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Review of Doctoral Thesis

Seismic waves in inhomogeneous, weakly dissipative, anisotropic media

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General Background and Content of the Thesis

The main methods to determine the structure of the Earth, and thereby improve understanding of the Earth's dynamics, is by using seismic wave propagation. In particular, seismic imaging (in seismic exploration) and travel time tomography (in seismology) have been very successful. However, despite these successes, there is still a significant amount for improvement. The Earth on all scales seems to be generally anelastic, so that both anisotropy and anelasticity have to be taken into account. Determination of the anelastic parameters however, is quite complicated. First of all, this requires accurate forward modeling (and a physical understanding of the quite complicated wave phenomena that can occur in viscoelastic wave propagation), as well as a better understanding of the source.

This last point about the source brings up another important aspect of seismology. The more one looks into waveforms and wave propagation the more one is required to know about the source. Moreover, the source characteristics are of course very important by themselves and in fact there are still many open questions regarding how earthquakes work. Various earthquake models exist. The simplest, and a very useful one, is the moment tensor, which among other things, is very useful in distinguishing between purely tectonic events, in which an earthquake is described by a double couple model, and events that contain a significant non-double couple component, which typically is an indication that the event is more complicated. An earthquake model that goes beyond the moment tensor assumes that earthquakes rupture on a finite fault patch. This leads to a variety of sub-models, including Brune's model for a circular fault patch. Typically, these models are developed for elastic isotropic media. However, as noted above, the source characteristics can not be decoupled from the wave propagation so that when attenuation is taken into account in the wave propagation the effects of the source should also be considered.

Over the years many people have worked on various aspects of anelastic (or 'just' anisotropic) wave propagation. It is quite important however, to further improve the theoretical and numerical efforts for anelastic modeling and use that to push the data processing to a higher level. Both these topics, improving the theoretical/numerical modeling and application to real data, are done in this thesis. This is in contrast to many papers which focus on data processing using existing methods. In particular, this thesis consists of a theoretical and modeling component (chapters 1 and 2), with some significant new results, a chapter on the determination of the attenuation (*Q*) using earthquakes (chapter 3), and three chapters (4-6) on practical estimations of *Q* for different datasets. The first of these datasets concerns a natural event caused by CO_2 degassing, the second is an induced event in a conventional reservoir in Italy and the third one an induced event in a shale reservoir in China, which in contrast to the earlier two events, was recorded by many receivers. Thus, the thesis contains an interesting and important mix of theoretical and modeling results on one hand and applied data analysis results on the other hand.

General Comments on the Thesis

Most of the material of chapters 1-6 has been published, either as a scientific paper or as a report on the publicly available 'Seismic Waves in Complex 3-D Structures' repository. In particular, four papers were published with Miłosz Wcisło as first author, one of them a reply to a comment, and one paper with Miłosz Wcisło as second author. Miłosz Wcisło was first author on two of the reports and second author on a third report. For some reason the papers were not included exactly as published in the thesis. Instead, they were rewritten and moreover, not all material in the papers was included in the thesis. As a rule of thumb a PhD thesis should, besides containing interesting and original work, consist of three published/publishable papers and that is the case. Moreover, as noted above, the topic of the thesis is important and the approach more original and broader than many papers in the field. In other words, the author of the thesis has demonstrated an ability for creative scientific work and I therefore recommend this work to be accepted as a PhD thesis. As an aside, it should be noted that the rewriting, and also some of the formatting, is not quite optimal, as there are many typos/stylistic/grammatical inaccuracies. This is a bit unfortunate as it distracts the reader from its contents. In the following paragraphs, I briefly discuss various aspects of the content of each chapter and this includes a number of comments/questions. Some of these are more general, others are more specific.

Chapter 1
In this chapter the author presents a method to compute seismograms through smooth weakly attenuating anisotropy media using ray theory. This method is used to study various aspects of attenuation on seismograms. The attenuation model used in the first part of the thesis is the Futterman model. This model is attractive, as the author also explains, because in this case the attenuation factor *Q* is frequency independent, so that attenuation is only represented by one parameter, but at the same time also causal. The seismograms indicate the three effects that the Futterman model of attenuation has on waveforms as the wave propagates: the pulse gets broadened, the amplitude decays more than in the isotropic case and there is a delay (compared to the isotropic case). All these effects are described in terms of the parameter t^*, which is the integral of inverse *Q* along the ray path. The main emphasis in the rest of the thesis is on the first effect, though also some attention is given to the other two effects.

Questions:
1. A weakness of the Futterman model is the presence of a reference frequency. The modeling results depend on the value of this reference frequency. The author takes the value of the reference frequency to be the same as the value of the dominant frequency of the wavelet. It would be interesting to know whether the author has any comments on this and, in particular, whether there are any significant changes in the seismograms if this reference frequency changes.
2. A related issue is the choice of the wavelet. In sections 1.1-3 a Müller signal is used whereas in section 1.4 a Ricker wavelet is used. It would be good if the author could comment on the choice of the wavelet, both in these sections and also in general, and how/whether this effects his modeling results.
3. The Green tensor for an isotropic homogeneous medium with a moment tensor source contains, in addition to far field terms, also intermediate and near field terms. This can be seen, for example, from the expression 9.13.2 in the textbook 'Elastic wave propagation and generation in Seismology' by Pujol. In a smoothly varying (an)isotropic medium one would expect similar terms. The author uses ray theory, which roughly means he only considers the far field terms. It would be good if the author could comment on the validity of this (e.g. for which distances/frequencies would ray theory be valid).

Chapter 2
Velocity models in seismology consist of a combination of smoothly varying media with sharp interfaces. This is a consequence of the way the Earth is built up: on many scales, the Earth consists of a number of layers (which may not always be flat) with smoothly varying inhomogeneities within the layers. An example, relevant for this thesis, are sedimentary layers, possibly with the inclusion of hard bedrock underneath. The consequences for wave propagation are that modeling methods need to be able to deal with smooth heterogeneities as well as discontinuities. The former was discussed in chapter 1, and it is logical that the latter are therefore discussed in chapter 2, both, naturally, in the context of anelastic wave propagation. This chapter consists roughly of three parts: a theoretical part, computation of reflection and transmission coefficients and the computation of seismograms and the results are very interesting, though perhaps there still is quite a bit of work to be done.

Questions:
1. The Brewster angle is mentioned a few times. However, it is not defined. It would be good if the author could define it and explain its significance.
2. Despite quite a bit of work by the author and his co-workers, and others mentioned in this chapter, on this topic there still are quite a number of fundamental problems. In particular, the correspondence principle is perhaps not applicable. It is possible to make some adjustments, as the author shows, but these are not very satisfying from a theoretical point of view. Perhaps the author can comment on a possible way out of this problem.
3. An issue related to the previous point, is that the derivation done by the author (and others) assumes plane waves. However, in practice, the waves from a point source (as used also by the author in his modeling) are not plane but spherical. This quite possibly has to be taken into account when deriving (more) realistic expressions for the R/T coefficients (using for example the Sommerfeld integral). A number of papers on this topic have been written for the (an)isotropic case. Would it help if this approach also was used in the derivation of the anelastic reflection and transmission coefficients? And, in particular, would this help with the difficulties at/near the critical angle and also, perhaps partly, explain the differences between the seismograms computed using the fully numerical method and the ray method with the plane wave reflection/transmission coefficients?

Chapter 3
Chapter 3 is a transition type chapter and consists of one main part and two smaller parts. The main part is related to the determination of (effective) *Q* using the broadening of pulses in seismograms as they propagate away from the source (the first of the three effects of *Q*). This is the main method used in the practical applications of chapters 4-6 and therefore deserves some justification. Moreover, there was a comment by Morozov on the paper

and some of this is addressed by the author in chapter 3. Because of the close relationship between chapters 3 and 4 I ask the questions these chapters in chapter 4.

Chapter 4
In this chapter the author applies his method to measure peak frequencies and then determines *Q* on a dataset from West-Bohemia. This is an interesting dataset as there is a considerable amount of seismicity in this region which is interpreted to be caused, at least partially, by movement of CO_2, which originates in the mantle, through the crust. The author concentrates on two swarms that happened in 2008. He first shows that the measured peak frequencies are independent of the magnitude of the events. This makes it possible for him to apply his method to determining Q_p and Q_s for each swarm and a number of stations. The author shows that there is a decrease of *Q* between the dates that the events occurred (10 and 28 October) and interprets this to be related to CO_2 movement. The author also shows, in a further analysis, that this trend in *Q* is confirmed by data from other swarms that occurred on 19 October and 24 November.

Questions:
1. Morozov commented on the author's 2018 BSSA paper in a 2019 BSSA paper. Some of his criticisms seem more valid to me than others. For example, the receiver effects, are discussed in the BSSA paper, and seem less important here (however, see my question on chapter 6). Morozov mentions the Brune model as an improvement and the author shows that both for the case of a flat spectrum before the corner frequency as well as a Brune spectrum his method of determining the peak frequency is valid. Does the author think that his method will still be valid if more realistic dynamic rupturing models are applied?
2. The Brune model assumes the earthquake consists of sudden displacement along a circular patch of the fault. However, the application in this chapter is to events related to CO_2 degassing. I would have thought that this means the events have a significant non-double couple component, in which case the Brune model is less applicable. It would be good if the author could comment on this and, in particular, if the Brune model (and specifically the source spectrum) is not applicable then what source model, would be applicable.
3. Morozov correctly mentions that often to determine *Q* spectral ratios are used, so more than one recording, and that this seems to be quite stable. The author uses one recording for a single *Q* measurement instead. For this dataset, or perhaps the datasets of chapters 5 and 6, it should be possible to use the spectral ratio method to determine *Q* and compare that with the author's method. It would be good if the author could comment on this.
4. Morozov's comment on the author's 2018 BSSA papers also include some remarks about using pulse-broadening to estimate *Q* (see for example the paragraph following Morozov's equation 12). The author does not seem to address that in his thesis. Perhaps he can comment on this.

Chapter 5
In this chapter the author estimates *Q* on a dataset from a reservoir in southern Italy. In this case the seismicity is caused by injection of wastewater into the subsurface. Also in this case the authors use their peak frequency method to determine *Q*. Also here, an interesting time dependency of the measurements, the V_p/V_s ratio, rather than Q, which is correlated with the pumping pressure, is noted.

Questions:
1. The author estimates Q_p and Q_s in chapters 4 and 5 and Q_p in chapter 6. Typically though, the effects of attenuation on Q_s are larger than on Q_p. Perhaps the author can comment on the relative importance of determining Q_p and Q_s (in terms of fluids and gases for example)? Some information is given on page 108 but it would be good if that can be expanded a bit more.
2. Generally speaking the results and the interpretations make sense to me. It seems perhaps a bit unsatisfying that only a few values of *Q* are estimated here. Would it be possible, using either this method, possibly combined with another method (like the increase in travel time or decrease in amplitude) to get a more detailed *Q* model? Would additional measurements be required (such as more stations or more events)? A similar question could be asked for chapters 4 and 6.
3. The author mentions saturation a number of times in this chapter. However, he does not really explain what is meant by this. Does saturation mean gas saturation?

Chapter 6
In this final chapter the author applies the peak frequency method on a dataset from a reservoir in China. This dataset is a bit different than the ones used in the previous two chapters: there are many (1771) receivers, which are arranged in a star like pattern consisting of 12 arms radiating from the centre and there is only one source (magnitude of just over one), which was located almost right below the centre. This dataset makes it possible to do more detailed measurements than in the previous chapters. These are used to determine different rupture directions and the rupture velocity. The estimated rupture velocity indicates that this event might be super-sheared. Super-

sheared events typically are observed for large earthquakes and as far as I know it is quite rare to determine it for a small event, which makes the paper even more relevant.

Questions:
1. The main topic of the 2nd half of the author's thesis, chapters 4-6, is the use of seismological data to determine effective *Q*. Generally, in seismics/seismology, there are a number of issues related to *Q*, for example what exactly does *Q* mean for a given type of data/waveform etc., as pointed out by Morozov in his 2015 paper in GJI. Perhaps the author can put chapters 4-6 in the context of this paper and indicate how his research relates to the various types of *Q*, the 'taxonomy', mentioned in Morozov's paper.
2. Morozov's criticism of the author's 2018 BSSA paper with regards to receiver effects could be partly valid in this case. The velocity model in figure 6.2, with the source being at 3300 m depth, consists of a more or less constant velocity, which then gradually decreases, until there is a region with relatively low velocity at a depth of several hundred meters. Presumably, this low velocity layer is caused by sediments. It would be good if the author could comment on this.
3. The author shows the travel time as a function of epicentral distance in figure 6.7 and mentions that there is no significant (azimuthal) anisotropy in this case. However, the data shown are only for 5 out of 12 arms and these arms roughly are N-S oriented. I assume that the travel times for the other arms also fall on the same line. Is that correct?
4. The reservoir is a shale reservoir and shale is strongly anisotropic. Why is there no anisotropy? Is that because the source is at the top of the reservoir and the overburden does not consists of shale or other anisotropic rocks?

I look forward to having a discussion with the author on the issues raised above.

Sincerely,

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