** List of points to address in revision.
The text following this list will
detail the problems. It has not been possible to arrange the detailed discussion
in a point-item list. I hope a context will emerge so that this perhaps unusual
means does provide help to the authors and the editor.

1. Produce a prediciton of deformation rates based on the no-variation model.
   Explain how viscosity is derived from sehar wave velocity, in particular noting
   that the seismic tomographic model used is focussed on anisotropy.

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.

3. Lithosphere thickness (LT) is handeled 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 heterogneous
   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 procdure is fixed.

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 increse the level of significance for the estimated parameters.

4. English.

** Detailed discussion
* Aim
Wu (2006, "W06") provides the foundation of this manuscript. While the primary aim of W06 has been to provide for planning of GNSS stations for optimum location in order to constrain lateral variations in visco-elastic structure using a Maxwell-rheology in a self-gravitating earth model, this ms accepts the reality of Bifrost, applying its analytic tools at the very site locations of the Fennoscandian GNSS network, computing sensitivity of 3-d rates of deformation due to block-wise structured viscosity variations in the earth model. The primary aim is thus seen in investigating the resolving power of this network to 3-d changes of viscosity structure ("v-str"), supporting research into viscosity inversion ("v-inv"), eventually combined with elastic parameters, keyword lithosphere thickness, maybe even lithosphere structure, my interpretation - and my hope) using GNSS-derived deformation rates. And prospectively in combination with additional techniques of observation.

* Findings
The primary findings ("C1") corroborate earlier work in concluding that it is mainly the v-str closest to a particular site in a depth range below 250 km that is most effective. The ms concludes also ("C2") an inversion for v-str is to employ the stations inside the area of the Pleistocene ice sheet ("PIS"), since ice thickness is seen to be coherent with the magnitude

of sensitivity. As a future step in suit the ms proposes a block division scheme tailored
on the GNSS-network.

According to the definition of sensitivity ("SE") in the ms, the measure is in units of m/yr
per log viscosity. Since 3-d rates are at maximum one order of magnitude smaller outside the
area of the PIS, there is little signal to be exploited for inversion given the observation
uncertainties. Seen per se, C2 is not much a surprise were it not for conclusions to the
contrary in a previous, similar study. This point needs more discussion and even
some exercise to provide arguments for the discussion.

To mention one of the inherent difficulties: Quote ("Q1") from W06:

QUOTE:
"(4) There is trade-off between radial and lateral viscosity variations
and that complicates the inversion of mantle viscosity. However,
with the inclusion of data at a broad range of locations, this
problem may be partially mitigated."
END QUOTE

The particular structuring into model blocks in the ms and the additional tailoring
to the Bifrost geometry that is suggested are still only first steps towards inversion.
Accepting the conclusion in this ms that sensitivity to one block of the (most interesting
v-layer #2 at 250+ km depth - agree?) is mainly limited to one
station; and the domains of sensible v-var's and sensitive stations are limited to an area
extending slighlty beyond the PIS or, respectively, decidedly inside the PIS,
is not really corroborating Q1. The ms does not put any effort in a mitigation of this
trade-offwhere it designs the block structure.

On a second point of W06 the ms hints to the contrary of Q2:

QUOTE:
"(5) Regions of anomalous viscosity lying under the ice sheet can
influence tangential velocities at far away sites."
END QUOTE

The ms comes to a different conclusion/recommendation: stations outside the PIS are
of limited value. More on this in section "linear superstition" [sic].

* The threshold of significant sensitivity and the capability of GNSS networks.
In reality, measurement of horizontal deformation meets a range of different obstacles.
In the horizontal, peripheral GNSS stations, let alone stations beyond
the periferal bulge, are affected by other processes than GIA:
Weak monumentation, thick sedimentary layers, intervening
orogenes and plate boundaries, and a small signal to dwell on in the first place.
Although these are arguments coming from an observer's practical point of view should
not speak against investigation of the GIA problem in isolation from plate tectonics
nor deny development in GNSS network technology, the ms appears to come to similar
conclusions as to which sub-networks to concentrate upon while not giving a single
thought to the problem of observability, resolution, signal-to-noise.

The ms can make a point in emphasizing that GIA model inversion calls for improvements
in stations in the perifery and in the far-field primarily concerning monumentation.
(In Fennoscandia the sitations were carefully monumented because the founding fathers
were much aware of the requirement of long-term stability in monument and observing

conditions).

In this context, the distinction into sufficient versus insignificant SE at the levels of
0.05 mm/yr (vertical) / 0.015 mm/yr (horizontal) ignores the present state-of-the art.
Present 1-sigma uncertainties are on the order of 0.3 (V) / 0.05 (h) mm/yr
(Lidberg et al., 2010) AT BEST --- except when assuming white-noise, something that has
been proven indadequate by a number of authors.
Supposing a benign Gauss-Markov noise process, the uncertainty decreases with the
square-root of duration. In realistic power-law noise the improvement would proceed
even slower (see various publications by S.D.P. Williams). Thus, in the most optimistic
view, a 0.015 mm/yr uncertainty needs at least 10 times longer duration of continuous
GNSS observations (where I am so keen as to compensate for a gain in technology with
the loss of repeatability due the power-law property). Not withstanding my cursory
approach to shwoing off with scepticism, the confidence level required in the ms
to point out significant boxes of SE seems still far out of reach. Inachnowledgment
the authors of future re-submissions will see time on their side. It's obvious what would
have happened with a higher threshold: Significant SE would occur less often but in the
nearest box, and who wants to see that? To me the threshold in the ms is a sweep
under the carpet of the insignificance of the results measured against GNSS
observation precision to be anticipated before the pension age of both readers and writers.
Add the uncertainty of the ice history in the range of wavelengths of 200 km or
less.

* Seismological earth model
About the adoption of reference model for the v-str., Ekström and Dziewonski (1998) ("ED98").
The motivation to use the results of ED98 needs to be substatiated.
Quoting ("Q3") from the Abstract:

QUOTE:
"The development and interpretation of tomographic models of
the Earth’s mantle have usually proceeded under the assumption
that fast and slow seismic velocity anomalies represent a spatially
heterogeneous temperature field associated with mantle convection.
Implicit in this approach is an assumption that either the
effect of anisotropy on seismic velocities is small in comparison
with isotropic thermal or compositional effects, or that the
tomographic results represent the average isotropic heterogeneity,
even if individual seismic observations are affected by anisotropic
structure. For example, velocity anomalies in the upper
portions of the oceanic mantle are commonly interpreted in terms
of the progressive cooling1,2 (and localized reheating3) of
a mechanical and thermal boundary layer consisting of rigid
oceanic lithosphere and an underlying, less viscous, asthenosphere.
Here, however, we present results from a global threedimensional
tomographic model of shear-wave velocity which
shows that the uppermost mantle beneath the central Pacific
Ocean is considerably more complicated than this simple model."
END QUOTE

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.
Please mention Wu, Wang and Steffen (2013, "WWS13") for motivating the particular
relation between shear-wave anomaly and viscosity.

ED98 present Pre-Cambrian cratons collectively (their Fig. 2 frame c).
It would suffice to point out that lateral SV-velocity variations in layers
#1 and #2 have been used as a proxy for viscosity variations, since the
field of motion in GIA is mostly spheroidal (toroidal being expected
only as a secondary effect of lateral heterogeneity). One would still
need to argue why viscosity is assumed to follow shear wave velocity
in the lower lithosphere, where composition would not have been mixed up as
much as in depth ranges involved in convection. While this could be
argued for, another structural property appears rather unconvincing:
The viscosity in layer UM1 varies from 10log(v) 24 to 21 from the east
to the west in the full depth range of 70-250 km. In effect this proposes
a lithosphere of 250 km thickness, and if lower viscosity is required
in an inversion, it would reduce the effective thickness of the lithosphere
at the same turn of the knob. For this reason, the alledged viscosity
variations are to be taken with a pair of quotation marks and a pinch of salt.

I have to add that I am not overwhelmed by the results of WWS13; 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; if such relations are
used in this ms, a prudent comment would be welcome.

* Trade-off, lithosphere thickness and upper mantle viscosity.
However, the fixed thickness of the lithosphere at 70 km and its
effective thickening by assigning higher viscosity is one of those entanglements
(trade-offs) at which W06 cautions us of. In the ms a sperate run of model
variants in which the lithosphere had been varied in thickness on top
of a radially symmetric mantle could have demonstrated whether (or where)
deformation rates develop unequivocal patterns.

The physics involved in a sharp viscosity contrast at a depth of 100 km or so,
GIA as well as mineralogy, is sufficiently different from a thick block with
an elevated, effective viscosity. And it will lead to different conclusions,
if, in the one case, one finds that LT cannot be distinguished from UMV, and in
the other, that one submits this limitation already a priori. The two
cases are within the modelling capability employed by the authors in this ms.

Remains to show that the LT-UMV-trade-off, that we know from joint adjustment
in radially symmetric models, persists when small blocks of
anomalous viscosity are varied. The smaller the horizontal cross-section of
the block the less will the lithosphere be engaged due its greater stiffness
as traction becomes more concentrated. It goes with wavenumber to the power of 4.
The horizontal rates are probably much less attenuated.

* A safe starting point
To begin with, the background reference model should be demonstrated to produce
deformation rates that provide a similarly good agreement with observations
as in e.g. Milne et al. (1004) or Lidberg et al. (2010), i.e. a normalized Chi^2
of fit near or below 10.

* Linear superposition or linear superstition?
There is another point that needs to address the method in its foundation.
In a radially symmetric case, the normal mode approach already produces
entanglement of viscosity in the indivudual layers to produce the relaxation times
of the modes, and for every additional layer, three additional modes are produced,

and all relaxation times and associated strength parameters are changed, some more, some less. Yet, a law of linear superposition of partical derivatives with respect to parameters in the elements of the system matrix appears to hold when perturbations are small. The result shown in Milne et al. (2004) on layer resolution in inversion to suggest that we would be overoptimistic to expect to resolve the viscosity of more than three layers. Note that viscosity variation in individual layers in such a model does not intervene with the orthogonality of the base functions.
In a laterally heterogeneous model, however, it needs to be shown that the sum of variations in boxes one-at-a-time is close to the impact of varying them in concert. The reason for expecting a difference there is because the structure obtains a long-wavelength perturbation in the second case and a series of short-wavelength perturbations shifted in space in the first case. It is not obvious that this difference would not matter. Co-variation of
this kind has not been addressed in W06 (using axi-symmetric loads and perturbations) while it is the more important to investigate in the present, full-3-d
block scheme and the asymmetric load.

** Summary.
At this point, the detailed investigation of the SE picked up by the GNSS network in Fennoscandia as we know it is of limited use. The conclusions in the ms reiterate some of the conclusions in W06 but misses opportunities pointed out in W06 (far-field effects; parameter entanglement). Resolving power is studied in a mono-causal setting, not quantitatively evaluating alternate causes of local deformation anomalies (differences from predictions with a radially symmetric model), particularly loading mass distribution (how much more local ice mass would produce a similar rate increment?) and lithosphere thickness. There is also too little awareness of the limitations of assumptions and methodolgy showing in the text. Too much positivism (in the sense of the philosophical basis of scientific conjecture) and too little sceptical rationalism.

The language, the phrasing in W06 was comprehensive and succinct, which can hardly be attributed to long stretches of the present ms.

The conclusion that can be drawn from the work are mainly that the location of V-anomalies with respect to the ice load, centre or edge, does matter; proveded the cave-at of linear superposition of box-variation holds. If it does, then a second conclusion can be draw, horizontal rates outside the region of uplift appears less important than in similar, earlier work.
I tried to point out some problems of this work which appear to me to be of more basic nature. To address them requires a rather different ms. These problems would favour much less of the site-related figures, giving more space to demonstrate the validity of the method and an acceptable fit with the unperturbed model, equivalent to M04.

So I tend to reject this ms, yet I would welcome a rebuttal before stating this as a final verdict. However it will turn out, both authors need to cooperate closely to address the problems and the textual presentation.
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.
Yes, I understand that we all need publications in order to stay in business. And I accept that contributions in the context of conferences or workshops rarely render fullblown, innovative results.

1. line 18-19:
"This area can be enlarged up to 800km when velocities of stations in the uplift center are investigated."
Can be? A larger area of sensitivity (800 km radius) is found when....

2. line 20-21
"We also note that in the first mantle layer (70–250 km depth) below the lithosphere, there is only small sensitivity to parts along the Norwegian coast."
... *low* sensitivity to *viscosity variation* *below* the Norwegian shelf / coastal region / northwestern stretch of the shield edge / subhorizontal to the .. '
(many alternatives, but please not "the coast"; that's a narrow, winding snake).

P. 2399 line 1 & 2
"...sensitivity...yields for.." and "sensitivity affects" and seems to have an "influence" ("Sensitivity" as an active entity?)
This § of Discussion draws conclusions ("It becomes evident"). O.k. to start with "We will show.." Sentence 3.. (line 3.. "However..." should appear at the end of the chapter.

line 9-10:
A blooper: "The central stations of Kiruna, Skellefteå, Vaasa and Mårtsbo ..."
The locations are more central with respect to the uplift area. They are not the railway stations of the towns listed (you can't even go to Mårtsbo by train).

"... are sensitive to the area along"
not to the area per se, but rather to the viscosity variation in this area.
"Sensitive to a box" in a context where box stands for the viscosity it encloses would be o.k. Suggest: Among the stations in or near the central area of uplift, Kiruna and ... are particularly sensitive to (the) strong viscosity gradients in the first (uppermost) layer where it stretches through the northwestern boundary of the shield. This structural zone coincides also with the edge of the Pleistocene ice sheet.

This list is only just begun.