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Reply to “Comment on ‘Attenuation, source parameters and site effects in the Irpinia-Basilicata region (southern Apennines, Italy)’ by I.B. Morozov”

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We thank Igor B. Morozov for his interest in our article and for his comment (Morozov, 2011) regarding the non-parametric attenuation curves for the Irpinia-Basilicata region obtained by generalized spectral inversion (Cantore et al., 2011). Morozov’s comment has its root in a new model proposed by Morozov (2008, 2010) for the interpretation of seismic attenuation data, where the author comes to the conclusion that the typically used geometrical spreading terms are oversimplified and argues in favor of a new geometrical spreading model basically including an additional corrective term decaying exponentially with time (respectively distance). Morozov (2008, 2010) reanalyzed a range of datasets presented by other authors in the past using this model and, contrary to previous studies, concludes that all these datasets can be well explained using a frequency-independent Q_e (which he terms as “effective Q”), in contrast to the classical power-law model often implying a significant frequency-dependence of Q.

The publication of his new model has led to a major controversy, and other authors provided a range of weightily counter-arguments to Morozov's point of view (Xie, 2010; Li, 2010; Li and Lu, 2010). In his comment on our article, Morozov (2011) essentially repeats the arguments that he has already discussed in great detail in his previous papers (Morozov, 2008, 2010). In view of this open discussion, we acknowledge that there are certainly different ways of interpreting our non-parametric attenuation results (see also the paper Oth et al., 2011), but also note that a detailed discussion of the differing viewpoints was beyond the scope of our article, for which we refer the interested reader to the cited publications. Nevertheless, we would like to provide a brief discussion of the key points of Morozov's (2011) comment in the following.

First of all, we would like to recall that the attenuation result obtained from our spectral inversions are the non-parametric attenuation functions themselves, not the Q(f) model that we provide as an ensuing interpretation of these curves. The step of fitting a parametric model to these non-parametric curves (equation 2 in Cantore et al., 2011) is not a part of the spectral inversion, as Morozov (2011) seems to suggest, but a simple interpretation of the non-parametric results (that might be done then also using a frequency-independent Q model). Indeed, no *a priori* assumptions are made on the parametric description of seismic attenuation when inverting the amplitudes (equation 1 in Cantore et al., 2011), and the non-parametric curves implicitly include all effects leading to amplitude reduction with distance (geometrical spreading, intrinsic and scattering attenuation), apart from potential 2D/3D effects. This is the great advantage of the non-parametric generalized inversion approach as first proposed by Castro et al.

(1990) compared to parametric inversion approaches (see for instance Oth et al., 2011, and references therein, for a more complete discussion of this issue).

Considering this fact, we disagree with Morozov's (2011) assertion that the interpretation of our curves with a power-law model would "reduce the accuracy of fitting the amplitudes" or would even "push the corresponding errors into the site factors", as he claims. As we clearly stated in our article, we corrected the spectra for attenuation with the non-parametric curves prior to separating source spectra and site response, not with the parametric Q(f) model. The Q(f) model as such does not enter anywhere in our remaining calculations, so the terminology used and interpretation of the attenuation curves have no implications whatsoever for the remainder of our article. Note that even if we would have used a parametric inversion scheme for the spectral inversion, a change in the parametric form assumed for Q would in our case still not have pushed the "corresponding errors" into the site factors, but rather in the source contributions, since we have used a site constraint to remove the undetermined degree of freedom between the source and site contributions and therefore, changes in Q would have mostly affected the source contributions. Parolai et al. (2000) show that when using a constraint on a reference site in a parametric inversion, the site responses of the other stations of the network can be stably estimated, with however clear trade-offs between Q and the source contributions, and in order to resolve this trade-off in such a parametric inversion, besides a reference condition for the site response, *a priori* information on Q (or a reference source) is required. This is however not the case in the non-parametric inversion scheme used in this study.

One of the main reasons for using a power-law model and the respective geometrical spreading term was to ensure the most easy comparability of our results with previous studies that did not provide non-parametric curves as we do. Morozov (2008, 2010) transformed the results provided in a power-law form from previous studies into his parameterization for discussing them in the framework of his model. We think that for simple comparative purposes as intended in our article, it is irrelevant whether we transform the results from previous studies provided in a power-law form into a different parameterization and interpret our non-parametric results in this given parametric formulation, or we directly interpret our non-parametric attenuation results using the power-law model.

Whether Morozov's interpretation of our non-parametric curves is superior to the traditional power-law model is a question that is difficult to answer with the current state of knowledge and a matter of an ongoing debate. Indeed, the trade-off between geometrical spreading and Q is inherent to the problem of seismic attenuation. Morozov simply suggests to shift the frequency-dependence that we obtain for Q when using the simple geometrical spreading function G(r)=5/r into a more complex G function coupled with a more simple Q. Moreover, the simple fact that the attenuation curves show a similar shape beyond a distance of about 20 km for all frequencies does not necessarily imply a frequency-independent Q. One possibility might also be that both G and Q show significant frequency (and depth) dependencies that may also effectively cancel each other. In Morozov's (2008, 2010) model, the additional geometrical spreading correction term γ is however frequency-independent, which may or may not be true. Furthermore, the physical meaning of this parameter is strongly debated (Xie, 2010). We acknowledge however that it might be interesting for future applications to show a comparison of both approaches.

We would also like to point out that no matter what interpretation for Q may be more appropriate, the complete spectral model that we developed can be used for practical applications (e.g., stochastic scenario simulations or corrections for site response) regardless of this discussion. For these purposes, the physical basis of the attenuation model is not of fundamental importance, and the only thing that matters is that attenuation is described with a model (whatever this may be) that adequately represents the amplitude decay with distance. If this condition is fulfilled using a geometrical spreading proportional to 1/r and a power-law Q(f) model, then attenuation can be appropriately taken into account in these applications (if the model is applied exactly in the given way), regardless of the physical meaning of this parameterization.

Finally, we would like to close our reply with the following statement made by Brian Mitchell (2010) in the epilogue of the forum on seismic attenuation in Pure and Applied Geophysics that he initiated: *But differences of opinion on the topic will undoubtedly continue until seismologists achieve a better understanding of seismic wave attenuation in the crust and mantle. One thing that most seismologists agree upon is that Q measurements are inherently difficult. If nothing else this forum will, I hope, remind many seismologists about those difficulties.*

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