Solid Earth Discussions

Solid Earth Discuss., author comment AC1
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Reply on RC1

David Hindle and Olivier Besson

Author comment on "A corrected finite-difference scheme for the flexure equation with abrupt changes in coefficient" by David Hindle and Olivier Besson, Solid Earth Discuss., https://doi.org/10.5194/se-2021-36-AC1, 2022

For the final response to reviewer one, we refer to our first comment and reply, but add the following for clarity.

1) As we stated, the paper actually has numerical examples which can determine accuracy by first order principles ("raft" can be deduced from first principles and force balance and broken plate can be compared directly to an analytical solution to an end loaded plate). Both are within small decimals of a percent of the numerical solution. We will however edit the text to make the importance of this point more clear.

2) The comparison to a 2D model was (and remains for us) an issue. It is certainly not clear that a 2D solution to a linear elastic problem is "correct" in any formal sense. It is a completely different approach to the problem, and although 2D, uses a 2nd order formulation. Mathematically, this cannot be the same as a 4th order equation, even if the 4th order equation is derived from a linear elastic starting point.

However, after some thought, we have produced the simplest and most fundamental comparison of the differential part of the equations, and in doing so removed all the additional geometric complexity of 2D (which has nothing to do with the similarity of 1D and 2D results). We have thus produced a simple, elastic beam model without a hydrostatic advective foundation (i.e. the term k u is eliminated leaving only the elastic beam equilibrium). For simplicity, we used FENICS and a modification of the 2D linear elastic tutorial, creating a beam with a "step" in its middle (its thickness reduced by 50% at a single point). The beam is self-loaded by its own weight and clamped at both ends. Hence, we have a step discontinuity in elastic thickness, modelled in 2D, using the linear elastic formulation. This geometry in 2D representation is on a single line, and "instantaneous". In 1D, the "step" must be spread over one single grid spacing. Hence there is always a tiny difference in formulation due to the nature of the approximations. We find a difference of ~3-4% depending on beam thickness, load and so on. However, large deformations are ruled out a priori, because both methods use the small strain approximation of linear elasticity (the 1D equation is actually less sensitive to this than the 2D finite element equivalent). We also note that no solution exists for such a problem with the whole station 1D discretisation. We would therefore propose to add this model comparison to the appendix, with a technical description, appropriate equations and figure, and reference it in the text, with perhaps a small amount of background.

3) Regarding 2D or 1D representations, the evidence of the simple comparison of the 2D finite element solution above is that there are relatively small differences. We have also noted that empirical comparisons show that even in cases of detached, separate, tilted blocks, the 1D comparison performs very well. It actually remains to test whether a 2D model could do the same. If not, we would strongly suggest the problem lies more with the 2D formulation, and the associated additional geometric and parametric complexity, particularly the implementation of the hydrostatic advection term, than anything else.

The remaining specific comments by the reviewer are addressed in our first response. We will modify the text as stated there to respond to them. We will add the Smoluchowski reference at an appropriate place in the text.

We thank Professor Schmalholz for his time reviewing our manuscript.