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Response:

The paper 'On the efficacy of recent crustal images of the Indian shield from receiver function' by D. S. Ramesh is essentially a comment on our paper[1]. Even a cursory reading of his comments would make it clear that these barely deal with the contents of our paper published in *Current Science*[2], and create a misleading impression of our *GRL* paper when read without that context.

In fact, Ramesh had sent similar criticisms of our *GRL* paper to the *GRL* editor in August 2003, simultaneously with his submission to *Current Science*. Our reply to his comments was reviewed by both the *GRL* editor and other *GRL* reviewers. They agreed with our conclusion that 'the analysis procedure is sound and results . . . robust', and rejected Ramesh's criticisms.

It is intriguing that Ramesh should have chosen to submit his comments on a paper whose substantial contents appear in *GRL*, to *Current Science*, ignoring the possibility that a reading of his comments by researchers and students, without the benefit of a ready reference to the former, would create a grossly distorted assessment of our results. We believe that it is to forestall such distortions, that scientists and publishers adhere to the well-accepted ethical practice of pointing their criticisms directly to the journals where the original paper appears.

However, for the benefit of readers to make their unbiased assessment we are submitting below our reply to the comments.

We refer to comments of Ramesh[3] on the data, analysis and conclusions of Gupta *et al.*[1]. Ramesh points out five problems with the work of Gupta *et al.*: (i) the quality of the conversions, (ii) flaws in identification of the phases, (iii) flaws in analysis, (iv) flaws in processing strategies, and (v) errors in estimate of crustal thickness (H) and Poisson ratio (we actually determine V_p/V_s (κ)). We take each of these points separately.

(i) We have worked on receiver function (RF) data from a large range of environments from central Asia to the central Pacific, and the south Indian data are amongst the highest quality RFs we have experience with. They are in general both simple and of high signal-to-noise ratio. We are intrigued at their being summarily branded as unreliable and of poor quality without any explicit basis. In our opinion, the data are authentic and we fail to understand how Ramesh[3] would have them made 'more authentic'.

(ii) The analysis procedure we follow is that of Zhu and Kanamori[4]. Ramesh states that there are flaws in our phase identification, perhaps not appreciating that we make no phase identification. As stated by Zhu and Kanamori (p. 2973), one of the advantages of this algorithm is that it does not require the picking up of arrival times of different converted phases. We apply the Zhu and Kanamori algorithm to find the maxima in H–κ space and use these to determine the crustal structure.

(iii) As to the comments about flaws in our analysis, we have tested our computer codes on synthetic waveforms and find no flaws in the analysis. Ramesh refers to a statement we make about the tangential component amplitude. 'The authors in Gl report lack of observable energy in the transverse RFs and rule out the presence of dipping structure. Paradoxically, the same transverse RFs were inferred to have observable energy that is modeled in terms of anisotropy[5]. The tangential receiver functions for a number of the stations discussed in Gupta *et al.*[1] are plotted in Rai *et al.*[6]. The comment in Gupta *et al.* referred to by Ramesh concerns the station MTP, not studied by Rai *et al.*[5] (abstract in a meeting programme volume 2003). The quoted comment from Ramesh is therefore quite out of context, because discussion in that meeting presentation by Rai *et al.*[5] (of which four of us were co-authors), primarily concerned a dataset from the stations in the Pan-African granulite terrane of southernmost India and Sri Lanka, not data from MTP or any other station in the western Dharwar craton. To quote Gupta *et al.*[1] out of context and Rai *et al.*[5] without knowing the contents of the meeting presentation, is misleading. Ramesh criticizes the 'unconventional way of presentation of data and results . . .' (Figure 3 in Gupta *et al.*[1]), but does not clarify as to what is unconventional in it. The broad structure of the south India shield is known from a number of controlled source experiments; results of fourteen of these are used in making the Moho contour map in Gupta *et al.*[1]. The H-scale is at least ± 5 km and the κ at least ± 0.05 about the Moho depth and κ values cited in Gupta *et al.*[1]. These plots were made in such a way as to let the reader see the spread about the cited values, not to hide other larger peaks. Ramesh goes on to mention the presence of other 'local maxima' in the H vs κ plots. Gupta *et al.*[1] do not claim that the crust consists of a uniform layer over a mantle half-space; we know from Rai *et al.*[6] and subsequent inversion of the RFs discussed by Gupta *et al.*[1] that the crust contains some internal structure and these can lead to local maxima. Zhu and Kanamori[4] comment that 'in principle, these phases have different moveout with ray parameter from those of Moho *PpPms* and *PsPms* + *PpSms* so that their energy will not be stacked coherently in $s(H, \kappa)$. However, the presence of these phases often smears the $s(H, \kappa)$ maximum and sometimes causes other local maxima. In the case of multiple peaks in $s(H, \kappa)$, information on the crustal thickness and V_p/V_s ratio from nearby stations or other sources can help to resolve the ambiguity'. We follow this practice.

(iv) Ramesh criticizes our choice of the phase weights in application of the Zhu and Kanamori[4] technique, and the fact that we did not migrate the receiver function to a common distance before forming the RF stacks. Choosing the phase weights is somewhat subjective. In this, we also chose to follow Zhu and Kanamori in weighting the *Ps*, *PpPms* and *PsPms* + *PpSms* phases as 0.7, 0.2, 0.1 respectively. We tested the effects of different choices of weights on the resulting estimates of H and κ and found that reasonable choices of weights had little effect on the resulting crustal model. Zhu and Kanamori (p. 2973) give their reasoning for this weighting: 'These values are chosen to balance the contributions from the three phases. Among them, the *Ps* has the highest SNR so it is given a higher weight than the other two. We also set $w1 > w2 + w3$ because the latter two phases have similar slopes in the H–κ plane'. Another reason for down weighting the multiples is that they sample a different part of the crust than does *Ps*. In addition, *PsPms* + *PpSms* consists of two phases which may not sample the structure in the same way and is normally a weak phase. When comparing RFs for events at greatly different epicentral distances, correcting for normal moveout is required but this is not necessary when events from nearly the same distance are stacked. Gupta *et al.*[1] (pp. 1–2) state that the stacks were over small distance and azimuth bins (both 5°). The moveout correction is to normalize the RFs to a common distance, but since our RFs are from a small distance range (± 2.5°), this correction is unnecessary. Stacking over small distance and azimuth bins is standard practice in RF analysis and has been discussed in a number of earlier publications[7,8]. For an event at 60° epicentral distance and recorded on the south Indian crust, the moveout for ± 2.5° is ± 0.02 s for *Ps*, ± 0.08 s for *PpPms* and *PsPms* + *PpSms*. The sample interval of the data is 0.05 s. So the peak broadening caused by ignoring the phase moveout is negligible. Correcting the RFs for events at greatly differing epicentral distances for normal moveout of *Ps* would cause the multiples to stack incoherently, exactly what we wish to avoid; hence the choice of the narrow stacking bins.

(v) Regarding error estimates of H and κ, Ramesh fails to note that the H values from the eastern Dharwar Craton stations discussed in Gupta *et al.*[1] were analysed further in Rai *et al.*[6]. The eastern Dharwar Craton RFs in that study were jointly inverted with the local surface wave phase velocity to determine the crustal structure, not just the Moho depth. This joint inversion provides much tighter constraints on the crustal structure because the weakness of the one dataset (e.g. the time–depth trade-off of RFs) is compensated for by the strength of the other dataset (e.g. control on the average crustal velocity of short-period surface waves). The results of the formal inversion for the data from the eastern Dharwar Craton are in good agreement with the H values for the same stations in Gupta *et al.*[1]. Since the publication of Gupta *et al.*[1], we have inverted the western Dharwar Craton RFs for the crustal structure and find no discrepancy greater than 2 km in H between the two techniques.

Therefore, the data analysed are good, the analysis procedure is sound and the results presented in Gupta *et al.*[1] are robust. We have taken Ramesh's[3] criticisms of a 'lack of natural flair' to heart, while his other criticisms would be clearly seen to be unsubstantiated.

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