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

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Here are my reply to the 5 main points of Anonymous Referee #1:

#1 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 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.

#2 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).

#3 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.

#4 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 will clarify this point in the revised manuscript.

#5 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.

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