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Reply to comment by R. Montelli, G. Nolet and F. A. Dahlen on 'Banana–doughnut kernels and mantle tomography'

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SUMMARY

Montelli *et al.* comment on a paper that we wrote in response to an earlier 'comment' and in which we argue that the Princeton models with or without banana–doughnut theory are effectively the same and that, thus, the beneficial effects of the use of banana–doughnut kernels (BDKs) on global tomography have been overstated. The models are highly correlated. There are (of course) differences, and some anomalies in the banana–doughnut models have higher amplitudes (perhaps by as much as 50 per cent or more) than in the ray theoretical models. However, this occurs mainly for small, weak anomalies, whose resolution by long period data has not been demonstrated. Because of differences in data and inversion strategy, and in absence of a 'ground truth', comparisons of MIT and PRI models do not provide insight into the efficacy and validity of BDKs or the accuracy of the models. With regard to plumes: if they indeed exist and have the appearance as suggested by Montelli et al. then our models are—in that respect—inaccurate. However, this is unrelated to the main point of our paper.

Key words: finite-frequency sensitivity, linearized traveltime tomography, plumes.

1 OPENING REMARKS

(Montelli *et al.* 2006a, hereinafter M3) gracefully admit having made an error in crust correction in their earlier models. We agree with them that this does not affect any of the main conclusions reached in the papers discussed here, but it is gratifying that our exchange is resulting in better models of Earth's structure. M3 submit that 'prolonging [this] debate about the efficacy of finite-frequency theory' (hereinafter FFT) is not productive. We fully agree. And since we just reply to their multiple comments there is a simple way to conclude it. In our view, this exchange merely distracts from the fact that there is much agreement about virtues of FFT in its general sense. Furthermore, M3's lengthy comment on HH05c does not really address the main conclusion of that paper, and a pointwise response to the issues raised would repeat much of de Hoop & van der Hilst (2005a,b, hereinafter HH05a and HH05b) and (van der Hilst & de Hoop 2005, hereinafter HH05c), or comments by Trampert & Spetzler (2006) and Boschi *et al.* (2006). For these reasons, we keep our reply brief.

HH05c do not question the innate promise of FFT but take issue with the simple banana–doughnut representation of it and, in particular, with the repeated claims about the effects of banana–doughnut kernels (BDKs) on published (global) tomography models. From the material presented in (Montelli *et al.* 2004a, hereinafter M1), we concluded that tomographic models produced with geometrical ray theory (GRT), that is back projection along infinitesimally narrow ray paths, or with banana–doughnut theory, with back projection along BDKs, are very similar. This similarity may have little to do with the theory. For instance, parametrization by means of relatively coarse basis functions can yield results that are essentially the same as ray theoretical (RT) inversions (e.g. HH05a, fig. 5). The models may be fine, as (linearized, traveltime) tomographic models go, but they must be evaluated at scales contained in the data used to construct them.

2 MACHIAVELLI AND FINITE FREQUENCY THEORY

Innovators will appreciate Machiavelli's eloquence on the challenges they face, but the quote deserves clarification. First, Dahlen and coworkers deserve much credit for bringing this topic to the forefront of seismological research, but one must realize that many others have analysed finite-frequency wave propagation and wave speed sensitivity. Second, and more relevant for this discussion, insofar FFT is the implied 'new order of things' there is neither controversy nor opposition.

HH05a–c acknowledge that application of FFT for multiscale seismic imaging with increasing volumes of broad-band waveforms is exciting and promising. With FFT one can use the scales in data (e.g. frequency) to constrain spatial scales in structure and in the physical processes that produce them. To achieve this one should consider full wave dynamics and use the elastic wave equation admitting heterogeneity with limited smoothness; the finite frequency then refers to how this structure is 'seen' by the seismic waves. In this regard, linearization between data and model parameters by means of (BDK) kernels calculated in simple (quasi-homogeneous) background media has significant limitations.

3 UPPER AND LOWER MANTLE

We appreciate M3's remarks about the upper-mantle part of their models, but suggesting that HH05c's analysis is biased through an emphasis on upper-mantle structure is somewhat disingenuous. HH05c illustrate the similarity between models with and without BDKs, as published by M1, by means of a figure showing a vertical section across the entire depth range of the mantle (fig. 1), a figure showing both upper- and lower-mantle structure (fig. 2), and a figure focusing on the lower mantle (fig. 6). HH05c's discussion about the amplitude enhancing effects of BDKs also concerns the lower mantle.

About fig. 1 of HH05c, M3 argue that '[images of slabs] have little bearing on the issue of the effectiveness of BDKs, since the laterally extensive character of slabs make them less prone to wave front healing than plumes'. The latter may be true, but if these structures are large and the data associated with waves passing through them are not degraded by 'wave front healing', then one should wonder why they are not resolved better. It also reveals a conceptual problem (see HH05b): in the (linearized way) they are used, BDKs know neither the level of 'wave front healing' of the data they are associated with nor the type (e.g. 'slabs' or 'plumes') or scale (large or small) of structure they sample.

4 BANANAS, APPLES AND ORANGES

At the end of Section 2, M3 remark that 'comparing models in M1 and M2 is like comparing apples and oranges'. We understand the differences and similarities between the models in M1 and M2. For reasons mentioned in M3, HH05c used the comparison between models in M1 to investigate the effect of theory, as M1 do, and the comparison between models in M1 and M2 to address effects of different data selection and fit. In our opinion, these are fair comparisons (and they show that the apple, e.g. the RT model in M1, is rather similar to the orange, e.g. the model in M2.)

Given their concern about appropriate comparisons, it is puzzling that M3 use differences between models PRI-P05 and MIT-P05 to assess the effect of banana–doughnut theory. The statement that 'the improvement in spatial resolution [$\cdots$] is an expected consequence of FFT' has little meaning. M3 acknowledge (in first part of their Section 4) that 'some differences between models PRI-P05 and MIT-P05 ... have little to do with the issue of finite frequency versus RT inversions'. There are, in fact, many differences in the way these models were constructed and one should not use comparisons between them to argue for or against the efficacy of BDK theory. Furthermore, it has not been demonstrated that the structures in question are resolved or that their resolution has been improved. A model evaluation as in M3 could be meaningful if we know the actual structures, but we do not.

It would, therefore, have been more logical for M3 to challenge the main point of HH05c (that their banana–doughnut and RT models are rather similar) by showing a convincing counter example using models with and without BDKs. In fact, because tomographic models are fundamentally non-unique, and because the existence of the small scale structures used to claim superiority of BDK inversions is not beyond doubt, an even more convincing response would be to calculate waveforms, for instance with the spectral element method (Komatitsch *et al.* 2002), both for the RT and banana–doughnut models, and to demonstrate that at the typical frequencies of the data used in the inversions they are different and that those from the BDK model match the observations better.

HH05c note that the amplitude of variation in MIT-P05 is generally lower, by as much as a factor of 2, than in PRI-P05. Plotting these models at the same scale will thus give the predictable effect that (relative to PRI-P05) MIT-P05 shows little structure for the regions selected. While it reveals M3's intention, it has no bearing on the efficacy of BDKs. HH05c's fig. 4 demonstrates that one can resolve—with a RT inversion of synthetic data—most of the features in PRI's BDK models. We showed this for the upper mantle, but the same holds for structure elsewhere in the mantle. This is a further indication that the differences between MIT-P05 and PRI-P05 are mostly due to other aspects of the inversions (e.g. different data; different parametrization; different regularization and different data fit criterion).

5 STATISTICAL ANALYSIS

M3 devote much of their 'comment' to justifying the statistical analysis that underlies the claim that anomalies in the BDK inversions are 30–50 per cent larger than in the RT inversions. We continue to take issue with some aspects of that analysis. HH05c simply showed that drawing conclusions from frequency distributions of ratios, as done in M1, is tricky, in particular if exclusion of certain values yields distributions that are far from Gaussian. Large differences can occur for weak anomalies, but their significance is questionable (see also Trampert & Spetzler 2006).

M1 plot histograms of the ratio—$\delta c_{\mathrm{BD}}/\delta c_{\mathrm{RT}}$—of the wave speed perturbations according to the BDK and GRT inversions. They do this for corresponding points in the models but they exclude wave speed perturbations $|\delta c_{\mathrm{BD}}| < 0.2$ per cent because they are regarded as noise. M3 argue that including these values would produce a bias toward a 'null result'—that is, a histogram centered around 1—because it ignores effects of 'wave front healing'. Exclusion of small values causes (of course) a shift in the average of the distributions to larger values. It is also true that, as noted in M3, choosing a larger threshold value, say, $|\delta c_{\mathrm{BD}}| > 0.3$ per cent, amplifies the amplitude enhancement effects of finite-frequency inversion. However, such shifts to ratios larger than unity are arbitrary and, in our view, meaningless. Consider, for example for the models in M1, the inverse ratio, that is, $\delta c_{\mathrm{RT}}/\delta c_{\mathrm{BD}}$. Exclusion of $|\delta c_{\mathrm{RT}}|$ larger than a certain threshold would then suggest that the amplitudes in the GRT inversions are larger than in the BDK inversions.

HH05c avoided the peril of analysing histograms of ratios and showed simple δc_{BD} vs. δc_{RT} scatter plots off all model points at 1350 km depth. Anomalies with a large ratio plot on a line with a steep slope in δc_{BD} vs. δc_{RT} space. The scatter plots presented in HH05c (their fig. 6)—or similar such plots for any depth in the lower mantle—show that, in general, the anomalies in the banana–doughnut and ray-theoretical models are highly correlated. Certainly, there are points at which the amplification due to the use of BDKs is as large as claimed by M1 and M3 (i.e. 30–50 per cent, or more), but this is only so for small values of δc_{BD} and examples of a wave speed reduction are probably as numerous. HH05c note that there may be an amplitude enhancing effect of the order of 10 per cent (perhaps increasing to 20 per cent toward the base of the mantle). M3 prefer to analyse the ratio in scatter plots after conversion to temperature anomalies. We confess that we are less confident about the accuracy of such conversions than they are, but the numbers given by M3 (9–13 per cent) roughly agree with those given in HH05c.

However, for the sake of the argument, let us assume that the local amplitude enhancement is as large as 20 per cent. Wave speed perturbations that are of the order of 0.50–1.00 per cent in GRT inversions would then be 0.60–1.20 per cent in BDK inversions. Can such differences be resolved? Larger amplitude increases (such as the 30–50 per cent claimed by M1) can, certainly, occur for weak anomalies. If it can be demonstrated that such weak anomalies as well as the resulting differences (say, the difference between 0.30 and 0.45 per cent) can be resolved by the long period *PP* or *SS* data, then M1-3 are justified in claiming that their banana–doughnut theory leads locally to a significant increase in amplitude compared to GRT inversions. In our opinion, however, such small anomalies are not resolved by the (long period) data used and (as M3 admit) small differences can easily result from slight changes in the control parameters of the tomographic inversion. It is not yet demonstrated that the subtle differences do not—in the words of Trampert and Trampert & Spetzler (2006)—disappear in the null space of finite-frequency tomography.

6 PLUMES?

Even though this was not a subject of HH05c, most of M3's 'comment' on HH05c is a defence—and reinforcement—of their previous claims about plumes in the lower mantle. M3 show that in the lower mantle beneath several 'hot spots' their *P* and *S* models reveal slow wave propagation whereas MIT-P05 does not. 'Plumes' or not, if these anomalies exist as prominently as claimed in M1-3 and if we do not see them in MIT-P05, then our model is—in this aspect—less accurate than PRI-P05. (However, again, this does not demonstrate the impact of BDKs since there are many other differences between the inversions.)

The issue is not as straightforward as portrayed in M3, however. Upon closer look, the PRI-P05 and PRI-S05 images are quite different. Consider, for instance, fig. 4 at 300 km depth: the slow *P* anomaly is in a different location than the *S* anomaly, and it has a very different appearance. This is the upper mantle, where (according to M1-3) the benefit of BDKs can be ignored, but in the deep mantle, for instance, at 2350 or 2800 km, there are also substantial discrepancies (even anticorrelations) between the *P* and *S* maps. Beneath other geographical regions the situation is not different. M3 note that the *S* images confirm the split of the 'Hawaii plume' at 1450 km depth as seen in the *P* image. However, to our untrained eyes the appearances of the anomalies at that depth are rather different from the structure in the *P* maps. At 1900 km the location of the slow *S* anomalies are also different from that in PRI-P05. In fact, a more quantitative comparison reveals that the *P* and *S* anomalies in the regions and at depths shown in figs 1 and 4 are poorly correlated ($R < 0.5$).

Of course, we could—and would, normally—pay attention mainly to the qualitative similarities between the 'plume signature' in the *P* and *S* maps, and perhaps attribute discrepancies between them to poor resolution. However, M1,2 state that because of the use of FFT small anomalies are now resolved better. If, on the one hand, small structures in either model are resolved, then the local discrepancies between *P* and *S* are significant. This would suggest that the cause of the wave speed anomalies is more complicated than a simple thermal perturbation. If chemical composition plays a role one could wonder if simple wave speed-to-temperature conversions are meaningful. If, on the other hand, these particular anomalies are (still) poorly resolved, then we could agree that the discrepancies between the *P* and *S* models can be considered insignificant. However then, what about the other small structures? Is this a Machiavellian conundrum or a simple reminder that the models should be evaluated at the length scales that can actually be resolved by the data used?

M3 seem to attribute differences between the *P* and *S* inversions to the use of short-period data in one of them. Indeed, Montelli *et al.* (2006b) make the surprising statement that they 'refrained from using [short-period] ISC *S*-wave traveltimes' and that 'The *S*-wave data thus lack the difference in extent of sensitivity that comes with different frequency content, and that greatly helped to constrain plume widths in the *P*-wave models.'. If, by implication, the short-period *P* data (for which ray theory was used) greatly helped constrain (thus far elusive) plume-tails, then we wonder why these data did not help improve images of subducting slabs and why M3 attribute the absence of some plumes in our model (solely) to the use of ray theory (for the short-period *P* data).

7 CONCLUDING REMARKS

HH05c questioned claims about the effect of BDKs in linearized traveltime tomography through a comparison between models produced by ray theoretical or banana–doughnut inversions. That M3 call this focus on the effect of theory 'an inappropriate rendition' of their papers is surprising because they themselves (in papers and public presentations) repeatedly emphasize the perceived consequences of their theory. Consider, for instance, the title of M2: *'Finite frequency tomography reveals [. . .] plumes'*.

It was not the intention of HH05c to question the existence (or the interpretation) of 'plumes' or to claim that one model is better than another, but M3 conclude their comment by challenging us—and others that have expressed scepticism about the effects of BDKs—to 'produce high-quality lower-mantle plume images using ray theory'. If lower-mantle plumes exist as imaged in M1-2, then our models fall short and we must produce better ones. However, that is not quite the issue. We concur that for proper imaging of plumes and other elusive mantle structures one may well have to go beyond ray theory. [In fact, 3-D sensitivity kernels (for P_{diff} and *PP* data) are used routinely in our global tomography (Kárason & van der Hilst 2001; Kárason 2002).] We just do not think that BDKs (in strict sense, that is, as used in M1-3) are the answer.

We can discuss details for a long time to come, but in our view repeated 'comments' and 'replies' merely distract from what really is an interesting and important development—and challenge – in modern seismology. As we have said repeatedly, here and in HH05a,b, we enthusiastically agree with Dahlen, Nolet, Montelli (and many others!), that proper consideration of finite frequency effects has tremendous promise for seismic imaging and tomography. Eventually, computers will be sufficiently powerful to enable numerical approaches (e.g. Tromp *et al.* 2005; Zhao & Jordan 2006) toward multi-scale imaging of Earth's interior. Until these applications are feasible at sufficiently high frequencies, theoretical analysis enabling wave-equation tomography is likely to play an important role in enhancing resolution of images of Earth's interior, because they account for the various wave phenomena—including caustics and coupling between *P* and *S*—that occur in (anisotropic) elastic media with limited smoothness and heterogeneity on a wide range of spatial scales. In order to develop a comprehensive methodology for elastic-wave-equation tomography, we have recently formulated approaches for transmission (de Hoop & van der Hilst 2005a) and reflection tomography (de Hoop *et al.* 2006), and we are also analysing shear-wave splitting from a wave-equation point of view (Long *et al.* 2006).

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