plate tectonics. Our evidence has been acquired by dint of detailed mapping for the last 20 years. We are not in favour of speculation based solely on geochemical data that are not supported by reliable field evidence.

Naqvi's remark that we visualise exposed oceanic crust in the Dharwar region (we presume he means Dharwar craton) is unclear. We have disputed his so-called oceanic crust because his claim is at variance with field evidence that the Dharwar Supergroup in the western part of the Dharwar craton accumulated on a continental basement (Peninsular Gneiss, >c.3000 Ma). We have always given credit to previous workers wherever relevant when we judged it appropriate. Naqvi's claim that we debated the work of others to support our interpretation is absurd.

We have every confidence that our new term **Dharwar batholith** and our view that the Late Archaean Dharwar craton evolved in an oblique convergent plate setting will have much longer half-lives than many of the speculations espoused by Naqvi. They include not only his current vacillations over the Sandur schist belt, but also his beliefs that the Bababudan Group was intruded by the Chikmagalur Granodiorite (Naqvi, 1981; 1983), the basement gneisses near Shimoga intruded their cover of the Dharwar Supergroup (Naqvi *et al.* 1978) and the Chitradurga Group in western Karnataka was deposited on a proto-oceanic floor of simatic composition" (Naqvi *et al.* 1988).

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Comment (2)

Comment on the paper "**Sr, Pb and Nd Isotope Studies and their Bearing on the Petrogenesis of the Jalor and Siwana Complexes, Rajasthan, India**" by Dhar *et al.* published in Jour. Geol. Soc. India, v.48(2), 1996, pp.151-160.

The paper reports, probably for the first time, high quality isotopic data on Jalor and Siwana complexes of Rajasthan. While we appreciate the painstaking efforts of the authors for generating immensely valuable data, we have certain reservations regarding their interpretation as stated below:

1. The authors fit an isochron using Rb-Sr isotopic data providing ages between 736 Ma and 723 Ma. They also stated that Sm-Nd and Pb-Pb isotope systematics of these rocks are disturbed, thereby not permitting construction of isochron. It should be noted in this context that in conventional isochron plane *i.e.* $^{87}Rb/^{86}Sr$ - $^{87}Sr/^{86}Sr$ coordinate system a line does not always represent an "isochron line" and a good correlation might be due to the mixing of two end members of different crustal residence time. Therefore, the age calculated from the slope of the line ($e^{\lambda t}-1$) may not have any geological meaning (Langmuir *et al.* 1987; Faure, 1986) and it is very important to differentiate an isochron from mixing line. Theoretically, if two end members (A & B) of different $^{87}Sr/^{86}Sr$ ratios and Sr concentrations are mixed in different proportions, the $^{87}Sr/^{86}Sr$ ratio of the mixture can be calculated from the following equation (Faure, 1986):

$$(^{87}Sr/^{86}Sr)_M = a/Sr_M + b$$

Where

$$a = \frac{Sr_A\, Sr_B\, [(^{87}Sr/^{86}Sr)_B - (^{87}Sr/^{86}Sr)_A]}{Sr_A - Sr_B}$$

$$b = \frac{Sr_A\, (^{87}Sr/^{86}Sr)_A - Sr_B\, (^{87}Sr/^{86}Sr)_B}{Sr_A - Sr_B}$$

and M denotes mixture

In case of mixing, the data will display a hyperbolic curve in Sr_M - $^{87}Sr/^{86}Sr$ coordinate system where two asymptotes will be the two extremities of two end members. Here two geological situations may arise:

a) The original magma resulting due to the mixing of two end members (with respect to both

Sr concentration and ratio) before the intrusion but the system remains closed with respect to Rb and Sr since intrusion.

b) A younger magma with high heat content intrudes an older rock resulting in an inhomogeneous mixture of two different end members.

While in the first case a good intrusion age will be obtained from the regression line (with delineation of both Rb and Sr content and $^{87}Sr/^{86}Sr$ ratios of the two end members), in the second case it will provide an erroneous age. We have plotted the data of Dhar *et al.* on Jalor granite, olivine gabbro, rhyolite and Siwana granite in Sr_M - $^{87}Sr/^{86}Sr$ coordinate system, which clearly shows a mixing hyperbola where the two end members are represented by the olivine gabbro and Siwana granite (Fig.1). Additionally to draw an isochron for a rock assemblage, it is necessary that the different units (including the end members) should be genetically related and/or have same crustal residence time. A close look at the local geology of these complexes, however, does not support it. The Olivine gabbro and Jalor rhyolite show a ring dyke complex within granite indicating different time of intrusion. Furthermore, petrography indicates that the Jalor granite is not a typical fresh granite and contains hornblende and often fayalite. Presence of fayalite itself indicates that this granite was contaminated with more mafic, high temperature magma (here olivine gabbro). Petrography, Pb - Pb and Sm - Nd isotope systematics show that the Siwana granites are relatively fresh compared to Jalor granite thereby indicating its possibility as an end member. Plot of Rb-Sr data on $1/^{86}Sr$ - $^{87}Sr/^{86}Sr$ and $1/^{86}Sr$- $^{87}Rb/^{86}Sr$ coordinate systems show distinct positive correlations (Fig. 2) where again olivine gabbro and Siwana granite represent two extreme ends. This, along with the scattered plot of the data in initial Sr ratios (taking the age as 725 Ma) - $1/^{86}Sr$ and $^{87}Rb/^{86}Sr$ planes clearly indicate mixing of olivine gabbro and Siwana granite (Fig. 3; Wendt, 1993).

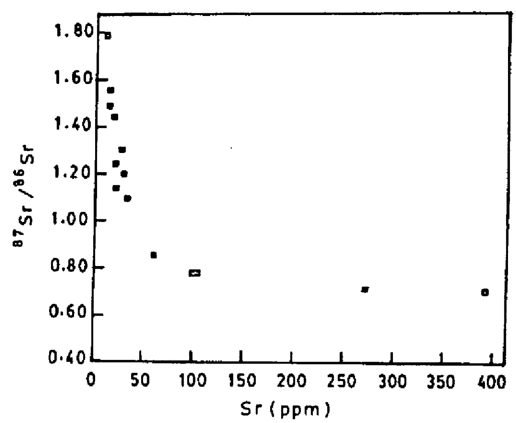

Fig.1. Sr *vs.* $^{87}Sr/^{86}Sr$ plot. Data showing a mixing hyperbola between two end members (olivine gabbro and Siwana granite)

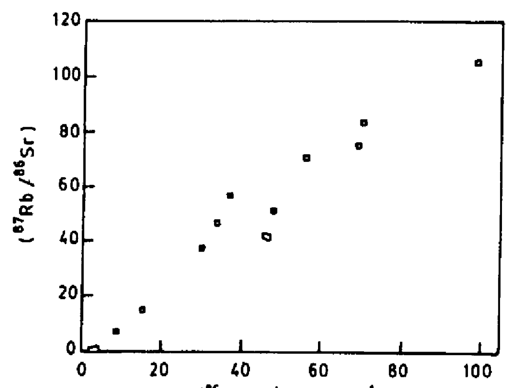

Fig.2. $1/^{86}Sr$ *vs.* ($^{87}Rb/^{86}Sr$) plot. Data showing a mixing line between two end members (olivine gabbro and Siwana granite)

2. The authors stated that the felsic rocks of both the complexes have been generated mainly by crystal fractionation processes of the mantle derived melt whose Pb and Nd isotopic compositions are more influenced by crustal contamination than the Sr composition. Since Rb is an incompatible element, it is difficult to visualise how crustal contamination will affect Pb - Pb and Sm-Nd system but not the Rb-Sr. As shown before, the good regression line (Rb-Sr) is nothing

but a fictitious isochron or a mixing line and does not represent an original undisturbed systematics as presumed.

3. The authors also comment that the slight scatter of Rb-Sr data of Jalor and Siwana felsic suites can be attributed to different crystal fractionations of the magmas. This statement is not tenable as crystal fractionation does not change the isotopic composition especially for higher mass elements *i.e.* no isotopic fractionation takes place at the range of magma temperature.

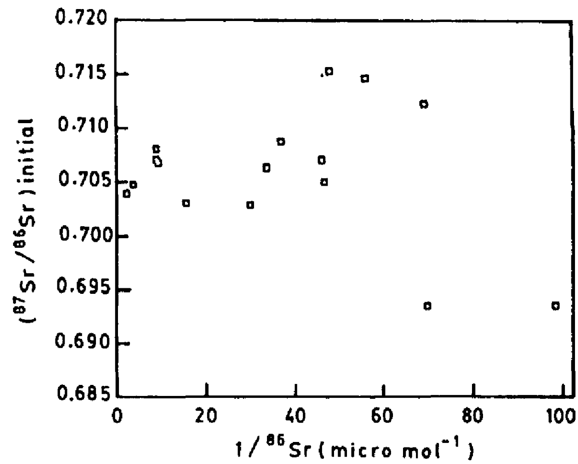

Fig.3. $1/^{86}Sr$ *vs.* $(^{87}Sr/^{86}Sr)$ initial plot. Scattered data points indicate mixing phenomena

4. The statement "the homogeneous ε_{Nd} (730 Ma) values of Siwana samples clearly support the interpretation of the 730 Ma Rb/Sr isochron age as an intrusion age" is not also fully correct. The value of ε_{Nd} depends not only on the age but also on Sm/Nd ratio especially $^{147}Sm/^{144}Nd$ and $^{143}Nd/^{144}Nd$ ratios of the rocks. Therefore, the consistency of ε_{Nd} values does not necessarily indicate intrusion age rather it points towards a less disturbed Sm-Nd systematics.

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Reply

We welcome the comments by Abhijit Roy and Anindya Sarkar on our paper entitled "**Sr, Pb and Nd isotope studies and their bearing on the petrogenesis of the Jalor and Siwana complexes, Rajasthan, India**". Their major citicism is directed towards the possibility that apparent Rb-Sr isochron relationships may also be interpreted as resulting from simple two-component mixing. In this respect they have touched an important and often overlooked problem in Rb-Sr whole rock dating and this merits careful consideration.

A closer inspection of the mixing diagram of AR and AS (their Fig.2) reveals that data points deviate significantly from a straight line and thus indicate that mixing cannot by itself explain the observed pattern. It is evident that data points of the Jalor magmatic suite (with the exception of one granite sample) define a clear hyperbola, not a line (our Fig.1), which is usually interpreted to evidence a single fractionation trend and not a two component mixing. Furthermore, the Siwana granites, really, as contended by AR and AS, cannot be lined up with the Jalor magmatic suite in this diagram (dotted line in our Fig.1). They consequently cannot represent potential felsic end members of the postulated two component mixing "line". We, therefore, conclude that the array delineated by all our Sr data in the diagram of Figure 2 of AR and As by no means indicate a pure mixing line but rather reflect the geochemistry of a fractionated suite which is characterized by low Sr concentrations and at the same time high Rb/Sr values for the evolved, felsic rocks, and by high Sr concentrations and low Rb/Sr values for the less evolved granites and the olivine gabbros. Furthermore, the rhyolites deviate from the fractionation trend defined by the rest of the Jalor magmatites (and from the Siwana tie line), therefore exhibiting variable but pronounced post-extrusive alteration.

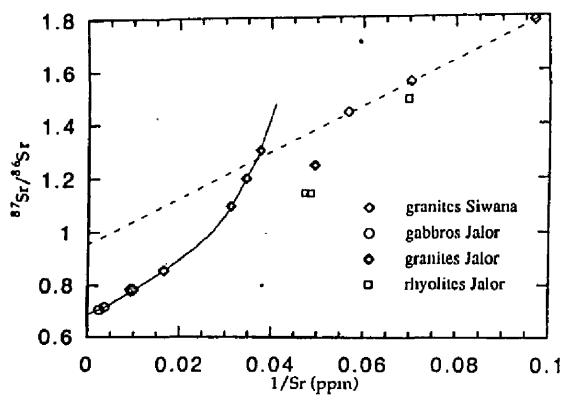

Fig.1 $^{87}Sr/^{86}Sr$ vs. 1/Sr (ppm) diagram with all data from the Jalor and Siwana ring dyke complexes. Arrangement of data points from Jalor magmatic rocks (with the exception of one granite) indicate fractionation rather than two component mixing. The stippled Siwana tie line does not connect with the Jalor gabbros. Jalor rhyolites do not plot on either correlation and therefore exhibit postmagmatic hydrothermal alteration (*see* text).

In the following we address the comments of AR and AS point by point.

1. Even if we consider the positively correlated arrangement of data points in a $^{87}Sr/^{86}Sr$ vs. 1/Sr diagram (Fig.2 of AR and AS) to evidence a two component mixing, two possibilities can be envisaged which either lead to geologically relevant (point a of AR and AS) or to meaningless (point b of AR and AS) age constraints. To test if the apparent Rb-Sr age in our paper represents a true geochronological age, it is essential to check, whether or not the potential two endmembers in a hypothetical mixing scenario represent geologically meaningful sources. In the case of Jalor, the question arises whether or not a primitive (olivine gabbroic) magma (suggested by AR and AS to represent the one endmember) was contaminated by older crustal material. If true, any age information from the Rb-Sr system would be obscured. In contrast, if the felsic contaminant (the other endmember) would be cogenetic and coeval with the primitive endmember and also characterized by an identical Sr isotopic composition (I-type magma), the resulting apparent age would still be geologically significant.

If a two component system is considered to explain the observed data spread of Jalor and Siwana samples, the felsic endmember would have to be a highly fractionated magma with a Rb/Sr ratio105 and a ^{87}Sr/^{86} of >1.78 (represented by the Siwana granites, as suggested by AR and AS). Taking the values reported for the felsic intrusions (or even more fractionated ones along the Rb-Sr regression line), we obtain Sr model ages relative to an upper mantle evolution (assuming a mantle ^{87}Sr/^{86}Sr of 0.7025) which range from 730-740 Ma. This implies that any endmember along this line exhibits a Sr model age which, within error, is of exactly the same age as our apparent Rb-Sr age for the Jalor and Siwana complexes. For this reason a *significant* influence of the Rb-Sr system by older crustal material can be excluded and our interpretation of the apparent age to represent an emplacement age of the ring complexes of Jalor and Siwana is still valid from Rb-Sr data alone.

Furthermore, as depicted in Fig.2 of AR and AS, samples which closely approximate the theoretical endmembers (highest and lowest Sr concentrations, respectively) do not show significant scatter. In contrast, enhanced scatter is exhibited by the samples with intermediate Sr concentrations. This is in conflict with an interpretation of the data array to result from mixing of a mafic magma with old (heterogeneous) crust. Such a mixing would produce the largest scatter for samples with the highest contamination level, *i.e.* with the lowest Sr contents.

2. As Pb and Nd concentrations are generally low in primitive melts relative to those in crustal rocks, they are easily affected by even small amounts of crustal contaminants during their genesis. In contrast, Sr is much more concentrated in the former magmas and therefore is less susceptible to crustal contamination during its genesis and ascent into the final emplacement level. In that it is not unusual that the effects of crustal contamination in I-type magmatic suite are more clearly "visible" in the Pb/Pb and Sm/Nd isotope systems than in the Rb/Sr system (*see* also for example Pinarelli *et al.* 1993). The slight scatter of data points around the Rb-Sr regression line may be interpreted to reflect Sr heterogeneity induced by variable degree of crustal contamination. Alternatively, the scatter is preferably interpreted to derive from postmagmatic hydrothermal alteration (Kochhar and Dhar, 1993), the effects of which can clearly be observed in the Jalor and Siwana area. Additionally, the low Sr initial values of all samples at about 730 Ma would only allow a contamination of juvenile crustal wall rocks (c. 730-740 Ma, see above). In contrast as we showed in our paper, Pb and Nd systematics point to an Archean contaminant. This again disproves the Rb-Sr systematics to be explained by the crustal contamination seen in the other two systems.

3. We have never pretended that crystal fractionation will change the isotopic composition of Sr, Pb or Nd in a magma. We pointed out that the scatter around the regression line may be due to (i) minor influence of assimilated old crust (as more clearly indicated by Pb and Nd isotope data) and/or (ii) by a secondary disturbance of the system. In this respect our expression 'loss of some radiogenic Sr' might have been misleading. Post emplacement hydrothermal alteration, as already described by Kochhar and Dhar (1993) for the Siwana and Jalor suites, is seen as one of the most probable processes to have led to the disturbance of the Rb-Sr system.

4. The homogeneous Nd initials calculated at the age deduced from the Rb-Sr data are clearly in line with an interpretation of the latter representing an emplacement age. However, on their own they do not unequivocally indicate an intrusion age, which anyway we did not stress. The more important aspect of the Nd data in the paper lies in their isotope-geochemical implications, *i.e.*, pointing to the presence of Archean crust.

As a consequence of the above, the Rb-Sr apparent age of 736 ± 9 for the Siwana and Jalor ring dike complexes should be still interpreted as the best approximaion of their intrusion age. Besides, this age is substantiated by identical age constraints for the Malani intrusive suite (Crawford and Compston, 1970) which document an important late Precambrian period of intraplate magmatic activity in Rajasthan.

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Comment (3)

Comment on the paper "**Occurrence of *Cochlichnus* Hitchcock in the Vindhyan Supergroup (Proterozoic) of Madhya Pradesh**" by K.G.Kulkarni and V.D.Borkar published in the Jour. Geol. Soc. India, v.47(6), 1996, pp. 725-729.

We have read with interest the above paper reporting "an unusual and peculiar" trace fossil found in Bhander sandstone. It appears that Kulkarni and Borkar (1996) are not aware that somewhat similar structures have been reported in the ripple marked quartzites of Delhi Supergroup as well by Singh and Bose (1985) and Das Gupta and Prasad (1995).

Even in the Vindhyans of Madhya Pradesh, vermiform structures are present in the Bhander sandstone of Bhopal area (one specimen can be seen at the entrance of GSI office at Bhopal). A close look at the photograph of ripple sandstone from Bhopal by Soni *et al.* (1987) shows the presence of vermiform structures in the ripple trough.

From the above it can be concluded that the vermiform structures in the Proterozoics of Rajasthan, Haryana, Madhya Pradesh and Karnataka are present in rocks having a wide geological interval. The assignment of "an undoubted Vendian age for Upper Bhander Sandstone Formation" by Kulkarni and Borkar is not justified.

The generic assignment of the specimen as *Cochlichnus* is itself questionable. The sinuous shaped structures, found in the ripple troughs of quartzites have earlier been named as *Manchuriophycus* (Rastogi and Srivastava, 1992). If we assign the Bhander specimen to this genus, the argument in favour of Vendian age of upper Bhander Formation will lose its sanctity.

At present, it is suffice to say that the creator of such structures existed for a very long time. Probably they thrived during the Delhi times but dwindled during Vindhyan times when a greater variety of life had started appearing.

The above discussion on sinuous structures from Proterozoic are based on the surmise that they are of organic origin. Nevertheless, the possibility of their being inorganic has not yet been ruled out completely.

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Reply

We thank Mathur and Ravindra Kumar for taking interest in the report of *Cochlichnus* from the Vindhyan Supergroup.

It would be incorrect to conjecture that Kulkarni and Borkar were 'not aware' of reports by Singh and Bose (1985), Das Gupta and Prasad (1995), etc., just because these were not cited. Various 'vermiform' structures described in these references from the Delhi, Vindhyan and Kaladgi Supergroups, according to Mathur and Ravindra Kumar, are 'somewhat' similar to the trace fossil described by us. However, such superficial similarity is in no way useful in identifying these sedimentary structures with *Cochlichnus anguineus.*

In our communication we have made it amply clear that every care was taken to rule out any possibility of the specimen being of inorganic origin, especially in view of the antiquity of the sediments in which it occurs. Following are the reasons for concluding that sinuous structure from Sagoni is of organic nature:

1) The trails have a uniform width throughout, without swelling or pinching anywhere (meaning that they are not spindle shaped).
2) Every individual trail is longer than any synaeresis cracks.
3) Sinuosity of each trail, presumably emplaced by different individuals of the creator, has a different pattern.
4) As against inorganic sedimentary structures which occur invariably over vast areas, the traces at Sagoni (and at adjacent villages) occur sporadically and are restricted to a few slabs.
5) Cross-section of the trails is 'U' shaped unlike inorganic vermiform structures which have a 'V' shaped cross-section.

Therefore, the authors have no hesitation in reiterating that the identified creator of trace as *Cochlichnus anguineus* as valid. A total lack of arthropod traces, complex burrow systems and body fossils is in favour of a Proterozoic age. In Precambrian sequences *Cochlichnus* from various basins is so far reported from Late Vendian strata and therefore, it is considered to represent an age not older than the Late Vendian.

Mathur and Ravindra Kumar have used the term 'vermiform structures' synonymous with *Cochlichnus.*

There is no point also in comparing C. *anguineus* from Sagoni with *Manchuriophycus* (Rastogi and Srivastava, 1992) because vermiform structures identified as *Manchuriophycus* are shrinkage cracks, and differ from *Cochlichnus* in : (i) having no uniform width, but being distinctly. spindle shaped, (ii) having polygonal pattern in place of sinuous, (iii) having a 'V' shaped cross-section in place of 'U' shaped, (iv) unlike *Manchuriophycus* (or even *Rhysonetron*) the trails at Sagoni neither branch nor coalesce.

The vermiform structures referred to by Mathur and Ravindra Kumar are of inorganic nature and we saw no necessity for citing them in our communication.

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