On improving the model of Earth's seismic attenuation

Igor B. Morozov

Department of Geological Sciences, University of Saskatchewan, Saskatoon, SK S7N 5E2 Canada

Abstract — The critique of my Forum paper by Xie (Pure Appl. Geophys. *167*, 1147–1162, 2010) is answered in detail. Despite Xie's conclusions, estimates of seismic attenuation can be improved by: 1) switching from Q to the attenuation-coefficient view and 2) measuring the variations of geometrical spreading (or focusing/defocusing) from the data instead of only predicting them by modeling. This appears to be the only feasible approach, because in different experiments (with body and surface waves, coda, free oscillations, and laboratory measurements), the "geometrical spreading" may take various forms which may be intractable by modeling. After such correction for variable geometrical spreading, frequency dependence of Q becomes no longer observed in many cases.

1. Introduction

The paper which opened current discussion (Morozov, 2010a; hereafter M10a, with similar notation for my other papers) was written over two years ago as a follow-up to M08 and was not intended for a forum. Similarly to M08, it gave examples of frequency dependences of the apparent Q and explained them by slight variations and uncertainties in the geometrical spreading (GS). Since then, the same point was also illustrated for long-period body (M09b), surface waves, and even free oscillations (M10b). It now seems clear that *empirically*, the frequency dependence of Q can often be explained by some sort of a "GS" and a frequency-independent "effective quality factor" which was denoted Q_e in M08. The key conclusions from these observations were: (1) the GS is typically not known accurately enough in order to observe the frequency-dependence of Q, and (2) GS variations can and should be estimated from the data. Recently, Xie (2010, hereafter X10) argued that this cannot be done and focused specifically on the functional form of the GS used in M08.

Unfortunately, X10 does not address the main messages (1)–(2) above but only focuses on the GS. The model proposed in M08 is presented as intending "to reduce the measurement error of $Q(f)$" (X10). This was not my goal, and the model was actually proposed in order to separate the structural effects from those of anelastic and small-scale scattering attenuation and to remove the ambiguity of Q related to background model selection. As suggested in M09b, Q may not even exist as a physical parameter of the medium. The key point of the present discussion is therefore not about the functional form of the GS but about whether the GS should be *measured* or *assumed to be known*, and what consequences for Q this choice may entail.

Because X10 is almost entirely devoted to interpretations of my recent papers (M08, M09a, and M10a), I am also compelled to offer some remarks regarding these interpretations. A number of key points appear to be unnoticed, misunderstood, or misrepresented in X10, and these points are succinctly reiterated here.

To briefly answer the question of the forum, it appears that the seismological Q often turns out to be frequency-dependent because it is *not an independent physical property of the medium* but only a transformation of the temporal attenuation coefficient, χ: $Q = \pi f/\chi$, where f is the frequency (M08, M10a). From this transformation, there arises a leading increase of Q with f, which is indeed found in many studies. By contrast to Q, χ reflects the effects of the structure and thereby may be closer to approximating the true physical reality.

2. Geometrical spreading

X10 presents my argument in a narrow sense, as an attempt for improving the estimates of seismological Q by using a different GS model. However, even in such specific sense, X10 arrives at a paradoxical conclusion: "… while … the previous Q measurements have limited precision, they cannot be improved by using the new GS model." It sounds like either we have already arrived at the absolute truth or the GS model used in my recent papers is somehow hopelessly poor. However, the "new" GS model only extends any of the existing ones:

$$G = G_0 e^{-\gamma t} , \tag{1}$$

where G is the true GS, and G_0 is some "background," or reference estimate for it. Both of these functions generally depend on the spatial coordinates, time, and frequency. The only simplification made in (1) is that G_0 is corrected by only a time-dependent factor. For $\gamma = 0$, models (1) are identical to the existing GS models, and therefore the main message of X10 is apparently that γ cannot be considered as non-zero *at all*. This can only mean that G_0 is already accurate.

Nevertheless, many studies including X10 show that the theoretical GS is rarely accurate enough in practical cases. Here, "enough" means accurate within several per cent which would allow measuring the part of attenuation related to Q. In M08 and related papers, this accuracy was tested quantitatively by using (1) as a single-parameter generalization of the conventional G_0. The data showed that γ is non-zero in most cases, and in most of them γ was also positive, meaning that the real GS within the lithosphere is typically faster than in a uniform half space. Because the data cannot be denied, in his Appendix 1, X10 reverses his argument and declares model (1) to be "invalid" not because γ should equal zero theoretically but because it turns out to be too large when determined from the data. However, large γ only means that G_0 is too inaccurate, and therefore attenuation estimates *can* actually be improved by using a different GS model. With the use of a more accurate G_0, such as the numeric models shown in M10a, the values of γ would reduce, and approximation (1) would enter the range of its formal validity.

2.1 Physical basis

While criticizing the general idea of variable GS, X10 focuses on the exponential correction to G_0 in (1). This form is stated to have "no physical basis," principally because of its decaying too quickly at large times. However, as explained in M10a, (1) represents a scattering-theory approximation, in which the rate of GS variation, $\delta G/\delta t$, is proportional to G. Similarly to all scattering models, this model should not be used at the limit of $t \to \infty$. Maybe not appreciating this point, X10 applies the $e^{-\gamma t}$ correction to ~300-km distances (his Appendices 1 and 2), whereas it was only proposed for up to 50–70 km, and also uses greatly exaggerated values of γ and large times in his Figure 4. Note that for focusing, γ should also be negative in this Figure. Finally, if the limit $t \to \infty$ is not considered, then both functional forms G_0 and $G_0 e^{-\gamma t}$ in (1) are equally acceptable, because G_0 itself is also only an *ad hoc*, reference approximation for the GS.

As an example of a "physically meaningful" GS, X10 offers an analytical model of a wavefront spreading in a 2-D structure with a linear velocity gradient (Appendix 2 in X10). However, this derivation is incorrect, which only illustrates the difficulty of mathematical approaches to GS. Formula (18) in X10 reads $G(x) = |x|^{-1/2}$, which means that the wavefront remains cylindrical in shape while propagating faster in the positive than in the negative-x direction. The resulting GS is consequently azimuthally-variable in time but not in space (Eqs. 22–23 in X10). Such wavefronts violate the Snell's law and are impossible. In reality, the ray-theoretical GS occurs in space and is caused by varying wavefront curvatures (*e.g.,* Červený, 2001), and for weak perturbations, it leads to our exponential approximation (1) (M10d). Note, however, that the direction of focusing (with rays bending toward the negative x) is correctly reproduced by the derivation given in X10.

2.2 Measured or assumed?

The presumption that GS can be established by mathematical modeling and not measured makes the basis of the conventional practice of frequency-dependent Q measurement, which is defended in X10. However, a realistic GS is practically impossible to define mathematically. Any controlled-source data section shows that "multi-pathing" (reflections, refractions, and mode conversions) is so pervasive for body waves that the wavefronts that could be followed for GS prediction are completely absent. At the same time, despite the absence of tractable mathematical formulations, the GS exists physically, and it can be easily measured empirically, as described in M08, M10a, M10b, and M10d. In practical observations, we only have the frequency-dependent attenuation coefficient to go by, and therefore we can only approximate the residual GS as the frequency-independent part of the attenuation coefficient, denoted γ in M08. Unfortunately, the effects of small-scale scattering cannot be unambiguously separated from this residual GS (M08, M10a), which is briefly discussed below.

3. In situ Q and viscoelasticity

The viscoelastic theory is often utilized as an implicit conceptual support for a frequency-dependent *in situ Q*, which is also used in X10. This is a very extensive subject that cannot be fully addressed here; however, two observations regarding this support were made in M09c. First, note that the quality factor was introduced in seismology from an intuitive analogy with acoustic or mechanical resonators (Knopoff, 1964) but it is still not the type of quantity that can be easily associated with a point within a medium. Rigorous physical theories, such as the model of saturated porous rock by Biot (1962), do not use Q to describe the medium. The difficulty of defining a "material Q" can be easily seen from the fact that there exist many factors responsible for elastic-energy dissipation, such as the rock-matrix properties, grain shapes, pore volumes, shapes and connectivity, fractures, fluids, and various dielectric and piezoelectric properties. Most of these factors are unrelated to the elastic moduli, and they cannot be lumped into the only two values of Q_K and Q_μ used in the traditional viscoelasticity. Thus, the viscoelastic model is *too simple* to describe real Earth materials.

Second, the viscoelastic model is also *too general* and permissive. It only reproduces the formal convolutional relationships between the strain and stress within a wave but does not constrain any mechanism of energy dissipation. Nearly any linear process can be presented as "viscoelastic." For a simple analogy, the ordinary Newtonian mechanics can be described by convolutional integrals relating the particle position to force history, yet such a picture does not replace the second Newton's law. Another fundamental problem of viscoelasticity is the absence of a unique definition for the elastic energy (*e.g.*, Carcione, 2007), which leads, for example, to incorrect complex argument of the acoustic impedance in the presence of attenuation (M10c).

4. Frequency dependence of the attenuation coefficient and Q

Turning from the basic principle (1) to its consequence, which is the frequency dependence of the attenuation coefficient, χ, X10 correctly points out (his Section 5.1) that it can be fit by using a frequency-dependent $Q(f) = Q_0 f^\eta$ (*i.e.*, $\chi(f) = \pi f^{1-\eta}/Q_0$) as well as by a linear dependence $\chi(f)$:

$$\chi(f) = \gamma + \pi f / Q_e \,. \tag{2}$$

The same was shown in M08, where a mapping between parameters (γ, Q_e) and (Q_0, η) was also derived. From this equivalence, X10 argues that the observed linear form of $\chi(f)$ "does not invalidate" the power-law $Q(f)$ above. Indeed, Eq. (2) does not invalidate the power-law or any other form of $Q(f)$, but then, why is such $Q(f)$ really required? This remains the big question, because it appears that $Q(f)$ is only suggested by the viscoelastic theory. The true reasons for using one attenuation model or another are not in the data fit but in their correspondence to the physics of wave propagation (M10a). For viscoelasticity, such correspondence can be seriously questioned, as noted in the preceding section.

In his subsequent argument (Section 5.2), X10 finds a "fundamental contradiction" of the linear $\chi(f)$ model (2) in the fact that among the several major long-period Rayleigh-wave models, it only agrees with PREM. Because this example was not analyzed before, it is instructive to consider it here. As Figure 1 shows, all of these four models cannot be accurate simultaneously, and they differ among themselves to about the same extent as from the linear $\chi(f)$ model (PREM in Figure 1b). Moreover, γ and Q_e in (2) are apparent quantities, and their constancy is just an empirical observation that does not have to apply to the entire frequency band. The differences between the $\chi(f)$ for these models consist in γ and Q_e changing at frequencies near 10–15 mHz, similarly to their variations at short periods discussed in M08. Note that the χ (Figure 1b) is the quantity that is measured from the data and inverted for. After its transformation into Q, the relatively modest (~20%) differences between the four $\chi(f)$ models are accentuated and lead to Q decreasing at low frequencies (Figure 1a).

An important argument in favor of a frequency-dependent Q within the mantle comes from lab measurements using rock samples (Section 5.3 in X10). Nevertheless, one should not think that the GS does not exist in lab experiments, or that such measurements are unaffected by elastic structural effects. Quite oppositely, the transformation of the measured quantities, which are the resonance-peak widths or strain-stress phase delays, into the inferred "material Q" is most complex for lab observations and relies on the most intricate models and numerous data corrections (Bourbié et al., 1987). These models cause pronounced effects in the elastic limit (M10d) and play the role of the GS.

5. Scattering

When considering scattering, it is important to differentiate between two meanings of this term: (i) as the process of wave propagation in a smooth background structure with random heterogeneities, and (ii) as a general theoretical approach for describing physical systems with weak interactions. In interpreting scattering, seismologists usually intuitively visualize it as "random scattering" (i), as consistently illustrated by the use of this term in X10. Nevertheless, in theoretical modeling and inversion, meaning (ii) is used, which is much broader than (i). In seismic-amplitude data from which the Q values are derived, there usually is very little information to prove the random character of the structure. Ray-bending in weak velocity gradients or reflections can also be modeled by using the scattering theory (M10d).

As explained in M10a, the attenuation-coefficient approach of M08 is of such scattering-theory type (ii). This is the exact physical meaning of the exponential approximation (1). Accordingly, within the context of attenuation observations, "scattering" represents *all* deviations from the reference model, and among these deviations, the strongest contribution should likely belong to the deterministic effect of the structure. As correctly noted in X10, scattering is inherently "confused" with GS, *i.e.* it represents its stochastic, small-scale part. Our primary concern is therefore to differentiate the combined scattering and GS from anelastic attenuation (M08, M09a, and M10a).

6. Scattering Q

X10 also emphasizes another important argument against measuring the variable GS: "… at sufficiently large distances, the cumulative effects of … velocity heterogeneities … cause exponential decay of amplitudes, … <which> is already statistically quantified by using scattering *Q*… <underline my – I. M.>." However, the distances at which such major features as the Moho and velocity gradients could be treated statistically are not attained in seismological observations. Describing such unique structures statistically distorts the interpretation and generally makes the scattering *Q* only an incomplete representation of the large-scale structure (M09a, M10a). At the same time, if one is only interested in matching the seismic amplitudes, the frequency-dependent scattering *Q*, as well as many other models, can adequately fit the data.

7. Conclusion: attenuation coefficient

The key to unambiguous interpretation of seismic-attenuation data appears to be in treating the GS, scattering, and anelastic attenuation together, as parts of a single attenuation coefficient, χ. This removes the uncertainty related to GS model assumptions. Near-linear trends with frequency are revealed within a broad range of frequencies (M08, M10a, M10b) and lead to simple and consistent interpretations.

In conclusion, my answer to the question posed in the title of X10 is that *yes, we can* drastically improve the estimates of *seismic attenuation*, provided that we: 1) do not rely solely on *Q*, and 2) are not prejudiced in favor of certain theoretical models. If we *measure* the unknown GS by using equations (1–2) or similar instead of making various assumptions, we can obtain a fully quantitative and physically clear picture of attenuation. Once *Q* is removed from the theory, the issues of its trade-off with GS and dependence on assumptions and measurement methods disappear. As a secondary result, once a significant portion of today's *Q*(*f*) is recognized as related to the structure, the remaining Q_e may often become frequency-independent.

Finally, I cannot agree with a side remark in X10 about the book by Bourbié et al. (1987) being obsolete merely because it does not discuss the ~1-Hz frequency band. On the contrary, by the depth and precision of its theoretical analysis, attention to the physics of wave propagation, and clear descriptions of many laboratory techniques, this book stands out among many other texts on seismic attenuation. All seismologists interested in the true mechanics of seismic wave propagation should study this work very carefully.

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Figure

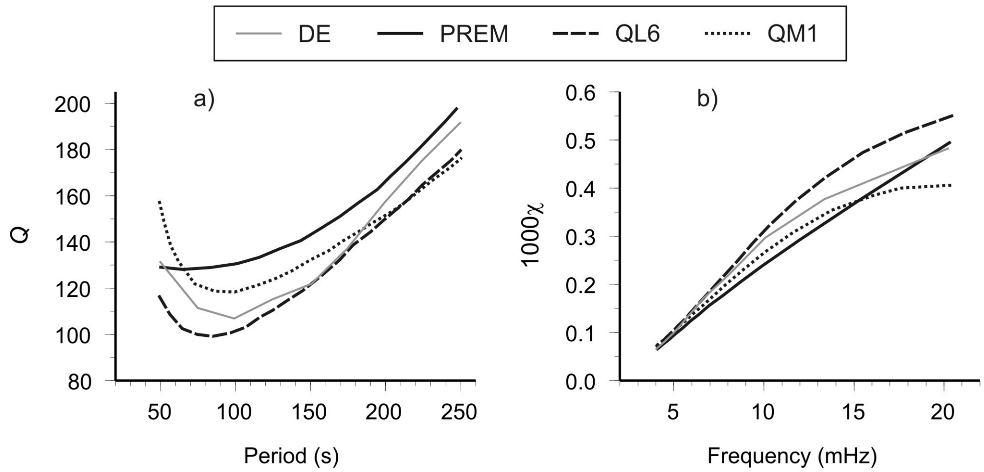

Figure 1. Apparent attenuation for four spherically-symmetric global Rayleigh-wave models: a) in $Q(f)$ form, b) in $\chi(f)$ form. Model labels: DE (Dalton and Ekström, 2006), PREM (Dziewonski and Anderson, 1981), QL6 (Durek and Ekström, 1996), and QM1 (Widmer et al., 1991). Note that $\chi(f)$ for PREM is near linear across the entire 50–250-s period band.