CORRESPONDENCE

AN ALTERNATE PERSPECTIVE ON THE OPENING AND CLOSING OF THE INTRACRATONIC PURANA BASINS IN PENINSULAR INDIA

Dr. Chalapathi Rao and Dr. Ashish Dongre have communicated that in the Discussion on the above paper published in JGSI, May 2015, pp.627-629, Prof. Kale has commented wrongly on their published papers. We are publishing here both their viewpoints - Editor

In his comment on the above paper, Prof. Vivek Kale makes a statement that “None of the papers (Dongre et al 2008; Chalapathi Rao et al. 2010– c.f. Basu and Bickford, 2015) has any petrological or isotopic evidence that demonstrates the comparison of the xenolithic limestone with either the Bhima or Kurnool Limestones” (sic). It appears to us that Prof Kale has not directly referred/read our papers on the Siddanpalli kimberlites published in various journals (Dongre et al. 2008, Journal of Geology, v.116, pp.184-191; Chalapathi Rao et al. 2010, International Journal of Earth Sciences, v.99, pp.1791-1804; Chalapathi Rao et al. 2010, Mineralogy and Petrology, v.98, pp.313-328). Had he done so, he would have found out that we indeed carried out a thorough petrographic, geochemical and stable isotopic (C and O) studies on these limestone xenoliths and attempted comparisons with such information available for the Bhima and Kurnool limestones. Even though our findings do not (cannot!) eventually prove that all of these limestones belong to the same horizon, given that the age of the Siddanpalli kimberite cluster is 1100 Ma (Anil Kumar et al. 2007, Precambrian Research, v.154(3), pp. 192-204; Chalapathi Rao et al. 2013, Chemical Geology, v.353, pp.48-64) field observation demands that the limestone xenoliths must be older necessitating their Mesoproterozoic age and thereby provides a circumstantial evidence (which cannot be discarded) until an alternate model comes up to better account the provenance of these limestone xenoliths. Prof Kale in this discussion further adds that ‘It is remarkable that the xenoliths have been reported from the Siddanpalli kimberlites (which are now exhumed) and yet have retained the characters of being ‘fresh limestones’. Subject to due corrections (in case I have missed the relevant literature), I find this a fallacious argument in the first place taking into account the petrology, eruptive /emplacement temperatures of kimberlites and the well-established responses of limestones to even slightest changes in temperature and pressure conditions’ (sic). It is true that a limestone is naturally expected to react strongly when it comes in contact with a hot kimberlite magma of high temperature (say 800°C) and we have reported that some of the limestone xenoliths from Siddanpalli were indeed recrystallised, upon such reaction, apart from extensive carbonatization of the olivines and groundmass phases in the host kimberlite. We have further documented that some portion of the limestone xenoliths were assimilated in the host kimberlite magma which is also reflected in the very high CaO content of Siddanpalli kimberlites- the highest than that known so far from the 70 odd kimberlites from the Eastern Dharwar craton. However, a portion of the xenoliths remained absolutely unaffected and found as ‘fresh limestones’ as is directly evident from their micritic nature with clayey intercalations under microscope and the laminations/banding visible even on a hand-specimen scale besides their geochemical attributes (lack of influence of kimberlite bulk-geochemistry). Such limestone xenoliths must have been incorporated into the kimberlite magma at a much shallower depths (close to the surface), thereby necessitating their survival and non-reactiveness, and certainly not ‘exhumed’ being entrapped in the kimberlite - as expressed by Prof Kale. We also wish to point out that the presence of ‘fresh limestones’ in a kimberlite is not a ‘remarkable’, aspect but in fact has been well-documented and quite familiar to those involved in research on kimberlites. For example, ‘fresh limestone xenoliths (some of them with profuse conodont fossils) have been recovered from the kimberlites occurring in the Slave (Crokenbo et al. 1998; Geology, v.26, n.5, pp. 391-394) and Saskatchewan (Lefebvre and Kurszlaukis, 2008; Journal of Volcanology and Geothermal Research v.174, pp. 171-185) cratons of Canada and constitute the only solid evidence that sea water inundated, in the geological past, in the now exposed cratons. Likewise, fossiliferous limestone xenoliths derived from the kimberlites of the Baffin Island, Nunavut, Canada (Zhant and Pell, 2014; Canadian Journal of Earth Sciences v.51, pp. 142-155) confirm the Paleozoic cover in this domain.

We do agree with Prof. Kale that despite more than a century of study many aspects of the Purana basins still remain open to debate. We also deeply appreciate the remarkable efforts made in recent years by Prof Abhijit Basu and his co-workers for reviving interest in these fascinating basins.

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Prof. Vivek Kale’s response:

Drs Chalapathi Rao and Dongre appear to have misunderstood the point I was making in my comment on the paper. I respectfully
also submit that their assumption that I may not have read the original papers is equally misplaced.

The intent of the specific paragraph (that has been highlighted by Drs. Chalapathi Rao and Dongre) was not to criticise any of their work on the Siddanpalli kimberlite, rather to point out limitation in the conclusion drawn by Basu and Bickford (2015) for the age of the Bhima Basin:

- Preferring to rely on an “inferred” correlation of “xenoliths” in a kimberlite, just because the age of the kimberlite is constrained by geochronological data; notwithstanding the fact that there is no physical connection with either the Bhima or Cuddapah basins;
- And questioning the multiple findings of biota by several groups that has been replicated over time.

Drs Chalapathi Rao and Dongre have pointed out in their comment that they have reported extensive and exhaustive characterisation of the xenoliths and CaO contamination in the kimberlite in their works. I completely agree with them on that account. Their papers cited bear testimony of the same. I also have no hesitation in re-stating my ignorance on kimberlite petrogenesis and in conceding to their expertise.

What I do disagree with is the claim to have ‘extensively compared’ the xenolith with the limestones in the Bhima and Cuddapah basins. There is not enough comparative evidence to ‘establish’ this, other than the ‘fresh’ (obviously meaning ‘unaltered and primary nature’) of the limestone xenolith. I had shared my reservations on the correlation of the limestone xenoliths from the Siddanpalli kimberlite with either the Bhima or Cuddapah (in a personal correspondence with Dr Chalapathi Rao); given that several Dharwarian belts (that are geographically much more closer to the Siddanpalli kimberlite) also have limestones beds, some of them also stromatolitic.

It is satisfying to note that there seems to be a general concurrence within all the authors to this discussion that although the Purana basins have been studied for more than a century now, it is only recently that we have started to really appreciate ‘what we do not know’ about them. Every new data, observations and interpretation about these unique set of basins is a welcome addition to science.

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