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Reply to "Comment on 'Measurement and implications of frequency dependence of attenuation'" by I. Morozov

In his comment on our paper entitled "Measurement and implications of frequency dependence of attenuation" (LMPR: Lekić et al., 2009), Morozov characterizes as "incorrect and… misleading" our determination of the frequency dependence of attenuation at long periods (>60 s) in the mantle. Morozov goes on to argue that our model was obtained by inversion and was "not constrained by the data fit", and that we do not prove that frequency dependence of attenuation is required by the data. We strongly disagree with Morozov's claims and believe that his erroneous interpretation may stem from a misreading of our paper. For the benefit of future readers, we wish to further clarify what our procedure entails.

The attenuation ($1/Q$) experienced by Earth's free oscillations (normal modes) depends on the mode's frequency ($\log Q \propto \alpha \log f$) and on the depth-profile of attenuation $Q(r)$ within the Earth (see pg. 347–362 of Dahlen and Tromp, 1998). Specifically, each mode's attenuation represents a specific weighted average of $Q(r)$. Confronted with a dataset of attenuation measurements, we can attempt to disentangle the frequency-dependent signal by comparing the attenuation measurements of modes that have similar dependencies on $Q(r)$ but have different frequencies. Anderson and Minster (1979) suggested exactly such an approach. However, no two modes have identical depth sensitivity, and a work-around is required. Thus, we seek linear combinations of low-frequency modes such that they have a combined depth sensitivity identical to a different linear combination of high-frequency modes. Each linear combination of modes can be thought of as a "composite" data-point, which is, with proper treatment of measurement uncertainty, just as valid as attenuation measurements of individual modes. If the low- and high-frequency "composite" Q measurements do not agree, we can undertake a forward-modeling procedure to determine the value of α necessary to reconcile them.

In Fig. 3 of LMPR, we show the likelihood of various values of α determined using a set of schemes for assigning modes to low- and high-frequency bins. It is important to note that Fig. 3 is just a different way of representing the *actual attenuation measurements*. Hence, Morozov's assertion that our model is "not constrained by data fit" is incorrect. Fig. 3 clearly shows that it is very unlikely that $\alpha = 0$ throughout the mantle and across all frequency bands, given the published measurement errors. Additionally, the complicated character of the likelihood estimates appears to be inconsistent with any simple constant α model. The frequency-dependent α model displayed in Fig. 4 is a result of a forward-modeling approach that produces behavior consistent with the data shown in Fig. 3. We do not, however, argue that this is a unique solution; nevertheless, it is a significant improvement over any model with constant $\alpha = 0$, strongly indicating that frequency dependence is required by the data.

The basic confusion underlying Morozov's argument may be rooted in his claim that: "The fundamental trade-off between $Q(f)$ and $Q(r)$ consists in the fact of multiple models fitting the data." On the contrary, we are not aware of any model that can fit all of the available free-oscillation attenuation measurements. In fact, the trade-off is due to the fact that free oscillations at different frequencies have different sensitivities to structure at depth. Morozov claims that "it is known and accepted" that QL6 (DE: Durek and Ekström, 1996) can fit available attenuation data with a frequency-independent Q. While QL6 is a very good model of attenuation in the Earth, it is by no means capable of explaining all of the measurements to within uncertainty (see Table 5 of DE). Furthermore, Morozov argues that if QL6 fits all the data to within uncertainty, then Q estimates from low- and high-frequency free oscillations will always be compatible, which would imply that Q does not depend on frequency. This is a circular argument, since the frequency-independent QL6 could fit all of the data only if attenuation within the mantle was actually frequency independent.

We believe that Morozov's claims are based on a misinterpretation of Fig. 5 in LMPR. Morozov states that in Fig. 5 the "inferred α values move the predicted attenuation values in the direction opposite to what the data suggest." That may appear so, at first glance, because the curve marked by triangles has significantly lower q values for angular orders larger than 64. However, that curve represents the predictions of a QL6 model arbitrarily combined with LMPR's model of α. It was intended to show that combining QL6 (which was obtained by assuming $\alpha = 0$) and LMPR's model of α yields a significant systematic mismatch of the data; therefore there is potential for improving the $Q(r)$ profile beyond QL6. Using a preferred model for frequency dependence, such as that shown in Fig. 4 of LMPR, necessitates the development of a different 1D attenuation model to be consistent with the data, which was not attempted by LMPR. We did not develop such a model in LMPR because our procedure allowed us to eliminate the need to invoke any depth-dependent $Q(r)$ model. This was the main point of LMPR.

We acknowledge that Fig. 5 of LMPR could be confusing and hope that these additional explanations will clarify its meaning for the readers. We remain confident in our determination of frequency dependence of attenuation at long periods (>60 s) in the mantle. We hope that future studies aimed at constraining the depth dependence of attenuation, as well as those that require applying a dispersion correction, will account for this frequency dependence.

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