This addition to the first review is divided into two parts. Short comments/reply to Patrick Wu's replies (5, 2389, 2013 / C961 and C970) in an attachment; and revisiting, reviewing, and summarizing the criticism and after the exchange of points of view following my recommendation (expressed in C932) that the manuscript cannot be published. Before determining this as the final verdict, I invited for a rebuttal, and the thematic editor, seconding to this effect, has arranged that the discussion page has been reopened to accept my – now last – words in the matter.

Apart from language issues, which per se would call for a major revision, the following list of items enumerates the major problems brought up in the first review report and in the deliberations.

1. Is the problem linear or (linear enough) in displacement rate versus log-viscosity when a box-shaped volume of laterally anomalous viscosity is introduced? (Anomalous with respect to the linear viscosity parameter, yet always staying within the linear Maxwell rheology).
2. Can the effect of a neighbourhood of such boxes with the same viscosity change (w.r.t. the laterally homogeneous structure) be summed up to equal the effect of a one-box representation of the anomaly?
3. 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).

As the ms is organized with its detailed study of single stations of the Bifrost network, the message conveyed is that the method laid out ion Wu (2006) is ripe for this specific application and that it can be ported from the geometry used by Wu (2006) to the (widely different) geometry in the present ms.

Dealing with rate observations, we know that an increase of viscosity leads to higher or lower rates depending on the point of time of the observation with respect to the Maxwell time of the matter. When regions that I normally would associate with the lower lithosphere are brought up as a suspect of viscous relaxation, I'm uncertain where on its Maxwell time scale we are located. I maintain that a low-viscosity zone starting below 70 km, no alternative explored, is not a convincing proposition for the old craton. Nothing else can I conclude from Ivins and Sammis (1995).

As a side-track since this reference has been brought up in the discussion:

While this study ends its temporal scope at less than one cycle per year, contemplates structures at 350 km depth and in the deep mantle, and absorption band versus composite Maxwell rheology, it's second thoughts about a uniform Maxwell rheology are confined to the concept's failure to simultaneously explain the time scale of GIA and the earth's anelastic response to tides. Quote from the Conclusions:

> If positive velocity anomalies beneath continental shields have a thermal origin, then the glacio-isostatic rebounds of Laurentia and Fennoscandia sample an anomalously 'stiff' upper mantle rheology with respect to global spherically symmetric average values. Long-wavelength viscosity anomalies in the shallow upper mantle explain the extremely rapid post-glacial emergence rates observed along Icelandic beaches. However, short-wavelength variability might also explain the observed rate difference.

Note: Iceland, not really notable for thick lithosphere, whereas … (quote continues right there)

Mantle models with laterally heterogeneous viscosity are a key factor in properly assessing rheology in the tidal band. A four-phase Maxwell viscoelastic composite rheological model provides a simpler physical description of shear modulus dispersion and attenuation at tidal frequency than do 'E-power law' models. We suggest that laterally heterogeneous Maxwell viscoelasticity may be a viable constitutive relation for periods as short as 12 hours and as long as a Milankovich cycle (~ 1OO OOO yr). Future geodetic solutions for solid-body tidal Love number dispersion (Sk,) might be used to constrain the deep mantle lateral variation in viscosity.

Notice: "We suggest…", and notice that the focus of the paper remains in the mantle and in the tidal domain. It would be against the fundamental notion of this paper to extrapolate the relaxation mechanism under investigation to the GIA domain as the authors had pointed out: There is certainly not a single relaxation mechanism; maybe there's a generalized Maxwell body with only a few constitutive parameters, maybe it's as complicated as rock composition; no, the problem is to fill the gap between tides+Chandler and GIA (and as the hope: beyond). Subsequently, work has been presented, e.g. Kaufmann et al. (2005), that employ the temperature-based viscosity scaling law using seismic v_s as a proxy for temperature according to Karato (e.g. 1993). (In Steffen at al., 2006, the reader is referred to Figure 4 in Ivins and Sammis, 1994, 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…)

Although I appreciate the logic in the Karato-Kaufmann-Steffen-Wu design of the viscosity law, the earlier papers make clear that the assumption always is that chemistry does not change laterally. Fair enough in a convecting mantle. At 70 to, say, 200 km depth below Fennoscandia, this would be unconvincing (Kaikkonen et al.,2000, arrive at 500 ^{o}C Moho-temperatures at typically 50 km depth). Considering the box volumes in that depth range, narrow as they are, to have anomalous viscosity on the premise that this would have to be attributed to lateral temperature variations on the same length scale. P-wave studies rather point towards a structural/mineralogical heterogeneity below the Baltic Shield (Sandoval et al.,2004). Looking at sheerwaves, Bruneton et al. (2004) points out higher than normal shear wave velocities, quote:

Our final shear wave velocity model shows lateral variations at each depth of ±3% around the average value. Heat flow [Kukkonen, 1993] and receiver function analysis [Alinaghi et al., 2003] require a very homogeneous thermal pattern for the upper mantle in the region. The lateral variations of seismic velocities are therefore most probably due to chemical variations.

[69] The obtained velocities are on average 4% higher than standard Earth models for the upper mantle down to 200 km. There is no evidence for a substantial low-velocity zone which would make it possible to define the lithosphere-asthenosphere boundary. Another criterion for defining this boundary would be the depth below which the amplitude of the lateral heterogeneities strongly diminishes, but our lateral resolution is poor below 150 km depth.

Seems needless to consider low viscosity there, seems inappropriate to neglect chemistry variation there.

Maths.

After having dismissed the concept of the narrow, Bifrost-adapted boxes in such shallow depth as a well-motivated and well-underpinned starting point--- just adding in passing that the anisotropy found in Ekström and Dziewonski might provide the clue to solving the riddle why GIA models routinely come up with thin lithospheres, without the detour via viscosity---I shall consider the mathematical viability of the treatment of linear superposition. I've argued with the diffusion equation in a homogeneous material and pointed out the nonlinear dependence of the solution on the diffusion coefficient, appearing in the argument of the erfc-function as its analytical solution. In Patrick Wu's reply, anything as nonlinear as the erfc-function wouldn't appear in the GIA problem, else he had missed something. Granted, even the erfc-function has locations, ranges, where the derivative wouldn't change significantly given not too loose margins. If that's the case on the branch of the relaxation curve that the observer is watching, so well. However, the ms should either demonstrate that or prove that the case has been demonstrated before. Else it is doubtful if the anomalous viscosity places us before, on, or after the Maxwell time of this volume. It turns out at least that linear superposition is not automatically warranted by a linear differential equation with an –--in this case---stress-independent rheology. Take the plate bending problem, four differential orders in space and, in pure elastic material, none in time, and add a feedback force due to buoyancy. The homogeneous equation features, ahum, hyperbolic and trigonometric cosines of location with the flexural rigidity (power of -¼) multiplying in the arguments. As for viscosity we hope for a linear relation between the log of viscosity (which enters flexural rigidity linearly when we are on the late side of Maxwell time) and the surface displacement (rate), especially when an anomalous viscosity resides in a limited region. **I more than happily admit that I have found almost exactly this** (using Mathematica, not unexpectedly reverting to numerical integration methods in laterally heterogeneous cases) **when I reduced the flexural rigidity at an off-centre location under a wide load that generously umbrellad the place of the anomaly.**

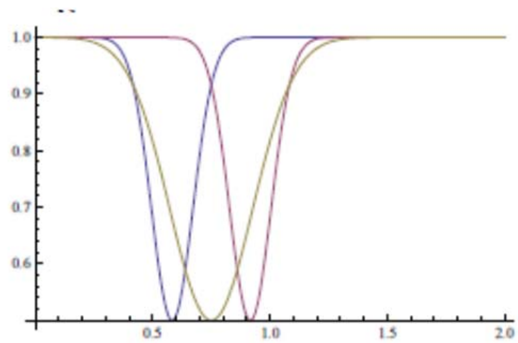
There was also a surprise, a feature observed in the ms but not really understood/commented/explained/explored further. When the anomaly is safely inside the loaded area, it does not matter how wide it is. The excess displacement (w.r.t the homogeneous case) stays almost the same despite the width of the anomaly, stepping it through more than one order of magnitude, and displacement peaks right on top of the centre of the anomaly. The findings in the ms are a.o. that an anomaly introduced below one GPS station affects this one station but hardly the neighbour. A thicker lithosphere placed on top could have spread out the displacement feature a bit…. There is also a critical interplay with the load at its edge within a surprisingly narrow range. Only then the size of the rigidity anomaly comes into play.

As contradictory as this might appear at first sight, but if the load spectrum is weak at the wavelengths of the anomaly, there is little to excite. A narrow zone of weakness is then as efficient as a wide one. So there's a rule: To obtain response at the wavelength of a structural anomaly, you need couplings with the load's spectrum, i.e. there must be a spectral overlap, places where derivatives peak must be near each other. Therefore the enhanced sensitivity at the load's edge.

Thus, a better understanding of the phenomenon based on a simple-case study would have been helpful here, arguing again in favour of a thicker lithosphere. The method with the fine grid creates an untenable situation of ambiguity. So if the paper could conclude something, after necessary recapitulation of the basic properties of the solution and the strategy, it's the dismissal of high-resolution grids as a feasible tool.

Finally, what about compounding two neighbour anomalies in one of twice the size? On this item I could not find a linear relation between size and rebound. The wavelength of the forcing enters displacement with the fourth power, and 2 times X^4 is not $(2X)^4$, even when X is small and the term is added to something already big (said with a pinch of irony, but I can demonstrate it, see the Mathematica notebook). Stresses below the lithosphere relaxing at a specific wavelength or length scale will, among other, exert tractions on the lithosphere to which it responds rather discriminatingly on behalf of the scale-to-the-power-of four rule. Again, the 70 km thick lithosphere is susceptible to 100-500 km wavelengths. So it's not only inside the relaxing medium that the spectrum gets filtered.

The test with one-wide-anomaly replaced by the sum of two half-sized anomalies of the same reduction of flexural rigidity (translate: viscosity (principle of equivalence, Laplace transform of Maxwell body….) demonstrates quite convincingly for the one who's mind is trained on the power-of-four law:



The gray shape produces approximately the same displacement as the sum of the blue and purple shapes computed separately. The gray shape is twice as wide. 50% reduction of rigidity. Load centre is at 0, load radius is 2, buoyancy 1/3.

In summary:

You cannot assume that a constitutional parameter acts always linearly on the solution of a higher-order ordinary or partial differential equation, linear as it might transfer input (traction amplitude) into output (displacement). Even a pendulum swings with the square root of l/g. I can only hope the structural engineers that design your double size new office floor know how to dimension the traverses. The claim that linear superposition holds in the aspect of wavelength / length scales is outrageous, and worse, calling the matter trivial is a clear indication that something rather essential has been missed.

Wavelength (scale-size) of perturbations, heterogeneities, and forcing play an intriguing, complicated game. There is no way around partial derivatives and small steps of iteration.

The question where the current relaxation of the heterogeneities investigated is located on the time scale with respect to the Maxwell time of the material is not contemplated. A decrease in viscosity might very well be neutral to or accelerating displacement **rate (!)** when the reference viscosity is high.

The manuscript is poorly underpinned as the method is concerned, and assumptions too.

Future work should take the demand for falsifiability of claims and hypotheses more seriously, as a trait of character of sound science.

If the ms would be published with all the points above perceived as shortcomings somehow amended/circumnavigated, yet stay with the box approach, the 70 km low viscosity zones, the pursuit of the linear superposition dismissing the effect of different spatial scales, the dependence of the vs-to-viscosity conversion on laterally homogenous chemistry, the conclusions would be somewhat discouraging for the geodesists. Better turn the research question around: What anomalies in the Bifrost results are you able to perceive and explain? Perhaps not perfectly in line with other evidence, but hopefully not in a head-on collision; or better still: head-om but with compelling evidence.

P. Kaikkonen, K. Moisio, M. Heeremans, 2000. Thermomechanical lithospheric structure of the central Fennoscandian Shield, Phys. Earth Planet. Inter., 119, 209–235

Sandoval, S., E. Kissling, J. Ansorge, and the SVEKALAPKO Seismic Tomography Working Group (2003), High-resolution body wave tomography beneath the SVEKALAPKO array: I. A priori 3D crustal model and associated traveltime effects on teleseismic wavefronts, Geophys. J. Int., 153, 75–87.

Bruneton et al. 2004. Complex lithospheric structure under the central Baltic Shield from surface wave tomography, J. Geophys. Res., 109, B10303, doi:10.1029/2003JB002947.

Enclosures:

1. Reply by Patrick Wu, C691, with my comments in the pdf.
2. Mathematica notebook.

Solid Earth Discuss., 5, C961–C963, 2014
www.solid-earth-discuss.net/5/C961/2014/

Solid Earth Discussions

SED
5, C961–C963, 2014

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

P. Wu

ppwu@ucalgary.ca

Received and published: 5 February 2014

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.

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

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.

SED
5, C961–C963, 2014

Interactive Comment

#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.

SED

5, C961–C963, 2014

Interactive Comment

In[340]:=
```
SetOptions[$FrontEndSession, FontProperties → {"ScreenResolution" → Automatic}]
```

In[341]:=
```
Needs["DifferentialEquations`NDSolveProblems`"];
Needs["DifferentialEquations`NDSolveUtilities`"];
```

$$\texttt{flr[ls\_, vp\_, mu\_] := } 2\,\frac{\text{ls vp mu}}{\text{vp ls} + \text{mu}}$$

The differential equation is

$$\text{DSolve}\left[\partial_y\,\partial_y\left(\phi\left(1+\varphi\,e^{-\left(\frac{y-\gamma}{\lambda}\right)^2}\right)\partial_y\,\partial_y w[y]\right)+\beta\,w[y]==f[y],\ w[y],\ y\right]$$

$$\texttt{Collect}\left[\partial_y\ \partial_y\left(\phi\left(1+\varphi\, e^{-\left(\frac{y-\gamma}{\lambda}\right)^2}\right)\partial_y\ \partial_y w[y]\right),\ w''[y]\right]$$

$$\left(\frac{4\, e^{-\frac{(y-\gamma)^2}{\lambda^2}}\,(y-\gamma)^2\,\phi\,\varphi}{\lambda^4}-\frac{2\, e^{-\frac{(y-\gamma)^2}{\lambda^2}}\,\phi\,\varphi}{\lambda^2}\right) w''[y]-\frac{4\, e^{-\frac{(y-\gamma)^2}{\lambda^2}}\,(y-\gamma)\,\phi\,\varphi\, w^{(3)}[y]}{\lambda^2}+\phi\left(1+e^{-\frac{(y-\gamma)^2}{\lambda^2}}\varphi\right) w^{(4)}[y]$$

With a little re-arranging:

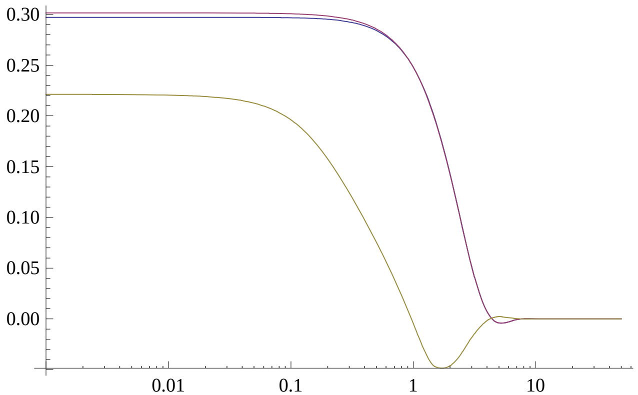
$$\begin{aligned}
&\texttt{sol}[\phi_,\ \beta_,\ \sigma_,\ \varphi_,\ \lambda_,\ \gamma_] := \texttt{NDSolve}\Big[\Big\{\\
&\quad w'[\texttt{Rationalize}[y]] == v[\texttt{Rationalize}[y]],\\
&\quad v'[\texttt{Rationalize}[y]] == u[\texttt{Rationalize}[y]],\\
&\quad u'[\texttt{Rationalize}[y]] == t[\texttt{Rationalize}[y]],\\
&\quad t'[\texttt{Rationalize}[y]] == \frac{1}{1+e^{-\frac{(\texttt{Rationalize}[y]-\gamma)^2}{\lambda^2}}\varphi}\left(\frac{4\, e^{-\frac{(\texttt{Rationalize}[y]-\gamma)^2}{\lambda^2}}\,(y-\gamma)\,\varphi}{\lambda^2}\, t[\texttt{Rationalize}[y]]\right.\\
&\qquad -\left(\frac{2\,(y-\gamma)^2}{\lambda^2}-1\right) e^{-\frac{(y-\gamma)^2}{\lambda^2}}\,\frac{2}{\lambda^2}\,\varphi\, u[\texttt{Rationalize}[y]]\\
&\qquad -\frac{\beta}{\phi}\, w[\texttt{Rationalize}[y]]\\
&\qquad \left.+\frac{1}{\phi}\, e^{-\left(\frac{\texttt{Rationalize}[y]}{\sigma}\right)^2}\right),\\
&\quad v[-50] == 0,\ v[50] == 0,\ w[-50] == 0,\ w[50] == 0\Big\},\\
&\ \ \{t,\ u,\ v,\ w\},\ \{y,\ -50,\ 50\},\\
&\ \ \texttt{Method} \to \texttt{"StiffnessSwitching"},\ \texttt{WorkingPrecision} \to 50\Big]
\end{aligned}$$

```
refsol = sol[1, 3, 2, 0, 1, 0]
```

```
{{t → InterpolatingFunction[{{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}},
   <>], u → InterpolatingFunction[
   {{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}}, <>],
  v → InterpolatingFunction[{{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}},
   <>], w → InterpolatingFunction[
   {{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}}, <>]}}
```

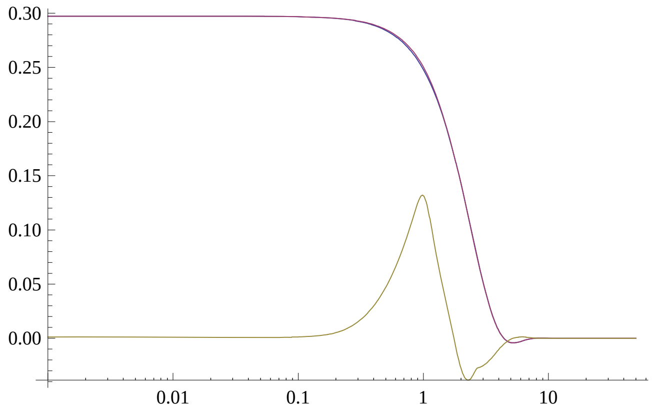
```
cbs = sol[1, 3, 2, -1 / 2, 1 / 8, 0];
```

```
LogLinearPlot[{w[-x] /. refsol, w[-x] /. cbs, 50 ((w[-x] /. cbs) - (w[-x] /. refsol))}, {x, 0.001, 50}, PlotRange → All]
```




```
sbs = sol[1, 3, 2, -1 / 2, 1 / 8, -1];
```

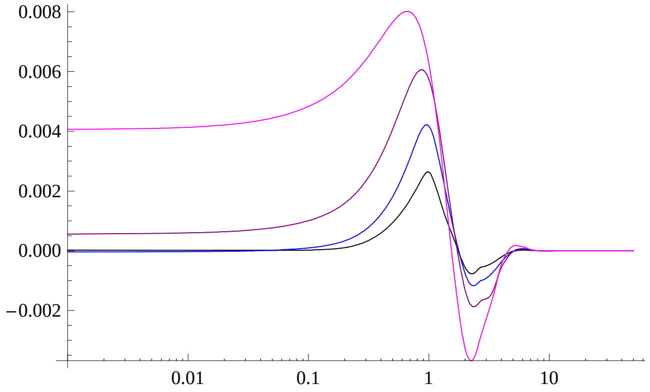
```
LogLinearPlot[{w[-x] /. refsol, w[-x] /. sbs, 50 ((w[-x] /. sbs) - (w[-x] /. refsol))}, {x, 0.001, 50}, PlotRange → All]
```



Now a suite of perturbations for a range of widths. Is there an obvious scaling relation between displacement and width?

```
wsbs0 = sol[1, 3, 2, -1 / 2, 1 / 8, -1];
wsbs1 = sol[1, 3, 2, -1 / 2, 1 / 4, -1];
wsbs2 = sol[1, 3, 2, -1 / 2, 1 / 2, -1]; wsbs3 = sol[1, 3, 2, -1 / 2, 1, -1];
```

```
LogLinearPlot[{
  (w[-x] /. wsbs0) - (w[-x] /. refsol),
  (w[-x] /. wsbs1) - (w[-x] /. refsol),
  (w[-x] /. wsbs2) - (w[-x] /. refsol),
  (w[-x] /. wsbs3) - (w[-x] /. refsol)}, {x, 0.001, 50}, PlotRange → All, PlotStyle → {Black, Blue, Purple, Magenta, Cyan}]
```

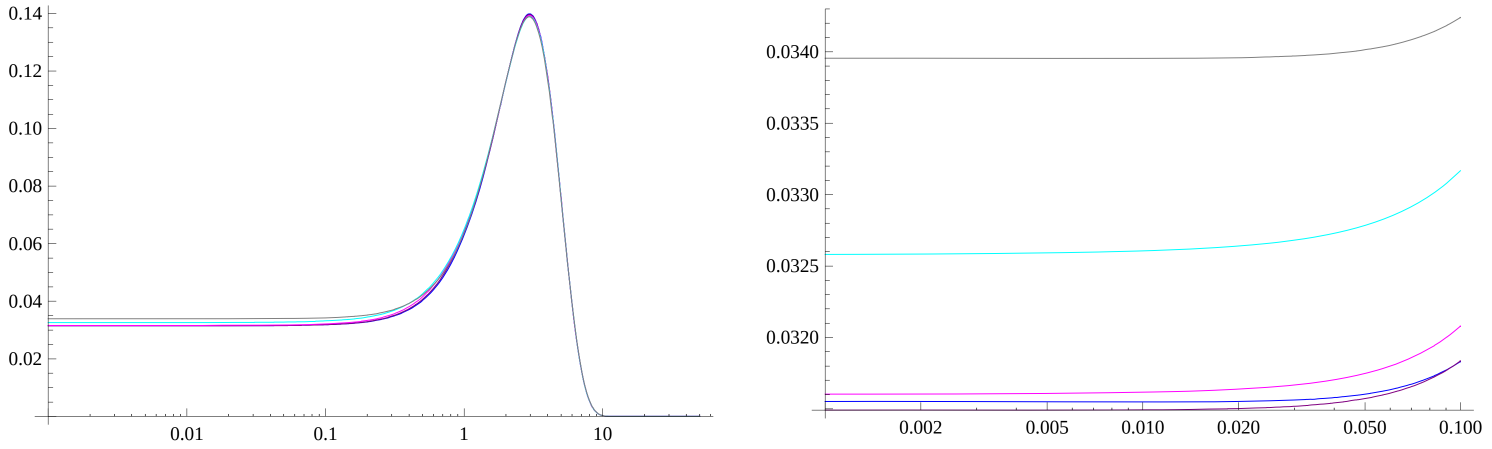


Displacement at the peak (which is not stationary in the maximum of theperturbation) and at the load centre show rather different kinds of scaling.

And twice the load width:

```
wsbs20 = sol[1, 3, 4, -1 / 2, 1 / 8, -1];
wsbs21 = sol[1, 3, 4, -1 / 2, 1 / 4, -1];
wsbs22 = sol[1, 3, 4, -1 / 2, 1 / 2, -1]; wsbs23 = sol[1, 3, 4, -1 / 2, 1, -1];
wsbs24 = sol[1, 3, 4, -1 / 2, 4, -1];
```

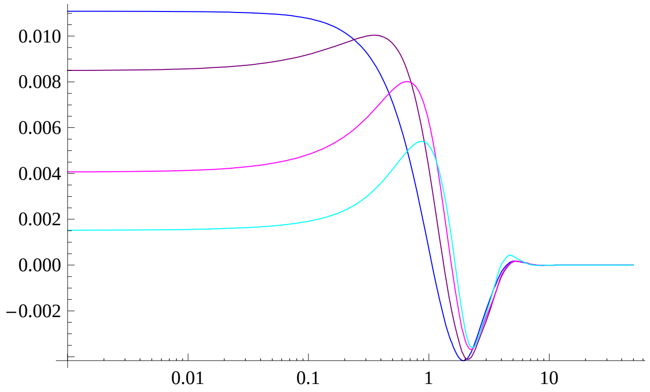
```
pwpall = LogLinearPlot[{
    (w[-x] /. wsbs20) - (w[-x] /. refsol),
    (w[-x] /. wsbs21) - (w[-x] /. refsol),
    (w[-x] /. wsbs22) - (w[-x] /. refsol),
    (w[-x] /. wsbs23) - (w[-x] /. refsol),
    (w[-x] /. wsbs24) - (w[-x] /. refsol)}, {x, 0.001, 50}, PlotRange → All, PlotStyle → {Blue, Purple, Magenta, Cyan, Gray}];
pwppart = LogLinearPlot[{
    (w[-x] /. wsbs20) - (w[-x] /. refsol),
    (w[-x] /. wsbs21) - (w[-x] /. refsol),
    (w[-x] /. wsbs22) - (w[-x] /. refsol),
    (w[-x] /. wsbs23) - (w[-x] /. refsol),
    (w[-x] /. wsbs24) - (w[-x] /. refsol)}, {x, 0.001, 0.1}, PlotRange → All, PlotStyle → {Blue, Purple, Magenta, Cyan, Gray}];
GraphicsRow[{pwpall, pwppart}]
```



When the load perimeter is much wider than the position of the perturbation, there is little effect of the width of the perturbation.

Now a suite of variations on the theme, load width = 2, shifting the position

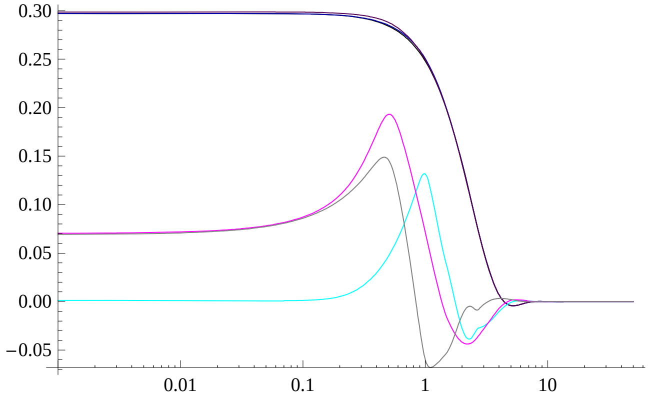
```
wsbs30 = sol[1, 3, 2, -1 / 2, 1, 0];
wsbs31 = sol[1, 3, 2, -1 / 2, 1, -1 / 2];
wsbs32 = sol[1, 3, 2, -1 / 2, 1, -1];
wsbs33 = sol[1, 3, 2, -1 / 2, 1, -3 / 2]; wsbs34 = sol[1, 3, 2, -1 / 2, 1, -2];
LogLinearPlot[{
  (w[-x] /. wsbs30) - (w[-x] /. refsol),
  (w[-x] /. wsbs31) - (w[-x] /. refsol),
  (w[-x] /. wsbs32) - (w[-x] /. refsol),
  (w[-x] /. wsbs33) - (w[-x] /. refsol)
 }, {x, 0.001, 50}, PlotRange → All, PlotStyle → {Blue, Purple, Magenta, Cyan}]
```



Next the response of two 1/8-wide perturbations at different positions, 1 and 1/2 (remember: the load width is always 2)

```
hsbs = sol[1, 3, 2, 0, 1 / 8, -1 / 2];
```

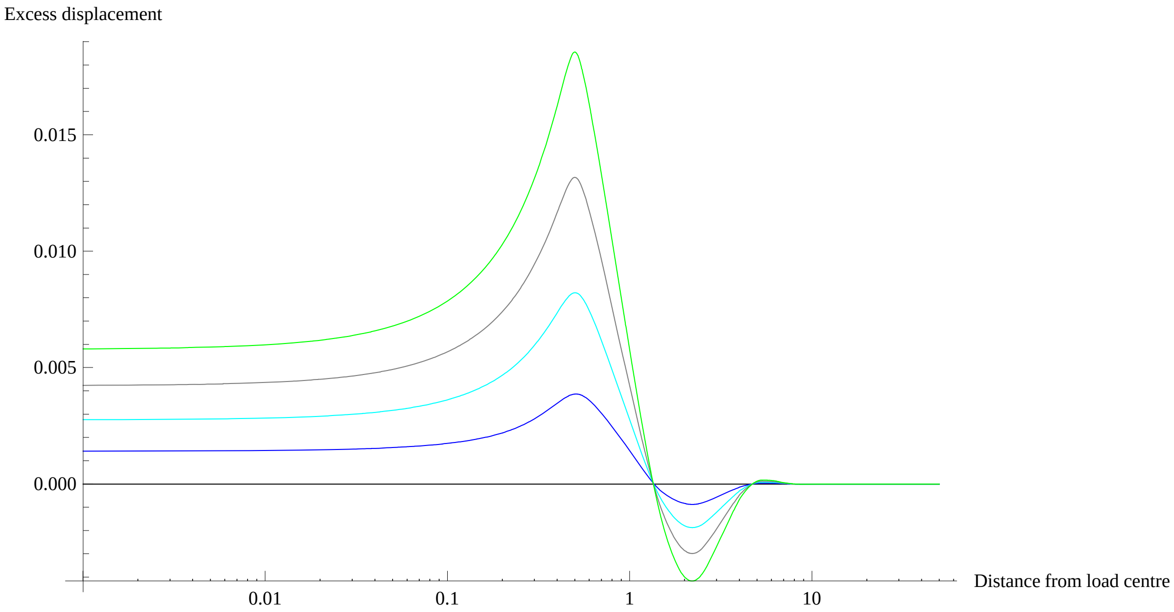
```
LogLinearPlot[{
  w[-x] /. refsol,
  w[-x] /. sbs,
  w[-x] /. hsbs,
  50 ((w[-x] /. sbs) - (w[-x] /. refsol)),
  50 ((w[-x] /. hsbs) - (w[-x] /. refsol)),
  50 ((w[-x] /. hsbs) - (w[-x] /. sbs))}, {x, 0.001, 50}, PlotRange → All, PlotStyle → {Black, Darker[Blue], Darker[Purple], Cyan, Magenta, Gray}]
```



Now a series of local relaxations of ϕ using φ = -1/2, -3/4..., i.e. $\varphi_n = -1 + (1/2)^n$, so log(ϕ) decreases linearly. Will $y - y_{\text{ref}}$ grow linearly too?

```
hsbs1 = sol[1, 3, 2, -1 / 2, 1 / 8, -1 / 2];
hsbs2 = sol[1, 3, 2, -3 / 4, 1 / 8, -1 / 2];
hsbs3 = sol[1, 3, 2, -7 / 8, 1 / 8, -1 / 2];  hsbs4 = sol[1, 3, 2, -15 / 16, 1 / 8, -1 / 2];
```

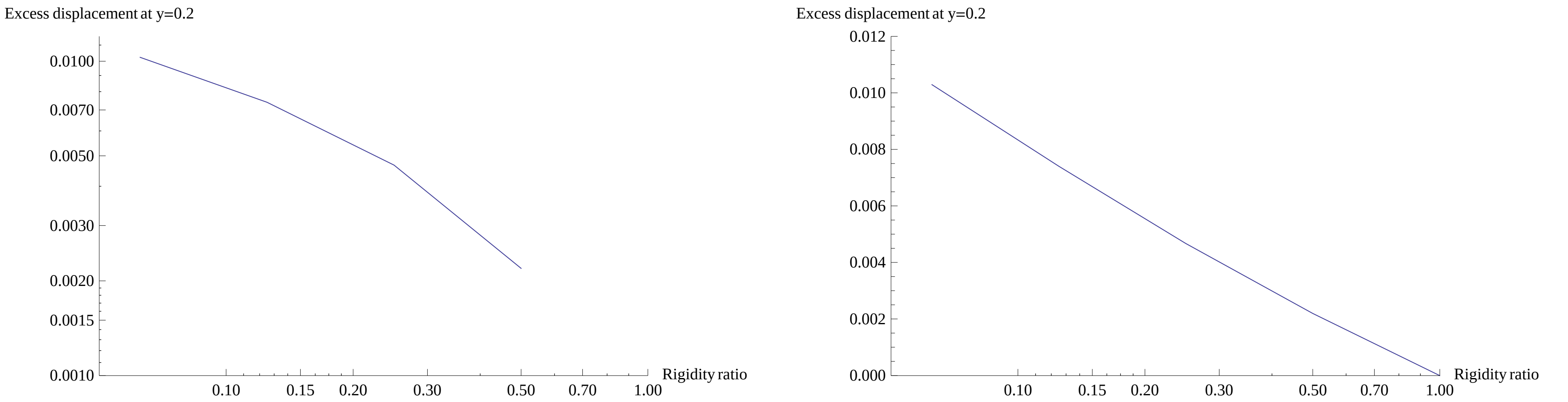
```
LogLinearPlot[{
  (w[-x] /. hsbs) - (w[-x] /. refsol),
  (w[-x] /. hsbs1) - (w[-x] /. refsol),
  (w[-x] /. hsbs2) - (w[-x] /. refsol),
  (w[-x] /. hsbs3) - (w[-x] /. refsol),
  (w[-x] /. hsbs4) - (w[-x] /. refsol)},
 {x, 0.001, 50}, PlotRange → All, PlotStyle → {Black, Blue, Cyan, Gray, Green},
 AxesLabel → {"Distance from load centre", "Excess displacement"}]
```



```
tbl = {Join[{1},  (w[-1/5] /. hsbs)  - (w[-1/5] /. refsol)],
  Join[{1/2},  (w[-1/5] /. hsbs1)  - (w[-1/5] /. refsol)],
  Join[{1/4},  (w[-1/5] /. hsbs2) - (w[-1/5] /. refsol)],
  Join[{1/8},  (w[-1/5] /. hsbs3) - (w[-1/5] /. refsol)],
  Join[{1/16}, (w[-1/5] /. hsbs4) - (w[-1/5] /. refsol)]}
```

$$\left\{\left\{1,\ 0.\times 10^{-50}\right\},\ \left\{\frac{1}{2},\ 0.0021940566885919961412994919690904718610225678644\right\},\ \left\{\frac{1}{4},\ 0.0046706028923487735905497429465326448768591814540\right\},\ \left\{\frac{1}{8},\ 0.0073998353896340961883630694777682079450850874222\right\},\ \left\{\frac{1}{16},\ 0.0102914129246740988177566517479158995321498300940\right\}\right\}$$

```
plolo =
  ListLogLogPlot[tbl, PlotRange → {{0.05, 1}, {0.001, 0.012}}, Joined → True, AxesLabel → {"Rigidity ratio", "Excess displacement at y=0.2"}];
plilo = ListLogLinearPlot[tbl, PlotRange → {{0.05, 1}, {0.0, 0.012}}, Joined → True,
   AxesLabel → {"Rigidity ratio", "Excess displacement at y=0.2"}];
GraphicsRow[
 {plolo,
  plilo}]
```



So far, the weakening by power of 1/2 results in a fairly linear relation of the increase of deformation with respect to the homogeneous plate. Now the same exercise with viscosity.

```
{flr[1, 1, 1], flr[1, 1 / 10, 1]}
```

$$\left\{1, \frac{2}{11}\right\}$$

```
vrefsol = sol[flr[1, 1, 1], 3, 2, 0, 1, 0]
```

```
{{t → InterpolatingFunction[{{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}},
    <>], u → InterpolatingFunction[
    {{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}}, <>],
  v → InterpolatingFunction[{{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}},
    <>], w → InterpolatingFunction[
    {{-50.000000000000000000000000000000000000000000000000, 50.000000000000000000000000000000000000000000000000}}, <>]}}
```

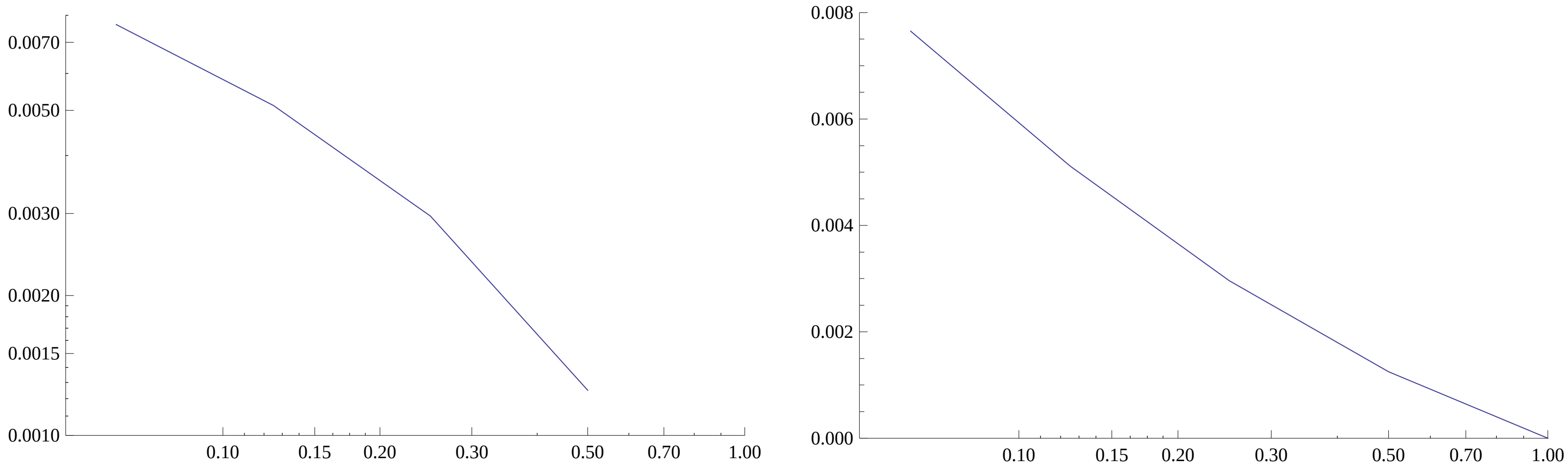
```
vhsbs = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1, 1], 1/8, -1/2]; vhsbs1 = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1/2, 1], 1/8, -1/2];
vhsbs2 = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1/4, 1], 1/8, -1/2]; vhsbs3 = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1/8, 1], 1/8, -1/2];
vhsbs4 = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1/16, 1], 1/8, -1/2];
```

```
vhsbs3 = sol[flr[1, 1, 1], 3, 2, -1 + flr[1, 1/8, 1], 1/8, -1/2];
```

```
vtbl = {Join[{1}, (w[-1/5] /. vhsbs) - (w[-1/5] /. refsol)],
  Join[{1/2}, (w[-1/5] /. vhsbs1) - (w[-1/5] /. refsol)],
  Join[{1/4}, (w[-1/5] /. vhsbs2) - (w[-1/5] /. refsol)],
  Join[{1/8}, (w[-1/5] /. vhsbs3) - (w[-1/5] /. refsol)],
  Join[{1/16}, (w[-1/5] /. vhsbs4) - (w[-1/5] /. refsol)]}
```

$$\left\{\left\{1, 0.\times10^{-50}\right\}, \left\{\frac{1}{2}, 0.0012507340296107797927662044356278321779444407682\right\}, \left\{\frac{1}{4}, 0.0029604741663963313133263688977067675226925608316\right\}, \left\{\frac{1}{8}, 0.0051183500309564112701220872374534032296278218641\right\}, \left\{\frac{1}{16}, 0.0076479569846584631710588584483077567717878749522\right\}\right\}$$

```
plolov = ListLogLogPlot[vtbl, PlotRange → {{0.05, 1}, {0.001, 0.008}}, Joined → True];
plilov = ListLogLinearPlot[vtbl, PlotRange → {{0.05, 1}, {0.0, 0.008}}, Joined → True];
GraphicsRow[{plolov, plilov}]
```



So, since the relaxation is not a linear function of log viscosity, there is a slight deviation from linearity of displacement versus log viscosity.

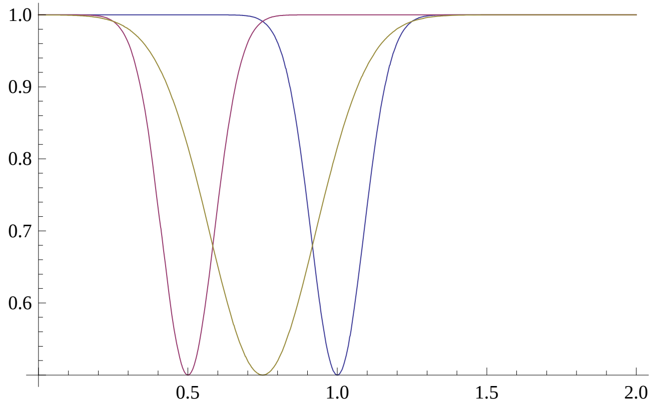
PART 2 - Superposition of two perturbations and the question whether one, a wide perturbation can replace it.

This is not straight forward with the Gaussian bells. By what criterion is one wide bell equivalent to two narrower ones, shapes that may overlap with a certain amount? An amount to be determined.

We have soultions sbs = sol[1, 3, 2, -1/2, 1/8, -1]   and   hsbs1 = sol[1, 3, 2, -1/2, 1/8, -1/2]

Is there a bell width that gives the same excess displacement at location γ = -3/4? We can produce a succession of 1/8-wide perturbations at different distances from 3/4.

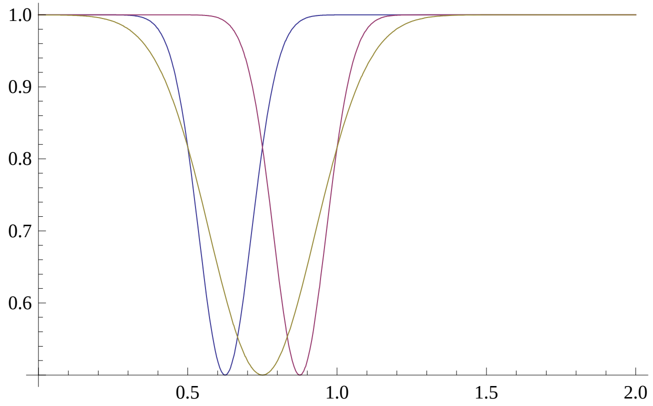
```
Plot[{1 - 1/2 e^(-((y-1)/(1/8))^2), 1 - 1/2 e^(-((y-1/2)/(1/8))^2), 1 - 1/2 e^(-((y-3/4)/(1/4))^2)}, {y, 0, 2}, PlotRange → All]
```



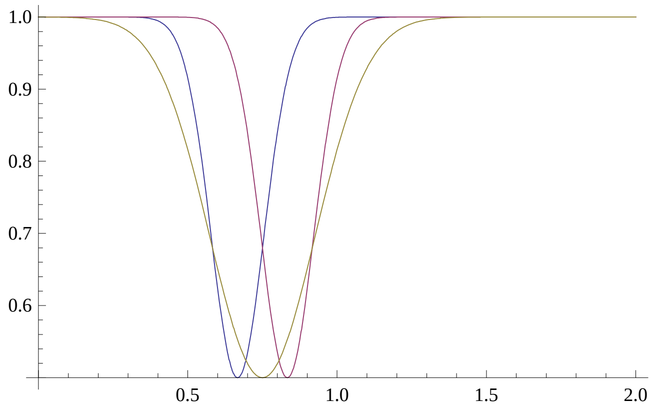

```
wsbs   = sol[1, 3, 2, -1/2, 1/4, -3/4];
nsbs01 = sol[1, 3, 2, -1/2, 1/8, -1/2];        nsbs02 = sol[1, 3, 2, -1/2, 1/8, -1];
nsbs11 = sol[1, 3, 2, -1/2, 1/8, -3/4 - 1/8];  nsbs12 = sol[1, 3, 2, -1/2, 1/8, -3/4 + 1/8];
nsbs21 = sol[1, 3, 2, -1/2, 1/8, -3/4 - 1/12]; nsbs22 = sol[1, 3, 2, -1/2, 1/8, -3/4 + 1/12];
```

```
nsbs101 = sol[1, 3, 2, -1/2, 1/8, -3/4 - 1/6];  nsbs102 = sol[1, 3, 2, -1/2, 1/8, -3/4 + 1/6];
```

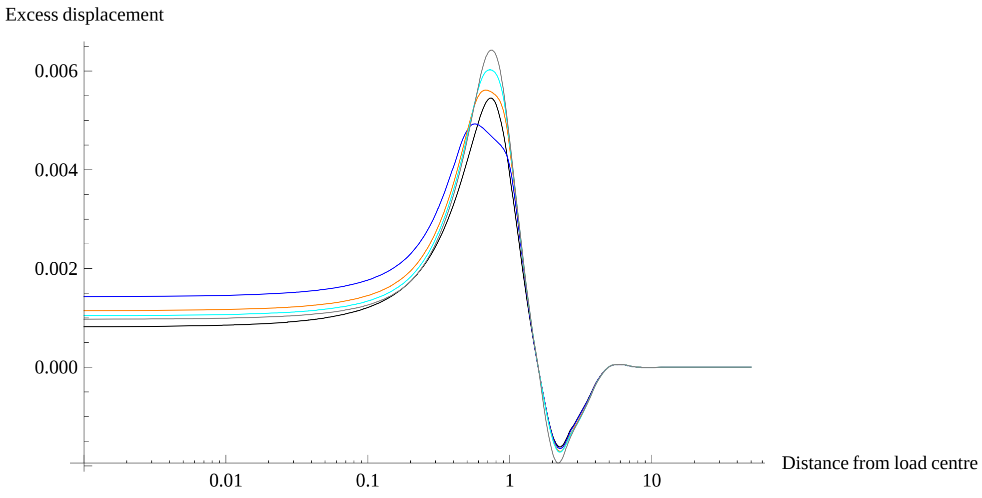
$$\text{Plot}\left[\left\{1-1/2\,e^{-\left(\frac{y-3/4+1/8}{1/8}\right)^2},\ 1-1/2\,e^{-\left(\frac{y-3/4-1/8}{1/8}\right)^2},\ 1-1/2\,e^{-\left(\frac{y-3/4}{1/4}\right)^2}\right\},\ \{y,0,2\},\ \text{PlotRange}\to\text{All}\right]$$

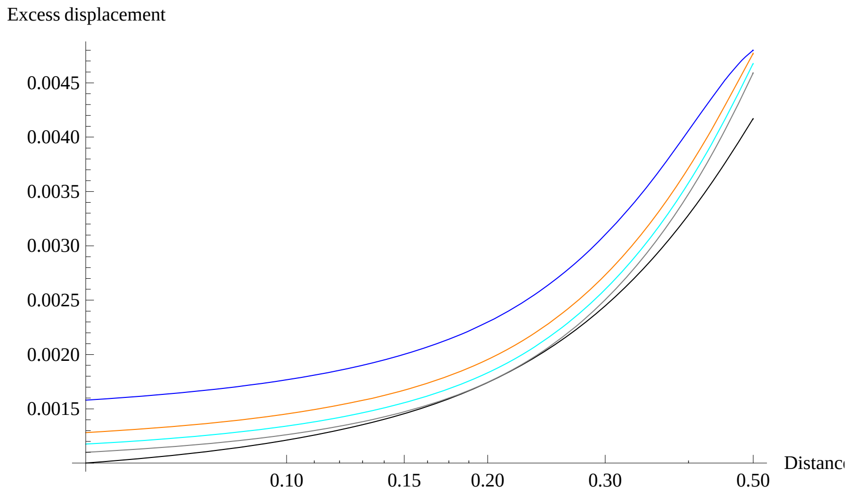


$$\text{Plot}\left[\left\{1-1/2\,e^{-\left(\frac{y-3/4+1/12}{1/8}\right)^2},\ 1-1/2\,e^{-\left(\frac{y-3/4-1/12}{1/8}\right)^2},\ 1-1/2\,e^{-\left(\frac{y-3/4}{1/4}\right)^2}\right\},\ \{y,0,2\},\ \text{PlotRange}\to\text{All}\right]$$



```
pall = LogLinearPlot[{
    (w[-x] /. wsbs) - (w[-x] /. refsol),
    (w[-x] /. nsbs01) + (w[-x] /. nsbs02) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs101) + (w[-x] /. nsbs102) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs11) + (w[-x] /. nsbs12) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs21) + (w[-x] /. nsbs22) - 2 (w[-x] /. refsol)},
   {x, 0.001, 50}, PlotRange → All, PlotStyle → {Black, Blue, Orange, Cyan, Gray},
   AxesLabel → {"Distance from load centre", "Excess displacement"}];
pnear = LogLinearPlot[{
    (w[-x] /. wsbs) - (w[-x] /. refsol),
    (w[-x] /. nsbs01) + (w[-x] /. nsbs02) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs101) + (w[-x] /. nsbs102) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs11) + (w[-x] /. nsbs12) - 2 (w[-x] /. refsol),
    (w[-x] /. nsbs21) + (w[-x] /. nsbs22) - 2 (w[-x] /. refsol)},
   {x, 0.05, 0.5}, PlotRange → All, PlotStyle → {Black, Blue, Orange, Cyan, Gray},
   AxesLabel → {"Distance from load centre", "Excess displacement"}];
GraphicsRow[
 {pall,
  pnear}]
```





The orange solution has been iterated manually to get near the black solution (the wide-lobe bell). There does not seem to be an optimum overlap, since there are side-effects into the centre of the area where the different curves are offset w.r.t. each other, and the wide-lobe solution has a different curvature. At the load centre, the smallest difference w.r.t. to the wide-lobe solution is the one that differs most above the perturbation. (compare gray versus black; see the first and last elements in the following list:)

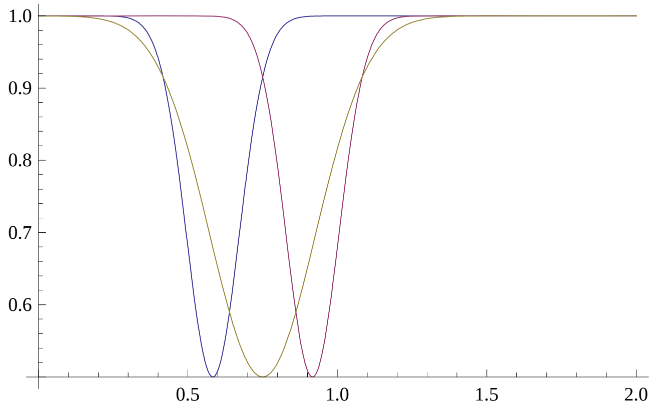
```
Join[
  N[(w[-x] /. wsbs) - (w[-x] /. refsol)],
  N[(w[-x] /. nsbs01) + (w[-x] /. nsbs02) - 2 (w[-x] /. refsol)],
  N[(w[-x] /. nsbs101) + (w[-x] /. nsbs102) - 2 (w[-x] /. refsol)],
  N[(w[-x] /. nsbs11) + (w[-x] /. nsbs12) - 2 (w[-x] /. refsol)],
  N[(w[-x] /. nsbs21) + (w[-x] /. nsbs22) - 2 (w[-x] /. refsol)]] /. x → 0
```

```
{0.000818403, 0.00142965, 0.00114572, 0.00104521, 0.000973223}
```

The greatest difference at the load centre is worth 75%.
I won't go further than express my doubts as to the proposition that linear superposition of any, disjoint to abutted, variations of structure parameters would be trivial.

The perturbation in the orange solution:

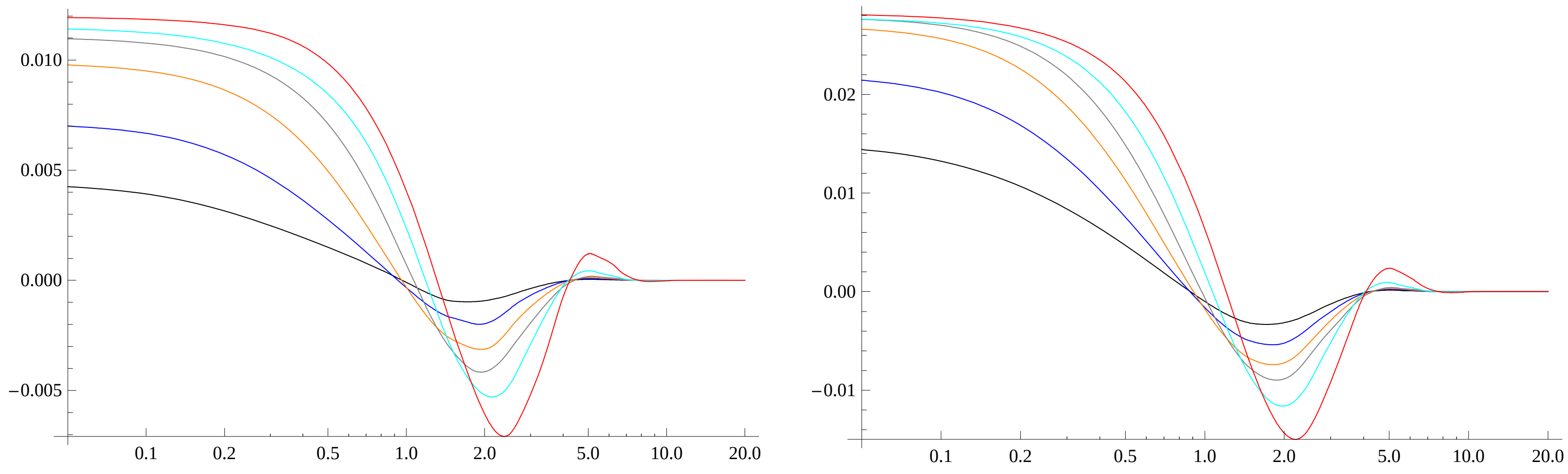
```
Plot[{1 - 1/2 e^-((y-3/4+1/6)/(1/8))^2, 1 - 1/2 e^-((y-3/4-1/6)/(1/8))^2, 1 - 1/2 e^-((y-3/4)/(1/4))^2}, {y, 0, 2}, PlotRange → All]
```



What happens for a load-concentric anomaly when we make it wider and wider?

```
wcbs10 = sol[1, 3, 2, -1/2, 1/8, 0];
wcbs11 = sol[1, 3, 2, -1/2, 1/4, 0];
wcbs12 = sol[1, 3, 2, -1/2, 1/2, 0];
wcbs13 = sol[1, 3, 2, -1/2, 1, 0];
wcbs14 = sol[1, 3, 2, -1/2, 2, 0];
wcbs15 = sol[1, 3, 2, -1/2, 4, 0];
wcbs20 = sol[1, 3, 2, -7/8, 1/8, 0];
wcbs21 = sol[1, 3, 2, -7/8, 1/4, 0];
wcbs22 = sol[1, 3, 2, -7/8, 1/2, 0];
wcbs23 = sol[1, 3, 2, -7/8, 1, 0];
wcbs24 = sol[1, 3, 2, -7/8, 2, 0];
wcbs25 = sol[1, 3, 2, -7/8, 4, 0];
```

```
pcbs1 = LogLinearPlot[{
    (w[-x] /. wcbs10) - (w[-x] /. refsol),
    (w[-x] /. wcbs11) - (w[-x] /. refsol),
    (w[-x] /. wcbs12) - (w[-x] /. refsol),
    (w[-x] /. wcbs13) - (w[-x] /. refsol),
    (w[-x] /. wcbs14) - (w[-x] /. refsol),
    (w[-x] /. wcbs15) - (w[-x] /. refsol)},
   {x, 0.05, 20}, PlotRange → All, PlotStyle → {Black, Blue, Orange, Gray, Cyan, Red}];
pcbs2 = LogLinearPlot[{
    (w[-x] /. wcbs20) - (w[-x] /. refsol),
    (w[-x] /. wcbs21) - (w[-x] /. refsol),
    (w[-x] /. wcbs22) - (w[-x] /. refsol),
    (w[-x] /. wcbs23) - (w[-x] /. refsol),
    (w[-x] /. wcbs24) - (w[-x] /. refsol),
    (w[-x] /. wcbs25) - (w[-x] /. refsol)},
   {x, 0.05, 20}, PlotRange → All, PlotStyle → {Black, Blue, Orange, Gray, Cyan, Red}];
GraphicsRow[{pcbs1, pcbs2}]
```



The grade of weakening and the length scale do interact, see the orange line!

UNFINSHED APPENDIX:

The following scheme would create more box-like perturbations:

```
(w[-x] /. wcbs14) + (w[-x] /. refsol),
(w[-x] /. wcbs24) + (w[-x] /. refsol)
```

$$\text{Collect}\left[\partial_y\,\partial_y\left(\phi\left(1+\varphi\,e^{-\left(\frac{y-\gamma}{\lambda}\right)^{2n}}\right)\partial_y\,\partial_y w[y]\right),\ w''[y]\right]$$

$$\left(-\frac{2\,e^{-\left(\frac{y-\gamma}{\lambda}\right)^{2n}}\,n\,(-1+2n)\left(\frac{y-\gamma}{\lambda}\right)^{-2+2n}\phi\,\varphi}{\lambda^2}+\frac{4\,e^{-\left(\frac{y-\gamma}{\lambda}\right)^{2n}}\,n^2\left(\frac{y-\gamma}{\lambda}\right)^{-2+4n}\phi\,\varphi}{\lambda^2}\right)w''[y]-\frac{4\,e^{-\left(\frac{y-\gamma}{\lambda}\right)^{2n}}\,n\left(\frac{y-\gamma}{\lambda}\right)^{-1+2n}\phi\,\varphi\,w^{(3)}[y]}{\lambda}+\phi\left(1+e^{-\left(\frac{y-\gamma}{\lambda}\right)^{2n}}\varphi\right)w^{(4)}[y]$$

$$\text{FullSimplify}\left[-2n\,(-1+2n)\left(\frac{y-\gamma}{\lambda}\right)^{-2+2n}+2n^2\left(\frac{y-\gamma}{\lambda}\right)^{-2+4n}\right]$$

$$\frac{2n\left(1+n\left(-2+\left(\frac{y-\gamma}{\lambda}\right)^{2n}\right)\right)\left(\frac{y-\gamma}{\lambda}\right)^{2n}\lambda^2}{(y-\gamma)^2}$$

However, it looks hopeless to me - out of intuition.