

# Letters to the Editor

## Author's Response

### Self-rated health may be adequate for broad assessments of social inequalities in health

From S V SUBRAMANIAN\* and KAREN ERTEL

We thank the authors for their engaging responses,<sup>1–3</sup> and especially applaud Singh-Manoux and colleagues and Huisman and colleagues for conducting additional analyses as a response to our letter.<sup>4</sup> We take this opportunity to summarize what we have learned from this exchange on the question that implicitly or explicitly motivated the three studies:<sup>5–7</sup> are assessments of social inequalities in health based on subjective health problematic? The quick answer, based on the original study and these exchanges, is, maybe not.

We reiterate—as we did in our original letter—we do *not* question the intrinsic relevance of examining whether the association between self-rated health (SRH) and mortality varies by socioeconomic status (SES). Our concern was mainly with interpreting these tests of interaction to comment on whether SRH-based health inequality assessments are misleading. The additional analysis conducted by Singh-Manoux and colleagues bolsters their initial claim that assessment of health inequalities based on SRH are likely to be conservative compared with assessment of health inequalities based on mortality. Their additional analysis also underscores that assessments of social inequalities in health may well depend on the specific measure of SES;<sup>1</sup> an aspect that needs attention in general.<sup>8</sup> The additional analysis conducted by Huisman and colleagues, meanwhile, suggests that assessments of social inequalities in health using SRH may depend on how we dichotomize 'poor' SRH.<sup>2</sup> Under one specification (when SRH=fair/poor health or not) there is some overestimation of social inequalities in health (compared with what is observed based on SES differentials in mortality). However, when SRH is dichotomized as fair/poor/good or not, the social inequalities in health using SRH are similar to those observed using mortality.<sup>2</sup> Thus, the studies by Singh-Manoux and colleagues and Huisman and colleagues, with the benefit of these additional analyses, bring us some

insights about the use of SES as well as the use of SRH in health inequalities assessment. At the same time, these additional analyses, in our view, do not suggest that we are in danger of getting the magnitude of social inequality in health completely wrong if we used SRH instead of an objective measure such as mortality.

Dowd and Zajacova choose not to conduct the additional direct test that we suggested in our letter.<sup>3</sup> Instead, they cite reasonable epidemiologic arguments to question our analyses (and comparison) as well as to defend their decision not to conduct a direct test in their sample, though it would have been possible. While we concur with their nuanced epidemiologic reasoning, we still think a direct comparison between social inequalities in SRH and social inequalities in mortality in their sample would have provided broad insights into this big question, with their concerns included as caveats. Without the benefit of a direct comparison, it is difficult to assess the following conclusions from their original study: 'the finding that SRH does not predict mortality as well at lower levels of SES has important implications for research on health inequalities that has assumed the magnitude of SES inequalities in SRH corresponds well to SES inequalities in mortality'<sup>5</sup> (p. 1220) and 'whether this commonly used measure is ultimately too misleading to be used in the study of health inequalities'<sup>5</sup> (p. 1220).

Finally, in their response, Singh-Manoux and colleagues cite Amartya Sen's influential editorial that showed discordance between self-reported health and mortality using aggregate data from India in the 1970s.<sup>9</sup> One of the authors of this exchange (S.V.S.) has examined more recent data at the individual level, and found the SES patterning in SRH or self-reported morbidity in India is in the expected direction (i.e. low SES are more likely to report poor health or report morbidity).<sup>10</sup> Indeed, other recent reports from developing countries,<sup>11,12</sup> suggest considerable face validity to self-reported health measures. In conclusion, while rigorous scrutiny of subjective health measures is necessary, it is comforting to know that assessments of the extent of social inequalities in health based on SRH, a measure frequently used due to its ease of assessment, are not likely to be misleading.

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## Sex-differential non-specific effects of BCG and DTP in Cebu, The Philippines

From PETER AABY,\* CHRISTINE STABELL BENN, JENS NIELSEN and HENRIK RAVN

In their recent paper Chan and colleagues<sup>1</sup> studied the potential non-specific effects (NSE) of DTP in The Philippines. It is encouraging that sex-differential NSE are studied and different ways to examine these effects are being explored. Very few studies have collected post-mortem vaccination information<sup>2</sup> and the present analysis is, therefore, particularly interesting because the team succeeded in collecting vaccination information after death from 99% of dead children. Interestingly, the study provided support for the importance of sex-differential NSE, which we have emphasized in previous publications.<sup>2–7</sup> However, the study emphasized several key messages which contrast with observations we have made in Africa.<sup>2–9</sup> As discussed by Chan *et al.* these contrasts could be due to regional differences in morbidity patterns. However, both the Cebu and our

data do not suggest major differences in NSE for major morbidity categories like diarrhoea, lower respiratory infections or malaria.<sup>1,3</sup> Hence, it seems worthwhile to explore whether the contrasts may be due to differences in methodology or vaccination policy.

### Comparing sequential vaccinations: which age range?

The Chan *et al.* analysis reported that DTP was associated with 57% (CI 12–79%) lower mortality among BCG-vaccinated children controlling for relevant background factors.<sup>1</sup> There was no indication that this difference was due to prevention of whooping cough<sup>1</sup> and the difference is, therefore, presumably a non-specific beneficial effect of DTP compared with BCG.<sup>1</sup> In Cebu 97% of the children received DTP<sup>1</sup> and BCG would, therefore, only be the predominant vaccine in the first 2–3 months of life. Hence, with age, BCG-vaccinated children who were not yet DTP vaccinated would represent an increasingly selected and *frail* subgroup of children too weak to be vaccinated. This pattern is illustrated by the fact that, whereas the child mortality rate

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