

## ***Interactive comment on “The sensitivity of GNSS measurements in Fennoscandia to distinct three-dimensional upper-mantle structures” by H. Steffen and P. Wu***

**H. Steffen and P. Wu**

holger-soren.steffen@lm.se

Received and published: 21 May 2014

**Comments to the reviewers’ report** on ‘The sensitivity of GNSS measurements in Fennoscandia to distinct three-dimensional upper-mantle structures’ by Holger Steffen and Patrick Wu submitted to Solid Earth.

We have revised the paper taking into account both reviewers’ comments. Below follows our response to the individual comments (marked in *italics*) by this anonymous reviewer.

### **Anonymous reviewer #2**

C1207

1. *As it stands, the paper also fails to provide convincing evidence that the results are more than a small ‘epsilon’ improvement over what has been done in the past and I offer suggestions of how this might be improved. ... The very poor use of proper grammar and sentence structure undermines the scientific presentation. This can be improved by the authors by performing iterative reading and rewriting (as most of us do) or by using a formal technical writing assistant for English. (It is not the reviewer’s job). The paper needs a major revision before being acceptable to publish. ...Finally the paper is too long for it’s content and the Discussion and Conclusion sections need to be shortened.*

We agree with the reviewer. We have completely revised the text and made major improvement to the English. We have also shortened those sections. Furthermore, we have added to the text to make clear that this work is not a small improvement over past works.

Many of the Detailed Comments are about the English, which we have completely revised. Below, we just list the comments not related to the English and give our reply.

2. *V. A recommendation is given at the end of the abstract: to give more weight to certain stations. Why not go further and recommend clusters of new stations that can heighten the sensitivity of geodetic observations to lateral heterogeneity?*  
This is included in the new version.
3. *VIII. Line 23-26 The fundamentals of Frechet kernel sensitivity is also (and mostly) developed in seismology. Please reference some thing like Dahlen and Tromp (1998) Theoretical Global Seismology, page 337, or/and Sneider, R. (1993) Global inversions using normal modes and long-period surface waves, Chapter 3 of Seismic Tomography: Theory and Practice (ed. H.M. Iyer and K. Hirahara), Chapman and Hall, London, pp. 842.). It is a little wrong not to acknowledge that the tiny GIA-community were not the ones to invent this.*

C1208

Done.

4. *XIII. Line 1 of page 2393 Please explain in more detail what the new viscosity structure model is and why you think it is better than what was used previously. The reference is to a 16-year old paper (Ekstrom and Dziewonski) . Why could this be a source for such improvement?*

We have now clarified what is new. This paper does not introduce a new viscosity model. Previous work on sensitivity only include a single rectangular block that is different from the reference model. The shape of the lateral structure as shown in seismic tomography models is not taken into account. This paper rectifies that problem. Also, in this preliminary study, we are not interested in small scale heterogeneities in the mantle, otherwise a lot of small mantle blocks have to be studied. S20 model is appropriate for the scale of heterogeneities that we are interested. Also, we have compared the structure in S20 below Fennoscandia and the gross features there in Kustowski et al. (2008, GJI). The differences are not significant, meaning that S20 is still giving a true view of the long-wavelength structure there.

5. *XIV. In this same paragraph it should be stated explicitly what the current accuracy is of the velocity data used here.*

That is an irrelevant question. The problem is that the reviewer assumed that the threshold value of sensitivity is related to the accuracy of GNSS measurements. We have clarified this in the version. Please also see item 4 in our reply to reviewer 1.

6. *Modelling I. The lower mantle viscosity is set to  $2 \times 10^{22}$  Pa s. Please motivate. Why?*

We have now given the motivation which is explained in Steffen and Kaufmann (2005).

7. *II. The authors have detailed upper mantle layering. Paulson, Zhong and Wahr C1209*

*published a paper some years back stating that lateral heterogeneous viscosity (and its indeterminability) makes it impossible to trust such fine layers. Please comment.*

In the models of Peltier (1998) or Mitrovica and Peltier (1993), their 1-D viscosity model contains many layers and the thickness of each layer is much finer than the radial resolution of GIA data. The high number of layers is clearly not warranted. However, Paulson et al. goes to the other extreme when they claim that only 2 layers can be resolved. The work of Lambeck and colleagues and our study find that GIA data can resolve 4 layers in the upper mantle.

8. *IV. It seems that the sensitivity for only ONE among a large (almost limitless) ensemble of relations to get from shear wave velocity to rheology is examined here. Why not examine another? This should really be explored. I would not think that this would be so much work.*

Several such relationships have been investigated in Steffen et al. (2006).

9. *V. Page 2397. It is mentioned that the threshold is 0.015 mm/yr on line 12. This seems very unrealistic if for no other reason than for the existence of unmodelable geophysical sources. It would therefore seem to be important to explore how the series of sensitivity tests work out (map-wise) if the threshold were raised by a factor of 6. Doing this is important to the disposition of the submission after the second round of reviews – at least from this reviewer.*

Please see item 4 in our reply to reviewer 1.