kote area, Jammu, occurs near the upper stratigraphic contact of the Subathu Formation and is dated Late Eocene in age[8]. Basaltic flows have been also reported from the lower levels of the Subathu Formation, Deoban window, from Peontra area, Himachal Pradesh[7]. Thus the rhyolitic breccia is of Late Paleocene age at Salal village, Jammu, occurring at the base of the Subathu Formation[2,9]. The ash bed from Kalka area[1] occurs within the overlying Early Eocene carbonaceous and coal-bearing beds of the Subathu while the ash bed at Sindkatauti locality, Kalakote, occurs at the highest level of the Subathu Formation[8] of Late Eocene age. Therefore, the Kalka ash bed neither represents any unique occurrence from the Subathu Formation, nor is it the oldest of such occurrences as have been claimed[1]. Further, naming them 'Basal Subathu Tonstein'[1] is thus invalid. As distinct from the 'tonstein' from Kalka area[1], the volcanic rocks so far studied from the HFB are mainly basaltic in composition with minor acidic components[4]. The major, trace and REE abundances of these basaltic rocks are of Continental Flood Basalt affinity and are not comparable to those reported from Kalka area 'tonstein'. This is due to difference in nature of parent volcanic rock and later devitrefication and alterations. High concentrations of Zr (515–735 ppm), Th (60–69 ppm) and Y (50–58 ppm)[1] indicate acid volcanic glass affinity, whereas moderate concentrations of Cr (166–185 ppm) and V (244–278 ppm)[1] are not compatible with acidic ash.

The authors state that the ash bed discovered by them has manifold importance. Since its stratigraphic position corresponds well with the India–Asia collision event, it is thus significant for its better understanding and is also a good proxy to understanding the nature of volcanism and the collision process[1]. The Eocene volcanism in HFB might have been caused by the thermal anomaly related to break-off of the Indian oceanic slab following India–Asia collision, and some deep faults in HFB[4,6,10].

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Taxonomic vandalism: The case of the giant wrinkled frog

A recent issue of *Current Science* carried a research communication, '*Nyctibatrachus karnatakaensis* nom. nov., a replacement name for the giant wrinkled frog from the Western Ghats' by Dinesh *et al.*[1]. Having read the communication, I am compelled to write the following comments in the interest of the readers of *Current Science* in general and amphibian taxonomists in particular.

The giant wrinkled frog was first collected by me in 1990 from a private estate, Neria in Karnataka. I had then referred the two specimens (5.0–8.0 cm SVL) first to the Zoological Survey of India, Chennai, where R. S. Pillai confirmed that it was not *Nyctibatrachus major*; the largest species in the genus known at that time. Pillai suggested that I compare it with yet another species *Nyctibatrachus humayuni* that was earlier described from northern Karnataka. When I did that, the specimens at hand so closely matched the published descriptions that it was identified as *N. humayuni* and a note published in *Hamadryad* on range extension of the species[2]. As I had no access to the types of *N. humayuni*, the Neria frogs were sent to the Bombay Natural History Society for comparison, where they were identified as *N. major* and deposited with appropriate registration numbers.

More than 10 years later, S. V. Krishnamurthy showed me photographs of the giant wrinkled frog that he had collected from the Kudremukh landscape. Since he had by then also made an attempt to describe the frog as a distinct species, I suggested that he submit the manuscript to *Current Science*. Ever since Krishnamurthy *et al.*[3] described and named the giant wrinkled frog as *N. hussaini*, there have been rumblings amongst fellow amphibian taxonomists. Various reasons were cited for disqualifying the validity of the species, and some like Das and Kunte[4] even suggested that the species be treated as invalid, mainly because there were no types deposited to authenticate the description.

In 2000, I reviewed a book on taxonomic procedures and guidelines for biologists. The review was published in *Current Science*[5]. The book by Judith Winston[6] has an exclusive chapter titled 'Common problems'. And according to her, the first and most common problem that taxonomists face is that of 'missing types'. She starts her discussion by saying, 'One of the problems you might have to deal with during background research for your project is what to do when you cannot locate the type material for a species that you need to study or compare. Perhaps, no type material was ever deposited in an institutional collection, or, although deposited, it has since been lost or destroyed'. Winston[6] goes further to describe the various circumstances when type material has been missing as follows: (1) Sometimes type material has been lost or damaged because a taxonomist did not make arrangements for the preservation of his/her private collection after death. (2) Types have been destroyed deliberately. (3) Museum collections have perished in floods and fires and especially during wars when museums were bombed.

According to Winston[6]: 'There are several ways to solve the problem of missing types. In some cases, descriptions and original illustrations are so indicative that there is no doubt about a species'

identity and no need to search for type material… For many species described in the eighteenth and early nineteenth century, no type material was preserved. Nevertheless, even some of those for which the name was based on an illustration alone, or an illustration plus an inadequate written description, may be identifiable from that original description…Anyone working on a species that resembles one in a questionable illustration or an unclear old description would do better to describe the species as new, making a note of the early name as a possibility. Should the original specimens someday be discovered, restudy might result in your name becoming a junior synonym, but at least the history of your name and decision would be clear.'

Going by Winston[6], there is little scope for the drastic step that Dinesh *et al.*[1] have taken. First, Krishnamurthy *et al.*[3] have provided good quality photographs to support their description. The common English name and a photograph and illustrations of the species have also been provided by me[2], with the following remarks: 'This species was first collected in 1990 from a private estate in Dakshina Kannada. The two adults obtained were wrongly identified as *N. major* and *N. humayuni*. It was only in the year 2001 that the species was described as a distinct species. This species apparently also occurs in northern Kerala and the northwestern hills of Tamil Nadu'. To invalidate the species due to lack of adequate supporting material[1] is therefore far from what the standard taxonomic procedures have recommended[6].

Second, there is evidence that the species is not endemic to Karnataka[7] and as such, to name it after the State is unwarranted. Third, Dinesh *et al.*[1] have accused the reviewer of Krishnamurthy *et al.*[3] saying, 'At the same time, the reviewers of the paper in the journal, as qualified taxonomists of amphibians, are also equally responsible for overlooking the error'. I do not know who reviewed the manuscript submitted by Krishnamurthy *et al.*[3]. But I do know that it was forwarded to *Current Science* by me and it was not published by oversight. It is also not clear as to what Dinesh *et al.*[1] mean by 'qualified taxonomists'. Finally, what is most shameful about the communication in focus[1] is that two of the authors had also co-authored the earlier paper[3]. It is unfortunate that Manjunatha Reddy and Gururaja are caught in a state of self-contradiction, unable to vouch for the species[3] that they had collected and described in 2001. Obviously, both have not had the necessary 'qualification' to understand and apply the standard taxonomic procedures while dealing with new descriptions. The fact that the senior authors of the publication under focus[1] are from the Zoological Survey of India, makes it appear all the more ironical unless, of course, the entire purpose is to vandalise an earlier taxonomic contribution. Thus in light of the above discussion and for all practical purposes, it remains that *N. hussaini* is the valid scientific name of the giant wrinkled frog. And instead of more appropriately redescribing a species based on a neotype, Dinesh *et al.*[1] have vainly created a junior synonym in *N. karnatakaensis*.

Serious efforts to conserve the endemic amphibians of the Western Ghats are being slowed down as many a young biologist is driven by a desire to collect (kill) and describe species. In this context, I wish to draw attention to a publication of mine[8] that Dinesh *et al.*[1] have cited. While it is critical that the correct identity of a species be established in conservation planning, 'recycling' of species names comes in the way as a treadmill. If the giant wrinkled frog can keep amphibian biologists on the treadmill for 17 years, it is hard to predict how long it will take before the 'treadmill syndrome' ends. The sooner it ends, the better will it be for the continued survival of more than a 100 species of precariously surviving amphibians in the Western Ghats.

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No reply was received from Dinesh et al.

—Editors

Formation of mini warm pool in the Arabian Sea

Deepa *et al.*[1] examine the plausible reasons for the formation of onset vortex in the presence of Arabian Sea mini warm pool (MWP) based on data for the period 2000–05. Deepa *et al.*[1] have concluded that the MWP was absent during 2000 and 2004, and all the descriptions were based on this assumption. Definitely, the statements ('It can be seen from Figure 2…', p. 796, para 4, line 1; 'MWP is absent in 2000 and 2004…', p. 798, para 3, last sentence; 'MWP was absent…', p. 798, para 7, last but one sentence; 'In the present study…', p. 799, para 3, first sentence; 'Absence of MWP…', p. 799, para 5, first sentence; 'Examination of weekly…', p. 800, para 2, first sentence) convey a message that the MWPs were not even formed in the Arabian Sea and were completely absent in those two years. In fact, MWPs (sea surface temperature (SST) > 30.5°C) were present in the southern Arabian Sea during both the years (i.e. during 2000 and 2004). However, they dissipated at an early date. In May 2000, an exclusive survey was made on-board *INS Sagardhwani* to study the