Review Kollo Spada Vermeer - Equations

HGS

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Kollo et al.'s equation (1), henceforth K(1), and with it the method, is seriously flawed. The text gives no clue as to the meaning of the averaging brackets. But obviously, the site-pairs' index ij is not what's averaged over, since ij labels also terms outside the bracket, and the left-hand side of the equation considers a pair of sites at a time. This only as a side-remark, and as a cue to look at the equation on a per-site-pair basis.

I shll demonstrate the flaw in a 2-d case, where horizontal and vertical strains vary along x and z, respectively, while strain is independent of the y coordinate and displacement along y zero. With reference to a basic text book, Turcotte and Schubert, the equation for the horizontal strain at the surface of the uni-axially bending slab is

$$\epsilon_{xx} = -\frac{D}{2}\frac{\partial^2 w}{\partial x^2} \tag{1}$$

where w stands for displacement in the vertical. I leave out the dots on the variables as all deformation parameters are to be understood as time derivatives.

Since

$$\epsilon_{xx} = \frac{\partial u}{\partial x} \tag{2}$$

equation (1) can be integrated from the crest to a point at distance d

$$[u]_0^d = -\frac{D}{2}\left[\frac{\partial w}{\partial x}\right]_0^d \tag{3}$$

Observe that the right-hand side contains the local derivative of uplift along the horizontal. Since this variable does not behave linearly with distance, you cannot approximate the derivative by large spatial differencing. In symbols

$$\left.\frac{\partial w}{\partial x}\right|_{x=d} \neq \frac{w(d) - w(0)}{d} \tag{4}$$

Two examples can be given that lead K(1) ad absurdum. If $u_1 = -u_2$ and $w_1 = w_2$ (points symmetrically located to the dome axis), D becomes indefinite. Observe that the correctly derived equation features the local partial derivatives of w, which have opposite signs, so there is no division by zero.

If $w = W \cos \frac{2\pi}{\lambda} x$ then with the local partial derivatives

$$\frac{\partial w}{\partial x} = -W \frac{2\pi}{\lambda} \sin \frac{2\pi}{\lambda} x \tag{5}$$

and

$$u = W \frac{\pi D}{\lambda} \sin \frac{2\pi}{\lambda} x \tag{6}$$

so that both equations could be used to eliminate W/λ and estimate D. (In reality you would, besides three-dimensionality, appreciate the non-periodic geometry and compose a softened spike or "Mexican hat" with a wavelength spectrum for W

$$w(x) = \int W(\lambda)\ d\lambda$$

so that (5) and (6) could become the two inputs to a cross-spectrum estimation program.)

The same situation, however analyzed with K(1), would lead to a division of the difference of two cosines by the sum of the corresponding sines,

$$u_1 + u_2 = W \frac{\pi D}{\lambda} \left[\sin \frac{2\pi}{\lambda} x_1 \ + \ \sin \frac{2\pi}{\lambda} x_2 \right] \tag{7}$$

and

$$w_2 - w_1 = W \left[\cos \frac{2\pi}{\lambda} x_2 \ - \ \cos \frac{2\pi}{\lambda} x_1 \right] \tag{8}$$

K(1) thus claims that the ratio of the two brackets, (8):(7), amounts to $-\pi d_{12} \lambda$. Set x_1 small ("Umeå") and x_2 near $\lambda/8$ ("Gv̈le"), and you see how absurd this implication is:

$$1 - \cos x =? \sin x \qquad 1 - \frac{1}{\sqrt{2}} \neq \frac{1}{\sqrt{2}} \tag{9}$$

D'accord?

The bottom line is that Kollo et al. use an illegitimate approximation of a partial derivative. Small but finite differences could do; however the authors do not give any thought to develop the appropriate tools to this end. In the present setting they are in fact forced to use large intersite distances in order to avoid to divide by small and uncertain numbers (w-differences affected by random errors), so the flaw of their approach cannot be cured easily. If they chose to stay in the present framework (pure geometry) they'd need to calculate $\partial w / \partial x$ from sparse geodetic data **locally**, and I cannot see any other way than by curve fitting, which would need careful constraints to not transgress the bounds of physical reality (a penalty function that makes physical sense, like in Scherneck et al., 2010 or better). In any case, I would not like to re-enforce the authors to ignore the physical side of the problem.

You could argue whether the elasticity and the varying thickness of the plate can be ignored, the boundary conditions laterally and at the bottom, where it touches the viscous mantle, too. A paper aiming at the inversion of surface geodetic data to attack a single parameter should demonstrate first a degree of robustness of the method to a number of limitations

- uncertainties and systematic errors in the GPS data
- boundary conditions as origins of other than bending stresses
- the vertical structure of the lithosphere going from $\mu \simeq$30 to $\simeq$70 GPa from the bottom to the top (varying Moho depth!) as a complication of the local uniform-thickness concept of bending while varying—at what scale?—across the area

to answer whether it is thickness at all that is recovered.

The authors should be certain about the reason for the missing of litho-thickness recovery in Scherneck et al., 2010; it was not a missed opportunity but rather the omission of an ill-posed problem. That paper did not argue why it avoided the matter, simply because you use to argue for what you do, paired with the recognition that $\partial w/\partial x$ is not directly observed while the constant thickness of their plate explained the observed and the GIA-modelled deformations to that extent that the residual was near to compatible with the uncertainties of the strain parameters with a persistent gross-feature still conspicuously correlated with the uplift pattern itself, so that a search for a better fit would rather invoke iteration of the GIA model than considering lateral structure variations. The Lidberg et al. [2010] data set still contained the then/now famous "bananas", although reduced when compared to earlier results from GAMIT, so the search for a better agreeing GIA model seemed and still seems premature to me in lieu of independent evidence (for instance using GIPSY).

The high degree of abstraction to reduce the problem to a few basic relations from geometry might look appealing if you can really sidestep the more complicated mechanics of an (even in-) homogeneous continuum to having litho thickness as the only parameter left of concern. However, the ambition reminds me of the Cheshire Cat [Carrol, 1865] who's persistent smile is like the D: The cat itself (the lithosphere) may disappear, but the grin is still there smiling at you. That's in the world of fairy tales.