Review on the 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 comments

The manuscript deals with in situ stress estimation in layered elastic rock. This is an important issue since current knowledge and approaches are often not sufficient or precise enough or unsatisfactory. The study focusses on the influence of stress at horizontal boundaries where rocks of different elastic properties interact.

The state of stress regarded as being adequately predicted (defined as "reference stress") is assumed as either the state of stress from measurements or the state of stress corresponding to a critical state of stress from frictional theory. Starting with the assumption of two fundamental states of stress which are (1) stress under uniaxial strain and (2) lithostatic stress the layered rock is subject to horizontal stress or strain to attain the reference stress in the layered rock.

The manuscript is well written and well discussed. Nevertheless, I have a number of issues with this manuscript which I briefly state in the following. Further explanation is given under "specific comments".

1) It is claimed that the study is about stress prediction through modeling. In the manuscript stress prediction is being considered as kind of minimum difference between either stress magnitudes from measurements or a critical state of stress and the result of modeling. I wonder <u>why the modeling is necessary. What is the benefit of modeling here, specifically in layered elastic rock?</u> Considering the case study, the stress data along with the remarks in lines 330-333 already give useful information on the state of stress.

2) I would find it helpful to regroup the <u>introduction</u> and to rewrite certain passages or write them more precisely, in particular lines 73-78 which are in large part not understandable without knowing the following sections (see remarks in "specific comments").

3) I miss a <u>description of the numerical models</u> (see e.g. remarks to lines 141/142/244/245/263/630 and 191-193 and 565-569)

4) In general, the <u>magnitude of the maximum horizontal stress</u> is not well accessible. In the considered normal faulting case it corresponds to the intermediate principal stress. The approach is therefore difficult to apply in strike-slip or thrust faulting regimes as far as stress data from measurements are taken as "reference stress".

5) It is assumed that <u>stress changes as well as strain changes are zero</u> in the horizontal orientation perpendicular to the one considered. (I would find it an interesting topic to investigate if anywhere on Earth this assumption is valid). For further explanation see "specific comments". I would like to see something like a proof or explanation that this assumption does not affect the conclusions made.

6) In my understanding it seems that the stress-driven theoretical models are actually strain-driven as the same strain seems to be applied at each layer. In the strain-driven models the layers are said to be coupled whereas they seem to be uncoupled (see e.g. comments to Figures 1 and 2). In case of stress boundary conditions coupling at the interface between

layers does not exclude shear stress and different strain within the layers and different displacement at the boundaries, except at the interface.

Specific comments

Line 35/36: None of the mentioned stress measurement techniques allows measuring the full in situ stress; only part of it like minimum principal stress or orientation of maximum horizontal stress.

Line 58: "… by applying stress updates to initial stress models. This is a common methodology to predict stresses in the crust …"
Without knowing what is explained later on, I think this expression cannot be understood.

Line 62: "Or, this model can provide bounds for the minimum and maximum principal stresses"
The model can provide bounds for differential stress (but not of the magnitudes of σ_1 and σ_3) or for one principal stress if the other one is known.

Line 65: "Therefore the stress differentials cannot exceed their shear strength."
It is well possible that differential stress exceeds shear strength.

Line 68: "stress predictions from the elastic properties of rock", Line 69-70: "stress predictions based on elasticity are commonly used…"
How should that work?

Paragraph line 60-66 could be added to line 49-52.

Line 73: "1D stress prediction": Maybe it could be mentioned earlier that this article is essentially about the magnitude of the minimum horizontal stress and not about the full state of stress or about the orientation of stress.

Line 74: "the models": so far only theoretical models were introduced whereas here it is referred to numerical models. These have not been adequately introduced up to that point. Or just say "in numerical models" instead of "in the models".

Line 75: I think it cannot be understood at this point what "updating strategies" means (see comment to line 58).

Line 76: "Several models are used and combined". It cannot be understood at this point that the "models" that are combined mean very different things here such as: 1.) different assumptions on a general state of stress in the Earth, 2.) different calibration measures and 3.) different modeling approaches.

Lines 83/85/89/94/217/260/473-474: "…Δ_{hl} and Δ_{Hl} are the local tectonic stress corrections."
This implies that no other processes such as the ones mentioned in lines 55-58 are relevant, except tectonic stresses. However, in line 89: "The stress corrections reflect processes such as erosion, sedimentation, temperature change, tectonic activity."
To be consistent either only tectonic stress should be considered <u>or</u> all of the mentioned effects. In the letter case it is questionable whether all the different processes can be represented in one additive quantity, as these processes act very differently.

Lines 91/93/107/141: Until now there is no technique the magnitude of maximum horizontal stress can be measured with. There are some approaches for estimation of the magnitude of the maximum horizontal stress, however these involve assumptions and are far from being accurate.

Line 135: why "and Eq. (7)"? Eq. (8) and (9) refer only to Eq. (6).

Line 137: It should read $\Delta\sigma_{hl} = \Delta\varepsilon_{Hr}\ E\nu/(1-\nu^2)$ instead of $\Delta\sigma_{hl} = \Delta\varepsilon_{hr}\ E\nu/(1-\nu^2)$

Line 141/142/244/245/263/630: "The problem becomes more complex if the second lateral regional strain perturbation is not negligible." "… we postulate that only the minimum regional stress $\Delta\sigma_{hr}$ or strain $\Delta\varepsilon_{hr}$ perturbation exists and that the maximum regional stress $\Delta\sigma_{Hr}$ or strain $\Delta\varepsilon_{Hr}$ perturbation is negligible"
What would be the criterion to decide if it's negligible? The general assumption would be that it is not negligible. Is the approach also applicable if it is not negligible?
Is the modeling done as a 2D section or in 3D? If 2D: plane stress or plane stress? In case of plane stress: is there negligible strain change in the perpendicular direction in the model? In case of plane strain: is there negligible stress change in the perpendicular direction in the model? If 3D: what are the boundary conditions to ensure that there is neither stress change nor strain change in the perpendicular orientation in the model? In fact there must be subsidence or uplift if this condition is to be fulfilled …

Line 156/157: "since no stress transfer or interaction occurs between layers"
This should be only a theoretical case, as it is not realistic, isn't it?

Line 162/163: "if the layers are not coupled"
again a theoretical case. Or frictionless detachment …

Lines 191-193 and Figure 2B/C: numerical and analytical solutions are compared. It seems that in the analytical model boundary conditions at the base and top are not appropriately defined. How are they defined? Or how to explain the difference? See also comment to Figure 2C.

Line 215: The term "reference stresses" has not been defined yet. Maybe shift the bracket in the following line or the definition in lines 227/228 to line 215. Note also that the term "reference stress" in geosciences is generally considered as an idealized stress state in a static crust with no tectonic forces and is thus defined differently from what the term is used in this manuscript ("initial stress" as it is used in this manuscript is a reference stress in common understanding). Another word for "reference stress" as it is used in this manuscript could be "benchmark stress", "calibration stress" or something like that.

Line 221: "We assume a Biot pore-pressure coefficient equal to 1."
Line 291: "low porosity shale members"
A Biot coefficient of 1 is reasonable for unconsolidated high-porosity sands where external stress is taken up by pore pressure. In a tight shale I would expect the Biot coefficient to be much lower, perhaps 0.5-0.7.

Line 248: "… the regional stress perturbation is assumed constant across all layers". Constant stress in each layer implies different strain in each layer if Young's modulus is different.
What is the difference between Figures 1A/B and 1E/F? If in the coupled stress-driven case stress corrections are individual for each layer but then strain corrections are said to be the same for all layers then it is essentially the same as the strain-driven case. See also comment to Figure 1.

Line 260-264: conclusion would be: in situ stress measurements as reference stress can only be used for a normal faulting regime given the maximum horizontal stress, which cannot be adequately determined by measurements, would be needed for a strike-slip or thrust faulting regime?

Line 272-274: “For the strain-driven model, the local stress corrections σ_{hl} are calculated analytically using the vertical variation of Poisson’s ratio ν and Young’s E with Eq. (8). Then, the depth-dependent local stress σ_{hl} is calculated with Eq. (1), the pore pressure P_p, and the appropriate initial stresses σ_{hli}.” Do I understand correctly that the strain-driven case is not numerically modeled but analytically calculated according to Eq.(8)? Then there would be no coupling, contrary to the statement in line 265.

Line 338: “The lithostatic stress σ_l increases linearly with depth”
Line 341: “the minimum horizontal critical stress σ_{3c} increases linearly with depth,”
Which means same density for all layers. Figure 4B is just used for calculation of elastic parameters?

Line 453/454: “the in-situ stress measurements are substantially lower than the critical stresses”
This either means that the stress measurements are wrong or that wrong assumptions were made in calculating the critical stress. See also comment to Figure 5.

Lines 565-569: what is the numerical resolution? Maybe provide element size, element type and whether first or second order elements are being used. How many layers of elements were used to mesh the individual Horn River formations? Is the vertical change in Young’s modulus and Poisson’s ratio as depicted in Fig. 4 represented in the numerical model or is some smoothening applied?

Line 578: “the combined analysis of all eight stress predictions helps reveal the true uncertainty“
The word “true” makes this sentence untrue. If all of the models include the same systematic errors or wrong assumptions, all of the models can deviate from the true state of stress.

Line 599: (A.2) holds, if there is no poroelastic coupling acting, otherwise the term α P_p $(1\text{-}2\nu)/(1\text{-}\nu)$ adds (Engelder and Fischer 1994).

Line 611, Eq. (A.3): why without α in contrast to Eq. (A.2)?

Line 619: Eq. (B.1) should hold only at the interface but not within the layers, if stress applied at the boundary is the same and Young’s modulus is different in the layers.

Figure 1: in case of strain-driven extensional and coupled stress-driven extensional boundary conditions (figures at the top and bottom on the right side): it should read $\Delta\sigma_{hls} > \Delta\sigma_{hlc}$ instead of $\Delta\sigma_{hls} < \Delta\sigma_{hlc}$ if $\Delta\sigma_{hls}$ and $\Delta\sigma_{hlc}$ are absolute values. The same extensional strain applied to a stiff and a compliant material results in lower stress in the stiff material than in the compliant one and in a greater stress **change** in the stiff material than in the compliant one. This can be seen in Figures 5C and 5D in comparison with Fig. 4D, provided that the extensional strain is applied uniform over this depth section.

Figure 1: both, σ_h and σ_H change due to the boundary conditions. How does this fit with the assumption that stress change in the other direction is zero (Lines 141/142/244/245/263/630)?

Figure 1B and 1F: should be $\Delta\varepsilon_{hls} = \Delta\varepsilon_{hlc}$ instead of $\Delta\varepsilon_{Hls} = \Delta\varepsilon_{Hlc}$?

Figure 1: I see no difference between Figure 1A and E and between 1B and 1F. If strain is the same so will be stress (not in the layers but in cases 1A/E and 1B/F).

Figure 2C. If applied stress is -10 MPa then why should stress reduction be less than 10 MPa in the compliant layer and greater than 10 MPa in the stiff layer? Close to the interface I do expect this difference but not far above and below the interface. In other words, I expect what can be seen from the numerical solution in Figure 2B. Conclusion would be that coupling is not correctly represented in the analytical solution. This is probably due to the fact that not only stress but also strain is assumed to be the same at the boundary in case 2C, while it is not in case 2B. If stress boundary conditions are applied then horizontal displacement is not the same along the boundary due to the different stiffness of the layers. See discussion why there is no difference between Figures 1A/E and 1B/F.

The critical stress state in the figures is based on a uniform coefficient of friction. Maybe the clay-rich formations in the case study have a lower coefficient of friction than the carbonates?

Fig. 5: Why are cases 3 and 7 not being shown?

Fig. 5: The critical stress state σ_{3c} exceeds all of the stress measurement data. In a normal faulting regime this cannot be the case as the vertical stress is the maximum principal stress and the stress measurements represent the minimum principal stress. This either means that the stress measurements are wrong or the coefficient of friction (and cohesion) assumed to calculate the critical stress state is wrong or the estimated vertical stress is wrong (wrong average density).

Figure 6: it seems that for the stress-driven cases applied stress change at the boundary is constant over the considered depth interval, at least in Figures 6B and 6C and that the stress boundary condition is adjusted for the depth of the measurements. Maybe it is more reasonable to assume an increase of stress change with depth starting with lower values at the top because stress becomes very small there and is over-critical. In contrast for the strain-driven cases stress changes not much with depth. Here as well a strain gradient could be more realistic when considering not only the depth of the measurements but the whole depth interval.

Figure 6: which criterion was used to adjust the state of stress to the reference stress? Minimum difference, least squares, eyeballed?

Technical corrections

Line 60: "… that the state of stress in the crust is close to the maximum strength that rocks can support…" This basically means a tensor is compared to a scalar value. Could be more precise.

Line 68: either "are" instead of "is" or "prediction" instead of "predictions"

Line 86: "an uniaxial strain model" instead of "a uniaxial model", see line 53.

Line 138/146/232/236/262/284: should read "normal faulting regime"

Line 153: should be "extension" not "shortening", compare Fig. 2

Line 170: should be $\Delta\sigma_{Hr}$ instead of $\Delta\sigma_{hr}$

Line 171: should be $\Delta\sigma_{hr}$ instead of $\Delta\sigma_{Hr}$

Line 172: exchange 1C/1D or σ_h/σ_H

Line 221: "either to the stress of the uniaxial strain model" would be more precise

Line 232: “normal faulting” instead of “normal fault”

Line 235: as the maximum horizontal stress cannot be measured, just estimated at best, one may erase some words “data point to calibrate ~~either~~ the minimum~~, or the maximum~~ principal stress at a specific depth. This could be done with in-situ stress measurements ~~available in one horizontal direction~~”

Line 237: insert “coefficient of” before “friction”

Line 247: should be Eq.(2) instead of (1) and Eq.(3) instead of (2) ?

Line 264: maybe “minimum” instead of “minimal”

Line 278: “An in situ initial state of stress”: maybe erase “in situ”

Line 310: should be NE-SW ?

Line 311: does also a gross difference exist?

Line 322/326/327: MPa km^{-1} , not MPa/Km^{-1} or MPa.Km^{-1}

Line 338/342: maybe erase “slightly” because there is a substantial difference between the two

Line 353: why “even”? Should be always the case.

Line 375/406/413/428 etc. I would find it helpful to insert “(Table 1)” after the strategy numbers a few more times

Line 384: “target” instead of “targets”

Line 442: should be “except”, not “expect”?

Line 445: should be $\sigma_{3c} < \sigma_h$ and not $\sigma_{3c} > \sigma_h$ if “infra-critical” means not critical (σ_I is the vertical stress, which is in a normal faulting regime greater than the horizontal stress, so the critical stress must be smaller or equal to the horizontal stress)

Line 446: should be $\sigma_{3c} > \sigma_h$ if “supra-critical” means more than critical

Line 455: maybe “requires” is better than “produces”.

Line 462: “MPa” instead of “MPA”

Line 462: “formations” instead of “Formations”.

Lines 484/489/492/596: “an uniaxial” instead of “a uniaxial”

Line 514: “fault” instead of “faults”

Line 525: maybe “elastic parameters” instead of “elasticity”

Line 554 thus are prone

Line 594: although it is about stress it should read “uniaxial strain model”

Line 596/597: “creates a uniaxial horizontal stress σ_u due to the lateral extension of the medium that is impeded by non-deformable walls (i.e., no lateral strains).”
I understand the meaning but it sounds like a contradiction, maybe write differently

Line 614: “lower”, not “higher”

Line 630: “…with zero stress and zero strain in the second horizontal direction…” It should read “stress change” instead of “stress”?

Line 640: “layer” not “layers”

Figure 2, diagrams on the right side: the shallowest depth should be 2200 instead of 2400 m

Line 847: must be extension not shortening

Line 848: Section 2.5 does not exist

Line 849: Sections 2.6 and 2.7 do not exist

Line 850: insert “m” after 2400

Line 850: maybe add density used to calculate the lithostatic stress

Figure 3: should be ρ instead of ν below the box “lithostatic model”

behavior/behaviour: sometimes with “u” (lines 306, 515, 557), sometimes without “u” (lines 28, 53, 57, 69, 81, 143, 156, 531, 616)

modelled/modeling, sometimes with two “l” (line 276), sometimes with one ”l” (line 1,47, 77, 187, 191)