

## ***Interactive comment on “Modeling of the in situ state of stress in elastic layered rock subject to stress and strain-driven tectonic forces” by Vincent Roche and Mirko van der Baan***

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### General comments

Thank you for the opportunity to review the manuscript. The manuscript address an important topic of stress variations in layered formations, which are usually poorly known, and are not well understood. It is applicable to a variety of practical and fundamental problems in subsurface exploration and geomechanics. The authors' methodology is well explained and discussed. However, I have problems with the basic premise of the analysis, and with understanding the benefits of, and the motivation for the chosen technique:

1) The title and the first sentence in the abstract suggest that the manuscript provides methods to predict stress variation for layered formations. The prediction strategies,

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however, are based on looking at the difference between one of the two simple models (initial models) and either stress measurements or a more realistic prediction (from critical state-of-stress model). What this analysis does, in my view, is showing how far those two initial models are from reality. This could be of some value, too, but should be framed as an assessment of those models. In the current formulation, the following questions remain: Why would we need to model stress at the points where we have it measured? Why would we need to start with initial models, which do a poor job of assessing realistic stresses, if we have a better theory providing stress limits from the critical state-of-stress theory?

2) How does the comparison between initial models and the 'locally measured stresses' (lines 227-229) allows assessing the magnitude of 'tectonic effects'? Would not the difference be comprised of the tectonic effects PLUS the local stress perturbation due to stress/stress partitioning along layer boundaries?

3) Maximum horizontal stress cannot be measured directly, and therefore, cannot be used in the 'reference' model based on measurements. This fact is skipped over throughout the paper, including the introduction and the discussion.

Technical corrections: Lines 49-50 and lines 60-65 describing critical state-of-stress theory are repetitive, consider reorganizing

Line 68: should be '...predictions ... are', not 'is'

Line 237: misplaced comma

Line 244-245: It's not clear whether this is a valid assumption

Line 308: should be 'constants ..are overestimated', not 'overestimate'

Line 871: should be kg/m<sup>3</sup>, not kg.m<sup>3</sup>

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