**Ms. Ref. No.: EPSL-D-14-00772**
**Title: On the robustness of estimates of mechanical anisotropy in the North American continental lithosphere**
**Earth and Planetary Science Letters**

**Response to Reviewers**

*Line numbers refer to the numbers in the submitted manuscript, as referenced by the reviewers, 'revised Line numbers' to the current version. We have generally tried to provide both where possible.*

**Reviewer #1:** Kalnins and colleagues present a careful re-analyses of gravity-topography spectral methods methods as used to detect mechanical anisotropy in the averaged, or effective, elastic thickness of continental lithosphere. Over the past 5 decades of admittance-coherence studies, more robust spectral estimation methods have been applied and developed, but not always with testing of 2-D synthetics. Spectral estimation methods can be used to determine mechanical anisoptropy, as demonstrated in Tim Bechtel's 1989 PhD thesis, which raised the question of detecting mechanical anisotropy vs anisotropy of topography and/or gravity signals with power spectra of their coherence. Kalnins et al use synthetic Bouguer models of isotropic lithosphere and N American topography, and clearly demonstrate their impact on the estimation of effective elastic thickness vs azimuth. Other parts of the paper veer off into literature review, and the structure of the paper swerves between something like Kirby's (2014) review paper, Simons and Olhede (2013), and the focussed report of new analyses. There is too much opinion or debate - best to let readers evaluate.

*We have tried to adjust the tone of various sentences, particularly those highlighted in the more specific comments, to reduce the sense of opinion/debate. We have also substantially reorganised the paper to constrain the background material more tightly to the Introduction and Method sections to reduce the literature element outside those two sections. However, given what is ultimately a quite technical argument leading to broad geological conclusions and the breadth of EPSL's readership, we feel some overview is necessary, especially for a complex and sometimes contested methodology.*

I also think that the 5th bullet point of the Highlights is over-stated - robust evidence in spectral estimation methods, yes, but there remain xenolith, seismic anisotropy, etc that are not evaluated in this study.

*We have rephrased the 5th bullet point to make it clear that we are referring to spectral $T_e$ estimates only.*

I would like to see the core of this paper published in EPSL, but the paper, as presented, could serve to confuse or even fan the debate regarding azimuthal anisotropy in mechanical properties of continental lithosphere. I outline some comments, suggestions, and questions that the authors may wish to consider in revision. They are intended to help shape this into a tight argument closing a door, rather than opening new. I think the authors are a bit cavalier in their treatment of the plates themselves, in part owing to fundamental omissions linking Te to plate rheology, the role of other factors (e.g., composition - Lowry and Perez-Gussinye, 2013), surface and subsurface loading, and tectonic domain averaging. Sure, the authors used 1400 km-wide boxes, but what tectonic or magmatic provinces fits precisely within that window? EPSL readers will want to see the full picture.

*We have added a brief paragraph introducing the concept of $T_e$ and its relationship to a complex plate rheology in revised Lines 12–20. The importance, and potential complexities, of surface and subsurface loading were originally mentioned in Lines 157–161; that is now earlier, part of the fundamental explication of the method (revised Lines 101–106).*

*The size and shape of data patches is a perennially difficult question for $T_e$ studies. Although we illustrate our analysis with moderate-sized 1400 km boxes, we present summary results from two larger sizes as well. We have also elaborated on the potential future role of arbitrarily shaped windows, e.g., to match tectonic boundaries, and our reasons for using a simple square in Lines 73–75 (revised Lines 93–97).*

1) Abstract and Intro: The first sentence isn't helpful. Maybe say something like - Tectonic fabrics imposed during orogenesis and rifting cycles of continental lithosphere may impart a mechanical anisotropy.

*We have changed the first sentence of the abstract to add more detail and highlight the link to anisotropy more clearly.*

As someone who has taught students about Te for many years, and who tries to communicate with mechanical engineers, effective elastic thickness doesn't directly translate to rheology or rock layering as detected by imaging or other techniques. The abstract and Intro would benefit from a clear statement of D, Te, and how this mechanical anisoptropy could potential be detected with seismic, MT, or xenolith data.

*We have added a brief paragraph introducing the concept of $T_e$ and its relationship to a complex plate rheology in revised Lines 12–20.*

Line 7 - Seismic anisotropy is accrued through several mechanisms, and the seismically fast directions may have no correlation with a mechanical anisotropy of elastic properties (e.g., aligned melt). It seems the authors have started with a particular interpretation (in response to Audet and Burgmann?) - better to give the overview of mechanical strength depends on thermal properties, composition, as well as strain rates (e.g., Burov and Watts, etc).

*We did not intend to imply that seismic anisotropy reflects exactly same properties as anisotropy in $T_e$, just to highlight the difference in the amount of attention paid to anisotropy of the lithosphere by these fields. We have modified that sentence slightly to try to make this clearer (Line 7, revised Lines 7–8).*

2) Again, the authors should be take time to place their work in broader context. The inverse models are one method to estimate plate strength, but predictive modelling of gravity and topography of well constrained profiles of foreland basins and rift zones are also reliable means to estimate plate strength.

*We have added information on forward modelling techniques in Lines 13–15 (revised Lines 22–24).*

3) Opinions and unnecessary amplification of debate - cut l) 29-34; 2) 72-78 - forget the debates with McKenzie - not relevant here

*We have removed the second sentence from Lines 29–34 (revised Lines 45–48) and modified the first to more clearly summarise how Audet & Burgmann linked anisotropy in coherence to tectonic boundaries, which was its purpose.*

*For lines 72–78 (parts of revised Lines 98–112), estimation of D and $T_e$ is a field with many complications and technicalities, and these lines are intended to clarify what aspects of this the present paper does and does not address and how its methods relate to those used in other papers. We feel that this is important and have chosen not to delete these sentences, but have modified them to try to make their role clearer. In keeping with the paragraph overall, it seems appropriate to note that we do not address the free-air method at all, but we have tried not to over-emphasise the controversy.*

4) f and D, Te - Lines 160-170 and 254-260 with equations are needed in the Introduction so

readers can understand arguments and preview the outcome - that the inherent directionality in topography and Bouguer anomalies represnting the isostatic compensation for the topographic loading introduces a significant azimuthal anisotropy in the estimated coherence. (Bechtel and I never reported measurements of anisotropy owing to the inherent directional bias in the data themselves). The ratio of surface to subsurface loading, the assumption of a compositionally (in terms of density) uniform plate, and that windows enclose provinces with uniform Te, are all important. I suspect that one of the reasons folks don't often use Te studies is the long-standing debates over these 'hidden' and fundamental assumptions. Lines 255-6 are key.

Lines 165-170 regarding choice of an assumed top to subsurface loading f = 0.5 - provide the reader with reasons for the confidence? It's refreshing to go back and read Bechtel and Forsyth, Nature, 1990 - a circumspect and honest appraisal of results relative to the several underlying assumptions.

*Lines 152–161 (revised Lines 30–34 and 98–105) have been incorporated into Introduction and Methods sections. For Lines 162–170, we have indicated our method for 'inverting' coherence for $T_e$ in revised Lines 107–109, but as the details are so interlinked with our geophysical significance test, we felt it reduced repetition to discuss them in the section on that test (parts of revised Lines 150–170). We have also elaborated on our choice of a constant $f^2$.*

*The key equation for the coherence (Lines 256–258) is now at the start of the methods section (revised Lines 56–60). The discussion of how anisotropy in coherence arises from anisotropy in gravity and topography (the remainder of Lines 254–260) is in revised Lines 184–188, which is part of the section covering the third test immediately after the introduction of the first two tests. (In the original manuscript it was introduced later.)*

5) Lines 114-115 - Stating what you are trying to prove? Cut opinion and focus.

*This sentence has been removed. (The remainder of the paragraph is now part of the Discussion, revised Lines 299-304.)*

6) 126 - missed a squared symbol on the gamma overbar.

*Corrected (revised Line 143).*

In summary, I see considerable merit in this paper, but think it needs a complete reorganization and shortening by about 30%, with a more circumspect approach that clarifies, rather than amplifies current debate.

Cindy Ebinger

*We have substantially reorganised the paper to try to present the general method, our proposed hypothesis testing, results from synthetic data, and the North American case study in a tighter, more self-consistent manner. We have also tried to increase conciseness, including reducing duplication between figure caption and text. However, the reduction in length of the original text is somewhat offset by the request to place this work in a broader context and provide more background on the fundamental equations.*

**Reviewer #2:** The manuscript: "On the robustness of estimates of mechanical anisotropy in the North American continental lithosphere" by Kalnins et al., is an interesting and detailed piece of work which is important and certainly merits publication. The authors envisage statistical tests to prove the robustness of mechanical anisotropy estimates using the coherence method. Coherence is estimated using multitaper and wavelet methods. I find the methods used robust and carefully chosen.

I have some comments of a more philosophical nature. This work shows that the anisotropic measurements presented in a series of papers over the last 10-15 years may be spurious. One of them was published by Audet & Burgmann's (2011) in Nature Geoscience. Using synthetic topography and gravity data, the authors show that anisotropic topography causes anisotropy in the coherence function, even when the flexural rigidity used to generate the synthetic gravity was isotropic. Hence, the authors discard all measurements of weak directions that are associated to anisotropic topographic features, which is a sensible thing to do. However, deformation of the lithosphere during extension or compression, causes anisotropic topographic features and most probably weakening along these topographical trends. Hence, even though coherence may not be a good method to measure mechanical anisotropy associated to anisotropic topography, this does not mean that the anisotropy does not exist.

Therefore, even though it may be true that, in general, papers like Audet & Burgmann's (2011), picked spurious anisotropic coherences that are not to be trusted, anisotropies along linear topographic features (fold-belts, rifts) may really exist, but can not be robustly measured with coherence, as the authors show. In a very similar paper Kirby and Swain, 2014, give a good discussion on this topic, which the authors could refer to. I think Kirby and Swain, 2014 paper needs to be referred to in other instances, for example, when the authors talk about the anisotropic directions being in the direction of the maximum Te gradient, this discussion and the corresponding tests are further developed in Kirby and Swain (2014) and the reader should be pointed to this work.

*We fully agree with the reviewer that many of the geological processes that produce marked anisotropy in the gravity and topography fields could easily produce anisotropy in the strength of the lithosphere as well — in some cases, such as orogeny and rifting events, it would be more surprising if they did not. This third test is thus more ambiguous geologically than the first two; however, with the present techniques, it cannot be ruled out that they are artefacts. They may be real, but they may not be, and hence cannot be considered robust. We have tried to highlight this in revised Lines 200–202 and 250–254 (immediately after original Lines 238–241). We have also modified Figures 4 and 5, as well as Supplementary Figures 1–9 showing the results before this third test is applied so that readers can see the extent to which such alignment occurs and judge for themselves.*

*References to Kirby & Swain (2014) has been added to the $T_e$ gradient discussion in Line 320 (revised Line 308) and to the discussion of aligned anisotropy in gravity/topography and lithospheric strength in Lines 324–332 (revised Lines 310–318).*

Minor comments

1- Line 78- Please cite Perez-Gussinye et al., JGR, 2014, as they also contributed to solving the controvery between using Bouguer coherence and free-air admittance.

*We have added Pérez-Gussinyé et al. (2004) to the list of references in Line 78 (revised Line 101) — we have assumed the year 2014 above was a typo, as the 2004 article fits the description, and we could not find a 2014 article that did.*

2- Lines 125-170 express something simple in a lot of space. I would recommend shortening this section.

*In Lines 125–151 (revised Lines 138-144 and 171–179), we have tried to reduce some of the duplication between words and mathematical symbols and between text and figure caption to make it more concise, but we feel that sometimes such duplication is useful to reduce confusion between, for example, the azimuthal, radial, and grand averages of coherence.*

*As mentioned in our response to Cindy Ebinger's comments, Lines 152–161 (revised Lines 30–34 and 98–105) have been incorporated into Introduction and Methods sections. For Lines 162–170, (parts of revised Lines 150–170), these have been substantially rewritten, but as Cindy asked for further explanation of some aspects, they are not necessarily shorter.*

3- Lines 238-241: I am not sure that neglecting directions which coincide with marked anisotropy in the topography or/and gravity is a good solution. I understand that tests with isotropic D and anisotropic topography produce anisotropic coherence, and this is why you are neglecting these measurements. But is there not a way to see if this anisotropic measurements are real or spurious? What happens if you make a synthetic test with an anisotropic topography and a weak Te trend along that topography? Does the resulting magnitude of the anisotropy change with respect to the isotropic plate? It would be very useful to have a way to use coherence to measure anisotropy in these cases.

*As mentioned earlier, we have tried to highlight this in revised Lines 200-202 and 250–254 (immediately after original Lines 238–241), and have also modified some of the figures and supplementary figures showing these aligned directions.*

*It might be possible to use synthetics to tease out whether aligned anisotropy is real or spurious, but it would be far from trivial: you would need to generate synthetics essentially on a point by point basis, taking into account the observed topography and isotropic $T_e$. There is also the question of additional anisotropy in the gravity data due to structures at depth that are not reflected in the surface topography. One could then test whether the direction and $T_e$ anisotropy from those synthetics were significantly different from the observed. We agree with the reviewer that it would be very useful to be able to do this, but we think it is beyond the scope of this paper to try to incorporate it here.*

4- Line 232-236: please check that the percentages of spurious coherence are coherent with those given in the figure caption.

*The percentages given are consistent: for the two datasets, 100% and 58% of the spurious directions that survive mathematical significance testing are removed by geophysical significance testing. A further 31% of the latter are removed by the topo/grav anisotropy bias testing, for a total of 89% (given in Lines 238-241, revised Lines 248-250). The figure caption reports only the final percentages, 100% and 89%, for brevity.*

5- Lines 337-339: Please see my main comment.

*We have added a sentence and edited the final sentence to make this caveat clearer (Lines 336–339, revised Lines 322–327). See also our response to the main comment.*