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Correspondence

Misrepresenting data : Deception or dogma?

Sir,

The editorial by Lone & Puliyel[1] misrepresents facts that have repeatedly been presented to national consultative bodies and the Indian Council of Medical Research (ICMR) in the past at forums where one of the authors was present. The data generated by ICMR sponsored HIB Probe preparatory phase[2] from Vellore describes a hospitalized pneumonia rate of 3.07/100 child years for children between 4 wk and 23 months and not "all cause pneumonia".

The authors also incorrectly conclude that the rate of all cause pneumonia in India is 30 per 1000 under-five children, which translates to 3.9 million pneumonia each year in this country, an over-estimate resulting from extrapolation of under two year rates to an under five population.

While the case fatality rate for hospitalized pneumonia was 0.7 per cent, the study which closely followed 1717 children from 4 wk of age to 23 completed months could not have made any claims about the mortality rate in under-five age group.

Lone and Puliyel seem to dismiss the need for vaccination against pneumonia and meningitis referring to data from the Hib probe preparatory study[2] suggesting that it demonstrates that pneumonia does not kill children in India. These children were visited each fortnight by field workers, provided free ambulance services at a phone call and free priority care at a premier healthcare institution. Hence, the case fatality ratio in this population reflects residual mortality after quality healthcare and cannot be extrapolated to the rest of India, where lack of timely access to care is the primary determinant of pneumonia related mortality. Studies in India where all deaths in a representative sample of the population were followed up with verbal autopsy showed that 22 per cent of under 5 deaths are caused by pneumonia[3]. It might help the authors to visit or work with those who look after children in remote and deprived regions of our country to recognize that children do die of pneumonia, before attempting to delay the access to these efficacious vaccines to children who need them most.

If the data from the Hib study[2] at Vellore did not form the centre piece for generating national policy, it was with good reason. Literature suggests that burden of Hib meningitis is 50 to 100/100,000 children in the first two years of life[4,5]. Even at the 100/100,000 incidence, prospective data from 1717 children would provide a very imprecise estimate of burden of disease depending on identifying one case of Hib meningitis from the cohort. Large cohorts of hundreds of thousands of children monitored for meningitis would take over a decade to yield the data, even if we were to start today. Unfortunately, the capacity for laboratory diagnosis of such fastidious organisms as *Haemophilus influenzae* and the surveillance network required for this scale of field epidemiology is not readily available in large parts of the country.

As the authors acknowledge, the expert panel considered all available information including the Hib probe study. However, unlike the authors, the experts considered the information from the cohort dispassionately and in its entirety. Lone & Puliyel fail to highlight the fact that Hib was the most common pathogen isolated from cases of meningitis (45%) in the hospital study at Vellore. They fail to refer to the 47 deaths from the block on which verbal autopsy was performed where 11 were due to respiratory causes and 2 due to meningitis as was discussed at an expert committee where Dr Puliyel was a member[6].

Medical practitioners, hard pressed for time, attempt to update their understanding of emerging issues often relying on journals' editorials for a balanced opinion. Such selective misinterpretation of data makes us wonder if the authors are too prejudiced to present a balanced view that an editorial demands. What is our responsibility as health care providers? Is misrepresenting

data ethical? Should the lack of home grown evidence, or the speculation that the incidence might be lower be a reason for depriving children of vaccines that are safe and efficacious? If India is truly different from the world in its burden of Hib, would the proponents of such theory provide us conclusive evidence that will stand scientific scrutiny?

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Authors' response

We thank Drs Jacob John, Anuradha Bose and Vinohar Balraj for taking time to respond to our Editorial[1] and we welcome this opportunity to clarify matters. In the first paragraph they say we have underestimated the problem of Hib but in the second paragraph they complain we have overestimated. However, the thrust of their argument seems to be that we must disregard data from Asia and instead base the national immunization programme on incidence of the disease in other regions. They ask, "Should the lack of home grown evidence or speculation that the incidence might be lower be reason for depriving children of vaccines that are safe and efficacious? If India is truly different from the world, would the proponents of such theory provide us conclusive evidence that will stand scientific scrutiny?"

This challenge in the last line is intriguing. As a nation we have spent 10 years doing exorbitantly expensive studies on Hib, funded by various international agencies. The IBIS study[2], the community study of Hib meningitis[3] and the ICMR multi-center study of pneumonia and meningitis[4] were all reported in our editorial. Many of these studies have included the Christian Medical College, Vellore (where the correspondents work,) as a study center. We also have data from probe[5] and probe-like studies[6] from other Asian countries. Our editorial has dealt with these too, in detail. Most of the studies (in Asia and India) were done in an effort to contradict the 'low incidence of Hib in Asia' concept. Unfortunately all systematic studies have only further confirmed the fact of low incidence. Finally we have the spectacle of the WHO resorting to misrepresentation of facts, to suggest Hib vaccine is useful and needed in Asia[7,8]. We will deal with each of the issues raised by the correspondents.

Regarding the exaggeration of pneumonia numbers discussed in paragraph 2 of their letter: we agree with the correspondents that extrapolation of the under-two pneumonia rate to an under-five population, inflates the magnitude of the problem. Unicef provides projections on under-5 mortality from pneumonia. It suggests that there are 14 deaths from pneumonia /1000 children under-5 in India[9]. Our aim was to show that even if we exaggerate the problem by extrapolating the under-two pneumonia rate to the under-five population, it falls far short of the projections for India made by the Unicef and the case for vaccination is difficult to make.

Regarding the underestimation of pneumonia deaths (paragraph 4); we are aware that deaths are less likely to occur in a clinical-study situation. The Unicef suggests that in remote areas with poor access to medical care, 10 per cent cases of pneumonia die[9]. We used this 10 per cent mortality rate, rather than the observed mortality of 0.77 per cent in our calculations. There is thus no justification for the criticism that we have underestimated the problem of death from pneumonia.

We note that the correspondents seek to underplay the significance of the findings of the Hib Probe preparatory phase study[4]. They point to two major deficiencies. They say the sample size was too small to look at the problem of meningitis and also that the

data show only the numbers of 'hospitalized cases' of pneumonia and it is not a reflection of the magnitude of the problem in the community. This seems incredibly disingenuous.

The sample size was calculated carefully prior to the study. To quote from the report, enrollment of approximately10,500 children in the cohort study from all the three study sites was considered to be sufficient to provide 80 per cent power to estimate the incidence of severe pneumonia with a confidence interval of ± 0.3 per cent and the incidence of meningitis with a confidence interval of approximately ± 0.1 per cent. The actual enrollment exceeded these requirements and so there is no justification to say the sample size was too small for purposes of the study.

The suggestion that this was merely a study of 'hospitalized children' which does not reflect the magnitude of the problem in the community also needs to be disputed emphatically. In the three study areas with a total population of 370,000 (Anaicut block in Vellore which was studied, itself had a population 128,000), eligible children under 2 were enrolled. Families were taught the signs and symptoms of pneumonia and meningitis and the location of study hospital. Community volunteers were appointed and trained from within the local community. They visited each household every 2 weeks for the entire study period. Volunteers provided assistance in seeking health care at the study center. To facilitate use of study hospitals and to ensure access to healthcare to study participants, travel fare, cost of hospitalization, treatment, antibiotics, laboratory investigations and cost of X-ray were all reimbursed. Community workers collected follow-up data for each enrollee on illness events during the study. Verbal autopsies were performed for children who died during the study period. It is difficult to imagine how a more elaborate community based study can be performed. We submit that the data from this meticulously executed multi-center study cannot be dismissed as merely a hospital based study - as the correspondent will have it portrayed retrospectively (misleadingly they use the term 'hospitalized children' to convey this wrong impression), just because the study showed that all-cause pneumonia and meningitis lower than projected.

The correspondents castigate us because we have not reported in our Editorial, a Vellore verbal autopsy study where 11 died due to 'respiratory causes' and 2 children had meningitis. There are innumerable such verbal autopsies performed. Even if it were published (the reference is not provided so we do not know) this is essentially a small anecdotal report and it is difficult to use it to plan programmes, without details of the population denominator.

Put mildly, the correspondents have suggested that our editorial is biased. This is a risk with any writing. The 'correspondence column' in scientific journals allows for extended peer review, to remedy such bias. We hope the Editor will keep this page open for all valid opinions that contradict what we have said and which will help us change our minds. As a nation, we must be open and willing to be swayed by the evidence. Unfortunately the evidence in present letter is not very persuasive.

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