Reply to "Comment on 'Thirty Years of Confusion around 'Scattering Q'?'" by J. Xie and M. Fehler

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In the preceding commentary, Xie and Fehler (2009) correctly identified the main points of my recent critique (Morozov 2009; hereafter M09) of widespread use of frequency-dependent Q in seismology, which were: 1) the faith in the existence of geometrical spreading (GS) corrections that are accurate enough to allow "good measurements" of attenuation; and 2) confidence in pervasive frequency dependence of Q within the Earth. Most of their arguments were answered in the more detailed paper (Morozov 2008; hereafter M08), in which the model and GS measurements were described. Unfortunately, Xie and Fehler did not mention this paper; therefore I will summarize the main points of M08 and M09 again here.

First, theoretically, there is no doubt that attenuation can be frequency-dependent (*e.g.*, Liu *et al.* 1976), and I did not intend to disprove this. In fact, causality requires that Q *must* depend on frequency, yet this constraint rigorously applies only to impractical frequencies below ~10^{-99} Hz (see, *e.g.*, Futterman 1962). Also, scattering causes wave attenuation and formation of seismic codas, and such processes are typically wavelength-dependent. However, such possibilities should not overshadow the observations of the Earth within the seismological frequency band. The question is how often $Q(f)$ is actually observed in the data.

Measurements should be independent of assumptions and described in terms of adequate physical quantities. In particular, one needs to clearly differentiate between the apparent (wave) Q (sometimes expressed by t^* or attenuation coefficient) and in-situ (medium) Q. As modeling shows, the apparent Q is frequency-dependent in layered structures (*e.g.*, Anderson *et al.* 1965; Mitchell 1991—not ignored in M09). These observations represent examples of "geometrical" effects discussed in M09. Nevertheless, our focus here is on the true medium Q.

Xie and Fehler attribute to me a statement that the frequency dependence of Q may be dubious because of the non-uniqueness of amplitude data fit. However, this was not the main reason for $Q(f)$ fallibility! Data fit may be uncertain, but physics still holds more important clues. My argument was that scattering is not the type of process to be described by a quality factor, *i.e.*, by fractional loss of mechanical energy *per oscillation period*. Incident wavelength is not a characteristic scale for scattering processes. The correct way to describe scattering, as well as wave attenuation in general, is by using the spatial attenuation coefficient, which can be parameterized by the differential cross-section, turbidity, or mean free path (Chernov 1960, pp. 35–57; M09; see also references given by Xie and Fehler). This attenuation coefficient, only rendered in temporal form, is the $\alpha(f)$ of M09.

Note that owing to its nature (essentially that of a spectral ratio), $\alpha(f)$ is measured in most types of attenuation observations and then typically transformed into a Q value by writing $Q(f) = \pi f/\alpha(f)$. However, are these two quantities "interchangeable," as Xie and Fehler say? Does this Q become a property of the medium (not to mention the rock quality factor)? My answer is no, because in real data, the measured $\alpha(f)$ always contains contributions from GS, which is neither intrinsic attenuation nor random scattering. This $Q(f)$ is apparent, *i.e.*, only a property of the propagating wave, but transformed to appear analogous to the medium quality factor. This connotation with quality suggests interpretations in terms of scattering, relaxation, fluids, and temperature, which may still be unfounded. It also encourages the use of $Q(f) = Q_0 f^{\eta}$ dependence whose main flaw, however, is in rejection of the basic possibility of $\alpha(0) \neq 0$.

It is important to differentiate between real observations in the Earth and modeling in a heterogeneous "Earth medium" (the term often used by Xie and Fehler). The Earth has a structure, which introduces unknown variations of GS into the measured amplitudes. Note that Q-related amplitude decays are typically subtle compared to GS, and less than ~10% variations in GS can eliminate the observed $Q(f)$ dependencies (M08). Such levels of structural variability should be common within the crust.

In their argument about the $G(t) = G_0(t)e^{-\gamma t}$ GS law not improving attenuation studies, Xie and Fehler again take a model-centric point of view. They point out that the $e^{-\gamma t}$ factor is insufficient for describing long paths and localized structures, such as lithospheric slabs. Indeed, this factor was introduced only as a first-order correction to $G_0(t)$ (Equation 2 in M09), and $G_0(t)$ should of course be approximated as accurately as possible in any (simple or complex) structure. However, with $G_0(t)$ modeled in the best possible way, can we safely set $\gamma = 0$ in this expression? Apparently not, because any model is only an

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approximation to the reality. By using parameter γ, the residual $G(t)/G_0(t)$ ratio can be *measured*, showing, for example, that γ is typically positive and equal ~0.01 s^{-1} for crustal body waves (M08 and M09). Such knowledge hugely benefits attenuation studies. Further, γ shows a most remarkable correlation with crustal tectonic ages (M08).

Finally, the closing paragraph of Xie and Fehler's comment incorrectly represents the main argument of both M09 and M08. The model is two-parameter (γ and Q_e; see Equation 2 in M09) and should be compared to (Q_0, η) or similar models. There is no increased parameter trade-off; on the contrary, the trade-off of both Q_0 and η with GS is removed. Also, in many cases, parameters (Q_0, η) can be transformed into (γ, Q_e) (M08).

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