



Figure 1. Closing the digital divide.

Table 1. Developing countries with the same starting point as developed countries

Country	Internet users per 100 inhabitants in 1999	Internet users per 100 inhabitants in 2005
Thailand	2.14	11.03
Barbados	2.24	14.07
Brazil	2.04	17.24
Panama	2.19	6.39
Turkey	2.23	15.31
	Average = 2.17	Average = 12.8

Source: ITU (ICT eye tables).

ing countries with levels close to 4.1 in 1999 is compared with developed countries at the same level.) The question then becomes as to why the latter grew so fast in relation to the former, rather than why the former have been catching up so rapidly with the latter. It is not a question that has to be posed for each and every latecomer, however. Perhaps

the most telling exception is the case of Korea (a latecomer), which I now briefly review.

The number of Internet users in Korea amounted to 1.6 million in 1997 (3.7% of the population). By 2001, according to most estimates<sup>4</sup>, the number of users had reached a figure of almost 25 million (or 56% of the population). Now, looking

again at Figure 1, it appears that Internet use of 3.7% was achieved in the developed countries between 1994 and 1995, so that subsequent growth in those countries can be compared with what occurred in Korea. The question, in particular, is how long it took the developed countries (from 1994/95) to reach a level of Internet use equal to 56% (the Korean case). And the answer is that this level had not been reached even by 2004, the end of the ten-year period covered in Figure 1. By 2001, in fact, developed countries had reached only 36.3% of the population, indicating just how exceptional the Korean case really was.

In conclusion, writings on the digital divide focus on the growth rates of rich and poor countries with different initial conditions and more specifically on how much faster the latter are growing compared with the former ('the closing divide'). When one corrects for the different starting points, however, developing countries seem to have been growing between two to three times more slowly than the developed countries, though one latecomer country, Korea, stands out as a striking exception to this general pattern.

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## New frogs from the Western Ghats

Criticism to scientific papers should be fertile and should stimulate the researchers to make better future products. Unfortunately the comments by Vasudevan *et al.*<sup>1</sup> to our taxonomic work of frogs<sup>2</sup> (and to those by Biju and Bossuy<sup>3,4</sup>) are

far from fertile and rather disappointing. Most of their arguments are based on misunderstanding, groundless doubt, and excessive demands which are practically impossible to fulfil due to the serious lack of comparable data. Their comments

do not contain positive suggestions which will serve for the progress of frog taxonomy.

First, they criticized that we 'distinguish *Philautus luteolus* from all other known *Philautus* based on colouration

and markings only from males<sup>1</sup>. And because we admitted that 'variation seems extensive regarding size, ratio of body parts, colouration, and so on', they stressed the need 'to accumulate precise data especially on live specimens such as colouration and acoustic features'. In reality, we admitted extensive variation within the genus *Philautus* in general, not within *P. luteolus*. Boulenger<sup>5</sup> described colour variation in *P. variabilis* as 'indefinite'. This situation may be exceptional and probably much exaggerated. Even if the variation is extensive, variation in a given species is usually limited within a range, and we recognized that the range of colour variation in this species was rather narrow, exemplifying two colour morphs of live *P. luteolus* that we observed. Because the colouration of live specimens is the most popular and outstanding feature of animals, to cite it as one of the diagnostic features is not blameable at all. To distinguish *P. luteolus* from *P. travancoricus*, which may have similar live colouration, we provided some morphological differences other than colouration<sup>2</sup>. Generally, to collect females is difficult in *Philautus*. Therefore, it is also not blameable to describe new species based on only male specimens.

Vasudevan *et al.*<sup>1</sup> questioned whether our acoustic data for *P. luteolus*, previously described as those for *P. cf. travancoricus*<sup>6</sup> were based on the types of *P. luteolus* or not. Why do they doubt this? In describing acoustic features, it is indispensable to determine which individual actually emitted the calls recorded. Naturally we got the specimen from which the call was recorded and, because we could not identify it exactly at that time, we tentatively gave the name *P. cf. travancoricus*. The call structure recorded from the holotype specimen was identical with that already reported by us<sup>6</sup>. Hence we simply mentioned it in our paper. If we described call structures again in our descriptive paper<sup>2</sup>, we would be criticized for repeating the same results in two different papers. The accession numbers for 12S and 16S rRNA genes of *P. luteolus* cited by Vasudevan *et al.*<sup>1</sup> are from our own data<sup>7</sup>. They have criticized that no attempts were made to validate the species status using these data, but these molecular data were obtained after the publication of our descriptive paper<sup>2</sup> and there were practically no other data to be compared with. How

could we validate the species using molecular data under these circumstances?

Comparisons with type specimens of the other species are important, of course. However, comparisons with all extant-type specimens are not only impossible, but also unnecessary. Only those type specimens which seem difficult to distinguish from the specimens at hand should be examined. Vasudevan *et al.*<sup>1</sup> claim that *P. tinniensi* should be incorporated in comparisons because *Ixalus montanus* (now synonymized with *P. tinniensi*) was described from Kudremukh, the type locality of *P. tuberothumerus* (not the type locality of *P. luteolus*), not because it is morphologically similar to our new species. The type locality of *P. tinniensi* (neotype) is Nilgiri Hills, and according to the original description<sup>8</sup> it has yellow-reddish, sometimes blackish dorsum and yellow inner finger. These descriptions and the photograph of the neotype<sup>9</sup> clearly exclude this species from detailed comparisons. Two nominal taxa, *I. montanus* and *P. melanensis* distinctly differ from *P. tuberothumerus* in size and colouration. The type specimen of *P. melanensis* has been lost.

After enumerating the 'defects' of our papers, Vasudevan *et al.*<sup>1</sup> have given concluding remarks which cannot be overlooked. Their conclusion is that 'any new data on *Philautus* should be used to revise the group and only after rigorous examination of all available names, new species descriptions should be published'. Without doubt, this statement expresses completely negative attitude and only serves to retard the progress in *Philautus* taxonomy. As to the first part of this statement, how shall we do it if the data indicate the existence of a new taxon? The word 'revise' does not involve description of a new taxon. From the context, it is apparent that the 'rigorous examination of all available names' means a revision work on *Philautus*. We do not deny the importance of the rigorous examination of all available names, but who is to do the examination. When will this work finish? Who judges the completeness or validity of the resultant revision work? Even if a voluminous revision work is published, there would be many defects or objections because such an examination (judgement in essence) always depends on the reviser's philosophy or subjectivity. In fact, Vasudevan *et al.*<sup>1</sup> brought forward an objection to

the elaborative revision work of the genus *Philautus* just published<sup>9</sup>. At any rate, nobody can suppress the researcher's publication activity. On reading their concluding remarks, we felt as if they had issued a ban.

The publication activity of researchers should not be discouraged by emphasizing or expecting the completeness of the data. We suppose that many of the Western Ghats frogs have rather limited distribution range, especially small frogs with limited dispersion abilities such as *Philautus*. That is, the speciation processes are proceeding actively and locally in the Western Ghats. Therefore, it is without doubt that many *Philautus* species (and in many other genera as well) remain to be described as new from various localities all over the Western Ghats. A rich amphibian biodiversity of the Western Ghats is probably still underestimated. From Boulenger's age to the end of the 20th century, the status of the genus *Philautus* in the Western Ghats had long remained unchanged not in the sense of Vasudevan *et al.*<sup>1</sup>, but in the sense that only one valid species has been added to the genus. Many authors should actively publish their own new findings, and this is the only way leading to the progress in the field of frog taxonomy. We appreciate the taxonomic works of Biju and Bossyut<sup>3,4,10</sup>, who are positively wrestling with this difficult but attractive group of frogs.

We believe that the acoustic and molecular data are essential to resolve the present confusing status of *Philautus* taxonomy, but only a few such data are available for comparison. Therefore, we are now accumulating the data with an expectation that these will play an important role in future taxonomic studies. The effects of these data may not become obvious at first, as our works on *P. luteolus* were criticized; but undoubtedly they will overcome the weakness of taxonomy based solely on external morphology. The sibling species, probably involved in *Philautus*, can be revealed only by these non-morphological features. Preserved specimens classified by these non-morphological characters should be compared carefully with available type specimens. We believe that only the researches with these procedures produce fertile results on the taxonomic revision of the genus and also clarify the problematical relationships between Indian and Sri Lankan *Philautus*.

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### Response:

It is unfortunate to see the response of Kuromoto and Joshy to our critique of the description of a new species of *Philautus* from the Western Ghats. Instead of taking the issues raised by us as a valid criticism, they contest the same with frivolous arguments. Interestingly, they rebut that our 'arguments were based on misunderstanding, groundless doubt, and excessive demands, which are practically impossible to fulfil due to the serious lack of comparable data', but end up providing new information in their response in support of the species described by them. This clearly establishes that there were inadequacies in their earlier descriptions of the new species and that our comments were not based on groundless doubts. Demands on scientific rigour by peers are only to enhance the quality of scientific output. This becomes more important when dealing with tough problems, such as the

complex anuran taxonomy, where there is a real need of high standards of reporting with reliable and accurate data. Through our comments<sup>1</sup> we hoped that in future, reviewers, authors and readers will exercise more caution and care while assessing new frog species descriptions; and definitely not to discourage the future studies as implied by Joshy and Kuramoto in their response.

Kuromoto and Joshy provide additional information on the validity of the species, which should have rightfully found its place in the original descriptions of *P. luteolus* and *P. tuberosus*<sup>2</sup>. Even this new information is full of ambiguity. It is mentioned that 'the range of colour variation in *P. luteolus* was rather narrow', exemplifying two colour morphs. Such a judgment can only be made if a number of individuals have been examined, but again there is no mention of how many individuals were examined to arrive at this finding. They provide a clarification that the calls were described based on the specimens that were later assigned as types of *P. luteolus*. If this information had been incorporated in the description itself, it would have avoided some of the concerns in our commentary. Similarly, their argument that 'practically there were no other molecular data available for comparison' is rather preposterous, because a number of sequences existed in the NCBI GenBank database, which has now grown to >300 (GenBank release 158 <http://www.ncbi.nlm.nih.gov>) for *Philautus* alone.

Further, they rebut that comparisons with all extant-type specimens are not only impossible but also unnecessary, which only reinforces our concerns about the poor-quality control in reporting and eagerness on the parts of authors to publish even partly validated inferences. Such mindset only weakens the cause of taxonomy, and rather gives an impression that it is an esoteric subject pursued by impulsive biologists. We have suggested methods that bring robustness to taxonomic work, wherein any new species once described, can be verified unambiguously by other investigators.

While we agree that researchers dealing with the taxonomy of *Philautus* should be encouraged to publish, their mistakes should also be pointed out, so that they refrain from erroneous and inaccurate reporting. It is a well-known fact that there are more undescribed species than described

ones<sup>3,4</sup>. Therefore, it is not surprising that many frogs from the Western Ghats are being described. But we are of the strong opinion that progress in *Philautus* taxonomy should not be merely measured as the number of new species described, but measured as the sum of information that has been collected, the number of young biologists inducted to deal with the task, the number of successful collaborations, the number of commentaries and finally the number of species described. These are the thoughts that motivated us to write the critique, and we sincerely hope that it would help strengthen the taxonomy of the group.

Kuromoto and Joshy question, 'When will the work finish? Who judges the completeness or validity of the revision work?' At this point in time it is not possible for us to comment on how long it would take to finish the work for a comprehensive review, but we can certainly say that this task would take much longer if erroneous and incomplete descriptions continue to trickle in the group. Further, to our understanding, the judge(s) of completeness or validity of the revision work should first be the authors themselves, followed by peers who review the manuscripts, and then the final adjudicators will be the researchers who would use the descriptions. Lastly, in our perception, even our critique of species descriptions and this present debate is indeed an indication of the progress in *Philautus* taxonomy.

1. Vasudevan, K., Chaitra, M. S. and Aggarwal, R. K., *Curr. Sci.*, 2007, **92**, 281–282.
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