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Interactive
Comment

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

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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 #1
The reviewer has expressed several concerns regarding the model set-up used in our

manuscript. Also, he/she notes that our chosen threshold value appears unrealistic and criticizes the language. He/she eventually recommends a rejection of the manuscript. The reviewer has provided two additions (RC965 and RC1040) to his/her first review (RC932) after response (C961 and C970) by the second author of the manuscript. In the following, we extract and answer the major concerns of the reviewer from these documents. Reviewer notes (corrected for spelling mistakes) are in italics.

We begin with the first review (RC932) and its 5 major concerns. Patrick Wu has already replied in a response to them. We base our reply on this response and add information from the other reviews and replies in case they address the issue. We have to add, however, that we are sometimes quite irritated as the reviewer not only attacks our manuscript here, but also some of our earlier work. We will show in the following that the reviewer did not read the manuscript and papers in question carefully.

1. *Produce a prediction of deformation rates based on the no-variation model. Explain how viscosity is derived from shear wave velocity, in particular noting that the seismic tomographic model used is focussed on anisotropy.*
   We quote the answer from RC961:
   "The predictions of the 1D reference model are already shown and compared to older BIFROST results in Figs. 8 & 9 in Steffen et al. (2006). The relationship between shear wave velocity and viscosity variation has been derived in detail in Ivins & Sammis (1995, GJI 123:304-322), Steffen et al. (2006, EPSL 250:358-375) and in Wu et al. (2013, GJI 192:7-17) where both the effects of harmonicity and anelasticity are included. Please refer to those papers for detail. We will provide the references in the revised version.
   Regarding the seismic tomography model, it is taken from the www.seismology.harvard.edu web site. There, one can find 4 models used in the paper of Ekstrom & Dziewonski (1998). They are models with variations in isotropic S velocity, SH velocity, SV velocity, and model with SV/SH anisotropy model. Normally, we take the isotropic S velocity for the conversion to lateral

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viscosity variations. However, the SH variation at a specific location always give the largest variation, thus SH is used if we want to study the maximum effect of lateral viscosity variations. However, if we use the SH component instead of the isotropic component in Fennoscandia, the effect is largest in the lithosphere which is elastic and so does not matter. Even at the top layer in the upper mantle, the effect on the converted viscosity is not that large (average factor about 3)! Also, it is important to note that for the computation of the sensitivity kernel, the magnitude of the viscosity perturbation is divided out! So, even if SH is used, the effect will not significantly affect the conclusions of this paper."

The reviewer did not discuss this item and our response in RC965, but in his addition RC1040, he/she asks:

"*Is the rheology for the layer below 70 km realistic for Fennoscandia? Is the structure properly inferred from the literature cited (including Ivins and Sammis, 1995, see C961).*"

Therefore, the reviewer seems to accept the choice of 70 km for our test study, but he/she still states:

"*I maintain that a lowÂĄ-viscosity zone starting below 70 km, no alternative explored, is not a convincing proposition for the old craton.*"

We note that the reviewer failed to understand that we **are not after** a final rheological model for Fennoscandia, we only want to investigate the sensitivity. There is no rheology model that is perfect or without controversy - because there is no consensus on mantle rheological model. Our goal is to investigate if realistically distributed GNSS stations such as from the BIFROST network are capable to resolve such a lateral heterogeneity in the mantle. However, the reviewer still questions if the viscosity model is correctly resolved. Steffen et al. (2007) addressed the issue with simple regular blocks, this paper only wanted to test this with a more advanced model than Steffen et al. (2007) based on a model used in Steffen et al. (2006). At this point, the reviewer eventually questions many earlier studies by Patrick Wu, Georg Kaufmann and colleagues.

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In regard to this discussion we also see that the reviewer did not carefully read the papers mentioned in RC961, nor did he/she read the manuscript carefully. For example, he/she states in RC1040:

"*(In Steffen et al., 2006, the reader is referred to Figure 4 in Ivins and Sammis, 1995, which is not only not the right paper to throw into the discussion, as I try to argue above, nor should it be added to the references in the present ms; while "Figure 4" is not the correct reference in Steffen et al., 2006, probably a typo. So you see in what cobwebs you end up when work your way into the past...)*"

Steffen et al. (2006) are not referring to Figure 4 in Ivins & Sammis (1995), neither is it a typo. Figure 4 as written together with a reference to Ivins & Sammis (1995) in Steffen et al. (2006) refers to Figure 4 in Steffen et al. (2006). In the PDF one can simply click on the 4 and is led to the corresponding Figure (4). It was the layout of EPSL that time and frankly, we are wondering how one can read this as a reference to Figure 4 in Ivins & Sammis (1995) at all! So no cobwebs for us, but a warm welcome to the reviewer to the 21st century.

A similar strange quote came already up in his/her first review RC932:

"*Anisotropy is the essence of this work. To convert the shear wave velocities SH and SV into viscosities in the uppermost sub-lithosphere layer and into the mantle lithosphere details are missing in the ms, nor does the reference given in the context (Steffen and Wu, 2011) present them.*"

However, anisotropy is not discussed in Steffen & Wu (2011). So, where did the reviewer read it? We do cite this reference in our manuscript (3x), but not in the context mentioned by the reviewer. This comment by the reviewer is therefore simply false.

Another strange quote is:

"*I have to add that I am not overwhelmed by the results of* Wu et al. (2013)*; consistent values for the beta-parameter from GRACE and Bifrost were only found when much of the GPS data was excluded. That work seems premature;*"

Wu et al. (2013) state on page 8 & 15: "In this preliminary study,..." and again

on page 15: “However, one must caution that this preliminary study is limited in several ways.” This shows that the authors were(are) well aware of the fact that their study is only a first step with many assumptions. No need to call it premature. Of course, we can investigate anything until it is without further questions like the reviewer wishes before we publish it. But, which geoscientific investigation has ever been done to that stage?
To sum up our response to the first item: we only want to test if GNSS stations that have a real background (such as BIFROST in Fennoscandia) are capable in resolving a more realistic 3D viscosity structure. We provide such a structure, but not never claimed it is perfect or that it is the structure underneath Fennoscandia. Any other structure would not be for the benefit for the study as planned. Thus, we do not see any need to come up with something else.

2. *Test that linear superposition of partial derivatives w.r.t. viscosity in the box scheme works over the length scales of the problem. The sum of the effect of individual boxes varied one at a time needs to sum up to their collective effect.*
   The answer from RC961:
   “Unlike the formulation of Peltier (1998) and Mitrovica & Peltier (1991), the formulation of the sensitivity kernel in Wu (2006) does not involve any partial derivatives in the derivation – no relaxation times nor strength of modes are involved. Unlike the conventional spectral method where perturbations are required to be small for lateral variations, our FE method can handle large and rapid lateral changes in material properties – as long as the changes are adequately sampled (with more but smaller elements). Although not mentioned in the paper of Wu (2006), the sum of variations from each element has been found to be the same (within numerical accuracies) as the effect as a whole - provided that nonlinear rheology is not used. The reason is that as long as the problem is linear, the principle of superposition works. Such finding was considered too trivial to be mention in the paper of Wu (2006).”

This reply was criticized by the reviewer in both RC965 and RC1040. In RC965, the reviewer tries to disprove or question the principle of linearity and superposition with an example from the heat equation for 1-D Conductive Cooling. The reviewer started with the differential equation which he or she claims is linear in κ and then shows that the solution of this linear equation is not linear in κ because it involves the nonlinear erfc function. According to the reviewer, this example disproves the principle of linearity. However, we must point out that to obtain his or her solution with erfc function, one must assume that κ is a constant and not a variable. If one first treat κ first as a constant in a differential equation, and then turn around and treat κ as a variable, then one can indeed get strange and illogical answers and conclusions!
RC1040 again questions the linearity of the relationship between velocity and log-viscosity and the principle of superposition. But these relationships have been used by all the work in GIA modelling that has been published - including the spectral method, finite-element method, or spectral-finite-element method. Again, the bases of that question is RC965, which is clearly wrong! RC1040 is also not well written, and it is difficult to follow what he or she is complaining about.

3. *Lithosphere thickness (LT) is handled as a side effect of changing viscosity in a rather thick layer. While previous studies using spherically symmetric models agree on the existence of a trade-off between LT and viscosity in the upper mantle, the entanglement of these two rather different causes affecting rebound rates limits the relevance of this investigation as it does not explore a trade-off in the laterally heterogeneous model. As a minimum effort I suggest to start a second (and a third) model suite with thicker lithospheres, and to tune the no-visc-var version to yield similar rates. That's for the case if the radial node geometry in the modelling procedure is fixed.*
This was answered in RC961:

"That is a good point and it is something that we plan to publish and clarify in the future. However, new model suites take time to run and a complete story is better presented in a separate paper so that the focus of this paper won't get distracted." We think there is nothing to add.

4. *The threshold of 0.015 mm/yr for terming sensitivity significant is inapplicable for a long time to come. The question the ms tries to answer could be reversed: How much viscosity change is required to be detected by more than one GNSS station (so there is a change for obtaining an over-determined system in an inversion)? The current 1-sigma level from Bifrost may suffice, and accuracy gain in observation would increase the level of significance for the estimated parameters.*
This issue was answered in the first reply, but we mark in bold the important part: "Please note that **the threshold has nothing to do with the current accuracy of BIFROST/GPS**. The threshold is something arbitrarily chosen for visual display only **as the sensitivity kernel is normalized**, see p. 2397. The whole discussion by the reviewer is something we are well aware of and is also something we actually discussed in Wu et al. (2010, GJI 181: 653-664). The threshold is set so that it is higher than all sensitivity kernel values for the station of Brussels as it is by far the station with the lowest values, and also set so that stations near the ice margin show at least one sensitive block in a layer. If a higher threshold (e.g. comparable to the BIFROST accuracy as used in Wu et al. 2010) is applied, then less blocks can show their sensitivities clearly in Figures 4-12. However, we have clarified this point in the revised manuscript."
The reviewer has had the following remarks in RC1040 related to this part:
"*Quote: "Figure 6. Sensitivity of radial (a, c) and tangential velocity (b, d) in North America (a, b) and Fennoscandia (c, d) above the observation error for 5 yr of GPS observation to changes in used ice model (see text, red area, lines from top left to bottom right)"*
"5 years", written in year 16-17 of Bifrost."

The reviewer did not read Wu et al. (2010) carefully. First, it helps to turn pages: Figure 7 in Wu et al. (2010) shows the same but with 8 years of GPS observations. Second, in view of choosing 8 years, Wu et al. (2010) states: "An optimal location is defined by where sensitivity lies above the current accuracy of GPS measurements." Note that it is "current". At the time of the review and revision process of Wu et al. (2010) the last paper with "current" accuracies for Bifrost was Lidberg et al. (2007), which is clearly stated in the manuscript: "The following discussion on GPS sensitivity is based on the accuracies found by Calais et al. (2006) in North America and by Lidberg et al. (2007) in Fennoscandia." Third, in reference to Lidberg et al. (2007), we note that the first years of Bifrost observations are disturbed due to "intensive developments", and thus Lidberg et al. (2007) avoided the years up to 1996 in their analysis. We believe the reviewer is very well aware of this fact, but he/she is free to ask the third author of Lidberg et al. (2007) for more information (Judge yourself!). Therefore, in view of our arguments, we rebut the quote in RC1040 as **unfair**.
Last but not least, it is rather unclear to us from RC965 and RC1040 if the reviewer has understood that the chosen threshold has no relation to the accuracy of Bifrost. We have now clarified that in this revision.

5. *English.*
The reviewer criticized the quality of the English. We have completely revised the text and the English.
Regarding the following statement of this reviewer:
"*The language problems are at a level where I would welcome a statement of confirmation by Patrick Wu that he has seen the submission at all.*"
Patrick Wu has responded in particular to this issue:
"Both authors have tried to make the manuscript as clear and readable as possible before submission. However, there may be something in English that we missed, so we will try harder in the revised manuscript. Also, the reviewer should

note that we do not put our names lightly on papers – especially we never put our names on papers that we have not read or have no contributions."

Frankly, we were quite irritated by the second part of the statement from the reviewer above and we are still irritated that the reviewer did not pick up this issue neither in RC965 nor in RC1040. This statement adumbrates that the first author does not communicate with his co-authors/colleagues. Moreover, it implies that the first author commits scientific malpractice and undertakes deliberate falsehood by submitting manuscripts without knowledge of the co-authors indicated. This is an insult! As a concession, we hope that the reviewer has simply been not aware of this fact when he/she stated it. However, the first author expects an excuse from the reviewer.

In summary, we have shown that the reviewer did not understand the manuscript, that he/she made false statements and that he/she discretized the authors. We admit that this may have been a result of the quality of the English. We will thus do better in the revised manuscript. Regarding the critics on the model, we note that this is just a test as outlined above, not a full-blown investigation regarding the mantle structure. We never wrote that. We therefore see no reason to discuss our chosen approach as there is no benefit for the purpose of this study.

Interactive comment on Solid Earth Discuss., 5, 2389, 2013.