

occurs because the poor people are easily lured by monetary incentives for their participation in these trials.

My objection to this is: how can a drug, designed to target a Western population be tested for pharmacokinetics and adverse drug reactions in a genotypically different group of people? Are the developing countries only a soft target?

Inadequate representation of developing countries in various international journals and research agencies has been a major hurdle. Many good studies from developing countries face rejection. The same is true for the patenting policy; for instance over turmeric.

The doctor-patient ratio in India is 1:2460 as against 1:40 in developed countries². This leads to doctors being overworked in India. In tertiary care centres in India, doctors, especially residents, are known to continuously work for 48 h. With this time crunch, how can anybody do quality research? Hence an increase in the health personnel would automatically and instantaneously show up as quality research work.

The recent revolution world over for 'open access' aims to curb the policy of 'no subscription – no information access'. Talking economics, the annual per capita income of an average Indian is US\$ 530 (ref. 3) and that of a Britisher is US\$ 27,650 (ref. 4). The average annual subscription rates of a few well-known journals like *BMJ* and *JAMA* are US\$ 337 and US\$ 160.60 respectively. This quest for subscription could be partly satiated by means of Public Library of Science (PLOS), information portals, Open Access Initiatives, Budapest Open Access Initiative, Health internetworking Access to Research, etc.

Somehow, we in the developing countries have failed to take the research results to the common man, which journals like the *BMJ* have been successful at. The media too plays an important role to convey research directly pertinent to the common man. For example, surgical procedures like minimally invasive cardiac surgery and painless labour techniques literally dance on the tongues of the layman overseas. Once the common man is

interested in research results, so would be the government and hence funds would flow in.

Thus investment in research in developing countries should be regarded as one that would improve the health status in the near future. For further details one may refer to ref. 5.

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Taxonomic reasoning: The case of giant wrinkled frog

A recent issue of *Current Science* carried a correspondence entitled 'Taxonomic vandalism: The case of the giant wrinkled frog' by Ranjit Daniels¹. The author is critical of a communication authored by us.

Daniels, in his correspondence¹, has narrated a background history: a claim of having made the first collection of the giant wrinkled frog, from a private estate, Neria in Karnataka; his attempts to fix the true identity of the specimens, and in this context approaching R. S. Pillai (Zoological Survey of India (ZSI), Chennai), for clarification and confirmation, and identifying the specimens as *Nyctibatrachus humayuni*, since they 'so closely matched the published descriptions'. According to him, having no access to the types of *N. humayuni*, the specimens were later sent to the Bombay Natural History Society (BNHS), where they were identified as *N. major*, and deposited with appropriate registration numbers. Here, certain inferences are clear. Daniels' identification of the specimens as *N. humayuni* was errone-

ous, even after they 'so closely matched the published descriptions'. The true identity of the specimen(s), whether they were *N. humayuni* or *N. major*, then had remained uncertain.

We need not, in general, doubt or question the reliability of the so-called history of the specimens or the 'species', but, in this context, the record must have vast strength. Daniels claims to have provided the common English name for the frog, a photograph and illustrations of the species in a base paper³ published by him in 1992. A scrutiny of the paper reveals that there was no explicit reference to the name 'giant wrinkled frog'; it was mentioned as a 'large species of wrinkled frog', obviously due to its large size and the wrinkled skin-feature. Besides, there were no photographs and supportive illustrations in the base paper³. The question arises as to why he did not reveal this to Krishnamurthy *et al.*⁴ and stop them publishing their work in 2001. Again, Daniel's argument that he had no access to types of *N. humayuni*, is untenable. It

is self-contradictory, because Bhaduri and Kripalani⁵ had deposited seven specimens of *N. humayuni*, six in BNHS (Cat. nos 577, 775, 427, 428, 429 and 430) and one in ZSI, Kolkata (Cat. no. 20628).

In 2005, 15 years after his claim of having found the Neria frog for the first time, Daniels published a book exclusively on *Amphibians of Peninsular India*⁶, which was incidentally reviewed by Gururaja⁷. In this volume on amphibians, the author vaguely states that the species (in question) was wrongly identified as *N. major* and *N. humayuni*. Here also, Daniels has not provided details about specimen deposited, voucher number, etc. The obvious question again would be as to why the author had not provided the specific details of the frog specimens then, which he is mentioning now, in 2008?

It is intriguing to us why the facts were not suggested to Krishnamurthy (who showed him a good photograph along with a manuscript of this species in 2001), to look into the collections at BNHS, or to

consider his 1992 publication³ as a reference material. One noteworthy point is that the base paper³, is not even cited in Krishmurthy's 2001 publication⁴.

Daniels has also questioned the naming of *N. karnatakaensis* based on the distributional status of the species. Naming of the species need not be related to endemism. It is an appropriately subjective consideration of the authors. The etymology in our paper² states that it was named in honour of the state of Karnataka, wherein the type locality, Kudremukh National Park, is situated. This species is known to be endemic to Karnataka, since there is no other reported incidence of its occurrence from elsewhere. Daniels has only presumed the distribution of the species extending to Kerala and Tamil Nadu⁶.

In order to endorse his views and points, Daniels¹ quotes Winston's work on taxonomy and his own review of that work. We intend to stress here that it is not in the perspective of Winston's work that our study result has been published², but based on the principles described in the International Code for Zoological Nomenclature (ICZN, 1999 with effect from 1 January 2000)⁸. The Code⁸ specifies that a species described after 1999 must have an explicit fixation of deposition in public collections of name-bearing types in accordance with Article 16.4, 72.3 and sub clause therein. We add that the name *N. hussaini* is invalid, not the species described under that unavailable name, because of the above-cited reason.

Its species validity having already been realized owing to its definitive description, we replaced its invalid name with *N. karnatakaensis*, with a type designate, depositing a specimen following the Code. This was the root cause of the taxonomic debate.

A technical debate was raised in 2001 by Biju⁹, not including *N. hussaini* in his publication, since it required re-evaluation. This point was emphasized by Das and Kunte¹⁰ in 2005, citing the ICZN norms⁸. In fact, this spurred the authors to rectify the procedural lapses, ultimately leading to the 2007 publication².

Correcting a discrepancy in one's taxonomic work is a necessity and is part of updating the knowledge on any taxonomic case or issue in question. That is what makes taxonomy a dynamic science. No taxonomist would think that it is an accusation when a flaw in his work is pointed out and corrected. In the present case², the taxonomists who described *N. hussaini* themselves have worked with mutual understanding, to set right the flaw that has crept in, and make it taxonomically a standard one.

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Thickness estimation of Deccan Flood basalts of the Koyna area, Maharashtra (India) and implications for recurring seismic activity

Naik *et al.*¹ have evinced interest in our paper which deals with the structure and seismicity of the Koyna region². However, they seem to have missed the message of the paper. The comments, when viewed in proper perspective appear totally unreasonable and show their lack of knowledge about recent findings as well as current thinking on Deccan traps based on geophysical studies. On this subject, several papers are available, including ours, which can be accessed through the internet. However, since Deccan traps have become an important topic

due to occurrences of large damaging earthquakes in the recent past like the Koyna (1967), Latur (1993) and Bhuj (2001), issues such as those raised by Naik *et al.*¹ need to be addressed in the broader interest of the general scientific community.

Naik *et al.*¹ often cite 4–5 decade old Geological Survey/Central Ground Water Board reports, which are normally inaccessible to readers. However, a detailed published work is now available on the nature of Deccan trap lavas and its underlying granitic–gneissic basement³. The

work describes full-fledged lithologic characteristics of vertical Deccan trap sequences as well as underlying crystalline basement penetrated by a 617 m deep borehole (KLR-1), drilled in the Killari–Latur seismicogenic region of Maharashtra (Figure 1). It was drilled by courtesy of the National Geophysical Research Institute (NGRI), Hyderabad and Atomic Minerals Directorate (AMD), Hyderabad, specifically to understand subsurface structure of the basement and possible reason for the occurrence of the Latur earthquake⁴. This unique borehole, located