

historical background to the astronomical material the book deals with and I have included them to make the book more accessible to the lay reader. If anything, I feel these have enriched the book. The reviewer has rather maliciously picked out the caption below Meghnad Saha's photograph (p. 39) to suggest that I have only made a superficial remark on Saha's work, while actually the book contains more than two pages of description of it and of its impact on modern astrophysics. The reviewer says that the book appears to be a world astronomy encyclopaedia and I do not see any reason to call it so, since it covers only the portion of astronomical research and discoveries that directly relate to Bappu's work.

Barring a couple of opening paragraphs, most of the review is an essay on Bappu's life and does not contain any additional material that is not already in my book. Any reader of this review gets the impression that these aspects of Bappu's life are not covered in the book at all. It is also liberally peppered with gossip and much of the interpretation is the reviewer's own. I dare say the book has hardly been reviewed. Much of what is written is, by the reviewer's own admission, based on personal conversations he had with Vainu Bappu during the period 1974–1982 and none of it is verifiable. Many others who knew Bappu as well may claim that some of the things written by the reviewer are not quite true. As an example, the reviewer says 'At the time of his arrival in Harvard, Bappu had had no real experience in speaking English and not very much background in astronomy'. But as I point out, Bappu had read a great deal of English poetry as a student in India and his exposure to

astronomy had begun when he was a child. His first astronomical paper, one on the variable stars in the constellation of Eridanus, appeared in 1946 much before he arrived in Harvard. The reviewer disagrees with my description of how the encounter between Harlow Shapley and Bappu took place in 1947, but what I have written is based on Yemuna Bappu's recollections. So, it is her version against Kochhar's. 'Secretary' here does not mean what the reviewer has in mind – a bureaucrat. Harlow Shapley was an honoured guest and the hosts might have had assigned a person to attend to his requirements. Similarly, the statement that 'in November 1985, when the 10-day General Assembly of the International Astronomical Union was being held in New Delhi, Menon brought the news that Prime Minister Rajiv Gandhi, along with his children would like to visit Kavalur to observe Halley's comet', is not quite right. The Prime Minister's wish was communicated to Bhattacharyya by a telex, when the latter was attending an IAU colloquium on hydrogen-deficient stars in Mysore that preceded the IAU General Assembly. Mallik was present in Mysore and saw the telex when it arrived late in the afternoon several days before the General Assembly was to commence. Nowhere have I claimed that on my visit to Kavalur in April 2010, I stayed in a room in the Vainu Bappu Telescope building as the reviewer has accused me of. On page 7 of the book, I wrote: We returned to our rooms late in the night. Handing over the key to my room, Mallik, said that it is a special room. I asked him what is special about it. He said, 'This was Bappu's room. He stayed here during his visits to

Kavalur'. During his time the room was only used by him. It is the same room where Rajiv Gandhi spent a night in 1986. I consider much of the criticism as nitpicking and has been made deliberately to belittle the work.

The most hilarious part of the review is the statement that Bappu once 'hid himself in the solar telescope tunnel' to avoid meeting with the well-known astrologer B. V. Raman, who was visiting Kodaikanal. What inspired the reviewer to make this statement is the title 'Stellar spectroscopy: the horoscope of stars' I chose for a chapter. The reviewer appears to have missed the pun here.

If Kochhar were so very particular about spellings and names, why is it that in his review Suri Bhagavantham's name has been consistently misspelt and the National Institute of Sciences of India has been called National Institute of Science?

Finally, I suggest that since the reviewer seems to know so much more about Vainu Bappu and perhaps possesses the correct perspective from which a definitive biography of Bappu should be written, he must write one and add to the growing collection of landmark biographies of eminent Indian scientists.

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## Science in the doghouse

In their recently published paper, entitled 'A dog's day with humans – time activity budget of free-ranging dogs in India', Majumder *et al.*<sup>1</sup> claim that free-ranging dogs in Indian cities are 'generally lazy and friendly animals', spending most of their time 'either sleeping, lazing or sitting'. Using results from behavioural observations of free-ranging dogs, the authors claim that perceptions of free-ranging dogs in India as 'noisy and

aggressive creatures' are biased, and that in fact dogs do not pose significant threats to human well-being.

We feel the need to write this letter for multiple reasons. Not only is this an example of a poorly conducted study, but major problems in almost every section of the paper raise substantial doubts about the veracity of their conclusions, which can have serious consequences. Free-ranging domestic dogs are not

unique to India, and are considered to be a public health issue, a financial drain on municipal authorities and shelters, and an animal welfare concern all over the world<sup>2</sup>. With complete nonchalance, the authors ignore these assessments. Instead, the highly biased and unsubstantiated conclusions from their preliminary study trivialises the issue and provides a highly misleading headline grabber for uninformed activists to further polarize

this debate. If we are not careful, this has massive implications for public health, animal welfare and wildlife conservation.

The paper is based on shaky grounds and problems begin at the introduction. The authors state that ‘Though dogs in India have lived outside of human homes for centuries ..., they have not undergone the usual domestication process to become exclusively pets as in most developed countries’. Are the authors suggesting here that the so called ‘Indian native’ dog is somehow evolutionarily distinct from other dog types in that they are not completely domesticated? Surely then, efforts by numerous animal welfare agencies to encourage adoption of ‘Indian native’ dogs should be viewed with extreme caution. The authors continue by claiming that dogs are ‘integral’ to the environment, but do not explain how and why, and even seem to suggest that streets are the ‘natural habitat’ for dogs. These bold opinions are not substantiated by published scientific data.

Ironically, the authors actually take strongly supported data and relegate those to mere opinions instead. The authors suggest that dogs ‘... are often considered as a menace by many people, as dirty animals that bark, bite and spread rabies. These notions are often founded on personal biases and little scientific data exist to either support or refute such claims.’ The facts that dogs indeed bark, bite and spread rabies are not notions, but realities of life in India<sup>2-4</sup>. A bias can only exist in the perception of the severity of these impacts, and the choice of epithets such as ‘dirty animals’. By calling these life-threatening facts notional, it is obvious where the bias of the authors lies. The authors further make the contention that ‘Though humans are generally tolerant of dogs, dog-human conflict is not uncommon, and a part of the human population in India is regularly affected by dog bites.’ Rather than simply being a nuisance, they neglect to elaborate that the ‘part’ of the human population that they are referring to is an estimated 17–20 million Indians/year that suffer from dog bites<sup>3</sup>. Tragically, this results in a person dying from dog-contracted rabies every 30 min<sup>3,5,6</sup>. Most researchers agree that even this high number is likely to be a severe underestimate, as rabies is not a reportable disease<sup>3-5</sup> in India.

Irrespective of this biased introduction, the stated purpose of Majumder *et*

*al.*<sup>1</sup> was to understand the behaviour of free-ranging dogs so that dog-human conflict may be mitigated. This is a completely valid objective. However, a closer look at the choice of study area and the study design reveals serious flaws that hamper strong inference. Their study was conducted exclusively in the campuses of two educational institutions situated in large metropolises and in a suburban township. It is hard to see how such sheltered locations are representative of the rest of India. Perhaps the authors failed to recognize that, as reported by several studies<sup>3,7,8</sup>, majority of dog bite cases originate in rural areas and more specifically impact poor and low-income households (80%). Even assuming that the aim of their study was to target urban areas, the study sites fall short of being representative. The two university campus locations are atypical in density, composition and socio-economic profile of the human populace, compared to neighbouring areas immediately adjacent to them.

Even ignoring the limitations in the choice of study areas, the convenience-based sampling methodology adopted by the authors severely limits the scope of interpretations. They conducted their observations of dogs during daylight hours when it was relatively easy to see them. Their contention is that this is the time when both dogs and humans are active. Several studies, however, have shown that free-ranging dogs are most active during crepuscular hours and at night<sup>9-12</sup>. Human activity does not end with dusk either. Exclusive recording of diurnal activity not only skews the estimate of the frequency of interactions, but the types of interactions that are likely to occur as well. The vague, incorrect and non-random methods continue with the choice of roads to sample, potential differences in sampling effort across sites, and even the type of sampling method used (instantaneous, instead of focal animal observation). We will not belabour our criticism with a detailed discussion of the serious flaws in the treatment of the data and the statistical analyses. Even high-school science students can clearly see that the behaviour of animals is likely to be influenced by others in the group, and pseudo-replication in space and time overestimates responses. The 1941 sightings and even the 1308 dogs that were aged and sexed cannot possibly be distinct individuals (else these would

be frighteningly high densities of dogs for such small areas). Thus, these results fail to meet basic statistical assumptions of independent data and one cannot draw robust conclusions from their analyses.

Given the numerous flawed assumptions, methodologies and analyses, it is not surprising that the authors come to conclusions that are incorrect. Most seriously, they conclude that dogs are not aggressive because they did not observe any aggressive interactions with humans. If that were indeed so, how do the authors explain the 17–20 million dog bite-related cases which include thousands of children, mostly from lower income rural families, who are grievously injured (sometimes fatally) every year<sup>4,7</sup>. Instead, the authors try to dismiss these ghastly realities using wordplay such as ‘occasional dog-human conflict’. They admit that dog-bite-induced rabies is a serious problem in India, but suggest that human-dog conflict should also be studied from the dog’s perspective. Is the human perspective insufficient to conclude that rabies is a fatal threat to both dogs and humans?

There are many other conclusions of the flawed sampling, some almost bordering on the comical. For example, a lack of vocalizing recorded during their diurnal sampling leads them to state that it is simply the inherent bias of *other people* that perpetuates the idea that dogs are noisy. We wonder if the authors would have concluded differently, had they sampled at night? Even assuming that dogs only bark for 3% of their time during the day<sup>1</sup>, at an estimated 59 million dogs in India<sup>13</sup>, that still amounts to a lot of barking.

Among the less egregious faults of this paper, the use of anthropogenically loaded language such as ‘friendly’ and ‘lazy’ to describe dog behaviour, stands out as clear example of the agenda of the authors. As domesticates and commensals of humans, dogs have surrendered their hierarchical status to humans. This makes them submissive. Furthermore, acquisition of food from humans requires extreme submissive behaviour, and can hardly be described as ‘friendly’. As carnivores, dogs are expected to spend a large amount of time resting – this does not make them ‘lazy’. By the same anthropogenic extension, would the authors readily conclude that a cow that chews cud all day long is hard-working?

Let us look instead at the reality of free-ranging dogs on India's streets. At an estimated 59 million, India has the highest population of dogs in the world<sup>13</sup>. At least Rs 2 billion/year is spent in the treatment of dog bite-related cases in India, with an associated loss of 38 million man-hours<sup>7,14</sup>. Add to that the costs associated with the Animal Birth Control (ABC) programmes countrywide, which are difficult to systematically quantify. The Animal Welfare Board of India alone doles out grants of approximately Rs 35 million every year to a handful of animal welfare organizations for performing ABC (<http://www.awbi.org/?q=node/60>, accessed on 15 April 2014). To be even more complete, we should also add the loss of human lives or the economic costs due to accidents involving street dogs, but these are rarely quantified. Even if one is emotionally ready to agree with the authors that the 'solution to dog-human conflict is not culling, but efficient management of garbage and rabies in the country, and a positive attitude towards the animals that are otherwise known to be man's best friend', we do not see how they have come to the conclusion that the 'general perception of these dogs as a nuisance is quite flawed'<sup>1</sup>. To do justice both to science and to the fate of millions of dogs, we wish the authors<sup>1</sup> showed more diligence in conducting their study. Instead of dispelling myths and scientifically contributing to a better understanding of dog-human relationships in India, they, ironically, perpetuate their own inherent biases.

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### *Response: Science threatened by subjectivity*

The comments on our recently published paper by Vanak *et al.* have left us surprised, disappointed and somewhat amused. Vanak *et al.* state that they felt the need to write their letter because they found major faults with all the sections of our paper, and also because this has 'massive implications for public health, animal welfare and wildlife conservation'. It is heartening to know that our study can have such deep and far reaching implications, and we thank Vanak *et al.* for bringing this to the focus. How-

ever, we would like to discuss some major flaws in their arguments put forth against our results.

Vanak *et al.* have picked a sentence from the introduction of our paper to ask whether we suggest that the 'Indian Native dog' is somehow special in its evolutionary history. A discerning reader would realize that this is a misrepresentation of the paragraph in which we provide a brief introduction to the free-ranging dogs in India, and state that these dogs have lived in close human proximity, but not as pets for centuries in this part of the world. Stating that the Indian native dog has not undergone 'the usual domestication process to become exclusively pets as in most developed countries' does not mean that the Indian free-ranging dogs are 'somehow evolutionarily distinct from other dog types'. We have simply stated that these dogs are not 'exclusively pets', and have no intentions of alluding to the evolutionary process of dogs here.

Vanak *et al.* cite some work on rabies to state that dog bites are a real threat to the Indian population. In trying to establish that we have a bias for dogs, Vanak *et al.* claim, 'they neglect to elaborate that the "part" of the human population that they are referring to is an estimated 17–20 million Indians/year that suffer from dog bites', citing Sudarshan *et al.*<sup>1</sup>. We have also referred to this very paper, which states that 'The annual incidence of human rabies was estimated to be 17,137 (95% CI 14,109–20,165). Based on expert group advice, an additional 20% was added to this to include paralytic/atypical forms of rabies, providing an estimate of 20,565 or about 2 per 100,000 population'. Sudarshan *et al.*<sup>1</sup> further state that 'The new estimate of about 20,000 (or 2 per 100,000 population) annual human rabies incidence based on this community survey shows a decline of about 30% from the earlier incidence of 30,000 (3 per 100,000 population) reported during the period 1990–2002'.

It is alarming and at the same time depressing to see such blatant misrepresentation of data. Vanak *et al.* have conveniently converted 17,137 to 17 million, and this increase by three orders of magnitude cannot be a typographical error. Though we acknowledge the fact that rabies is a serious problem in our country, we chose to present our results honestly, and without bias either for or