

Reply to reviewer 3

Manuscript entitled “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

MS No.: se-2016-141

General aspects

RC1: 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, 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?

If one had absolute faith in the measurement then there's no need to model these points. But these are not absolute measurements but inferred values. Their estimation depends strongly on chosen parameters and even personal preferences. Hence the modelling can be required to give additional confidence in their validity. But mostly modelling is used to obtain insights both in between and beyond individual measurements in surrounding layer.

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?

In various cases the initial models give an accurate representation of the in situ stress field and the bounds depend strongly on the material constants (e.g., cohesion and internal angle of friction) which are not necessarily well known. If the bounds are narrow then they are certainly useful in numerous applications (predictions of slip tendency, caprock integrity, fault growth etc) but generally the bounds are quite wide. Likewise, to our knowledge, it does not allow directly to take into account complex heterogeneous system.

RC2: 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?

It is right that the difference between initial models and the 'locally measured stresses' may not be only the magnitude of the tectonic effect and may contain several effects. We assume that one effect dominates.

RC3: 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.

We modified the manuscript to take into account this comment (l.35-39, section 6.2).