General comments

The manuscript describes a suite of tools for the modelling of mechanical anisotropy, both its development for a given mineralogical model under geodynamic strain and the evaluation of various seismic parameters. The tools are (mostly) not new, but have been collated and refactored to make building them into a workflow simpler. The process of this workflow has also been nicely documented in an extensive manual for the suite, and several instructive examples.
Such activities are often not well documented and shared, so I appreciate the extra efforts the authors have made to make their work accessible and useful to the community. While there are of course some limitations to all of the tools in the suite, there are a large range of important, interesting problems that can be tackled with these tools either singly or together which make the publication of a manuscript describing them worthy.
I have some specific comments which I think that the authors should address before considering the manuscript finalised. These are (mostly) focussed on the seismic end of the problem. However, I would also like to endorse the general comments made by RC1, and agree that the two missing sections identified would make the manuscript much more complete. A version of record on Zenodo (or similar) would also be sensible.

Thanks for the comments, we have carefully addressed them as reported in the replies to RC1's comments, and as the following:

Specific comments

Line 30-33, sentence beginning "Seismological and ...": This implies that the suite presented does something different, which – as far as I can see – is not really the case. The calculations of (for example) SKS-splitting here is not done on the original geodynamics grid, for example. I'd also argue that this is not really as definitively a problem as stated, either.

The true and most important novelty of ECOMAN is that it allows to perform P- and/or S-wave isotropic and anisotropic tomographies using real datasets or synthetic datasets generated from the geodynamically-derived elastic tensors and densities. To our knowledge, this is unprecedented, except for the isotropic approach employed by some groups and that is cited in the following sentences (Styles et al., 2011; Schubert et al., 2012; Maguire et al., 2018).
We respect the opinion of the reviewer, but we believe that the problems mentioned in that sentence (in particular, the validation of geodynamic models against observations, and the interpretation of the imaged seismic structures in terms of geodynamic processes) are of paramount importance, and indeed dedicated sessions are routinely organized in the main scientific meetings (e.g., https://meetingorganizer.copernicus.org/EGU24/session/49108).

Line 81, s/b "At the same time ...": These are not the earliest examples of 3D anisotropy inversions. There are a range of these in the literature, dating back (at least) to Panning et al (GJI, 2006) for global models, shear-wave splitting (Abt and Fischer, 2008; Long et al, 2008; Wookey, 2012), and surface waves (Debayle et al, 2005), to name but a few.

Here we meant that the methods recently developed by a few groups (us, Prof. Zhao's and Prof. Plomerova's) are capable of simultaneously recovering for the isotropic velocity anomalies and seismic anisotropy for arbitrarily oriented fabrics. Previous methodologies such as those employed by Panning et al., (GJI, 2006) and Debayle et al., (2005) are capable of inverting for either the radial or azimuthal components of the shear wave anisotropy, while those presented by Abt and Fisher (2008), Long et al. (2008), and Mondal and Long (2019, GJI) can only retrieve the anisotropic component of S-waves.
Nevertheless, we acknowledge the importance of these previous studies and mention them in the text.

Line 279. s/b "the fraction of ...": It might be worth noting that the tensor 'fractions' from this approach have been suggested to not be a good approximation (Tape and Tape, Journal of Elasticity, 2024).

We are aware of the work by Tape and Tape, 2024 (see their acknowledgements), and we clarify this problem in section 3.2 about the current software limitations.

Section 2.3.1: What equations are solved to calculated the splitting? How is the frequency included?

The methodology behind the SKS splitting calculation is now briefly explained in section 2.3.1. The methodology details have been already described in Schulte-Pelkum and Blackmann, 2003 by the software developer.

Line 445, s/b: The underdetermination is inherent in the problem, at best MC sampling might be able to constrain the uncertainty due to the underdetermination, which is I suspect what the authors mean by this, but it should be clearer.

Right. We have clarified this aspect accordingly.

Technical corrections

Lines 318, 438: font issues

This was a problem that occurred during PDF conversion, now fixed.

Line 39: 'in' -> 'of'

Corrected

Line 415: It is more useful to describe MATLAB and Python as *interpreted* languages, rather than high level. C is also technically a high level language, and is as fast or faster than Fortran.

Corrected

Line 417: '100.000s' -> '100,000s', or use words to avoid decimal point confusion.

Corrected

Line 424: 'it's' -> 'its'

Corrected

Line 451: 'surface waves' -> 'surface wave'

Corrected

Thanks for the valuable feedback

Manuele Faccenda, on behalf of all co-authors