Review

Understanding the stress field at the lateral termination of a thrust fold using generic geomechanical models and clustering methods

by A. Adwan et al.

Dear Editor, dear authors,

first of all, a brief summary of the manuscript (as I understood it):

The study investigates by means of a numerical model the reactivation of a thrust fault above a detachment horizon in a compressive tectonic regime as well as the expected geometry of a lateral continuation of that thrust fault, depending on the friction of the pre-existing thrust fault and of the detachment.

With ongoing tectonic loading the Coulomb criterion is reached at some point in the model domain. At this stage the state of stress and strain in the model domain is related to certain measures quantifying the proximity to failure of the rock. The spatial distribution of these measures is used to predict at what locations a fault is likely to develop in the lateral extent of the inherited fault. It turns out that the locations of developing faults can be quite different depending on the friction angles of the detachment horizon and the inherited fault. The pairs of friction angles (of the basement surface and inherited fault, respectively) cause significant changes in the medium principal stress (minimum horizontal stress) whereas the maximum and minimum principal stresses (maximum horizontal and vertical stress) are not much affected by the frictional properties. A large amount of friction angle pairs  are tested and grouped into clusters with respect to the location of the emerging fault and reactivation of the pre-existing one.

I think the selected topic of the expectable location and shape of a laterally continued thrust fault along strike of a pre-existing fault is relevant. The manuscript is well written. Overall, I find the study worthwile for publication but there are some points that need clarification regarding the model set up and analysis. Although a generic model is presented it would be valuable to have a discussion on whether and/or how the findings of the study relate to observations.

Below you find comments refering to specific lines. The comments are grouped into more fundamental issues and small ones.

**Main comments**

Lines 65,66 and Table 1: why is only density, cohesion and internal friction angle specified for the rock mass? As I understand, the Coulomb criterion is used to define critical stress states but below this threshold there is elastic deformation involved which would require the specification of elastic parameters such as K, $\mu$ or E,$\nu$.

Line 71: it would be helpful to mention what kind of software it is: what method (finite element method?) is used to solve which equations?

Line 74: Why is the load unknown? There is no numerical solution if boundary conditions are lacking.

Line 90: lower bound limit analysis: what is that?

Line 91, 97: triangle and tetrahedral elements are those elements with the lowest numerical precision. And the number of elements used seems rather small to me and the mesh in Fig 10 looks rather coarse. Did you perform a test in order to evaluate whether the specified resolution is sufficient for reasonably small numerical errors? I see that you run quite a large number of models which would take a lot of time with high numerical resolution. But you could test one model with smaller elements for one specific set of friction angles for the fault and the detachment and compare the result with the model you have. Or even simpler, use the tetrahedron mesh and switch to second order elements if available and compare the result with the result from the model with linear elements.

Line 97: mixed bound analysis. I asume that it is explained in the given reference, but it would be helpful to briefly explain what it is. Is the reason why you use mixed bound analysis here and lower bound analysis in 2D (Line 90) the dimension (2D vs 3D)?

Line 109: ok for the 2D case but does it also hold for the 3D case if S1 is not parallel to the x axis (which is likeley the case in some areas of the 3D model)?

Line 126: I'm missing definitions of $\phi$ and c

Line 186 and 195 and caption of Fig. 4: is it the angle between S1 and the x axis or is it the angle between S1 and the x-y plane (S1 is not necessarily in the x-z-plane in the 3D case)?

Line 207/208: optimized external load: 1) what is the amount of the load? 2) with respect to what is the load optimized? Is it what you write in lines 209/210? If so, I would mention this already in the section where you describe the model set up.

Line 244: Why do you choose the value of 0.008? Do you expect your results to change qualitatively if this value is changed? Why do you choose another value than in 2D, compare Line 139? In Line 245 you call 0.008 a low value but it is four times as high as the one you apply in 2D.

Line 258: It took me quite a while to realise that the geometry of the created fault on the right side is an outcome of your simulation (or subsequent analysis). When looking back there were some hints on that (e.g. Line 49, Fig. 1c, Line 248 „three distinct options emerge", Line 278, etc.). Maybe, this is just my slow understanding but it may be helpful to others to make this more clear in advance in the introduction.

Line 262, caption of Fig. 7, Line 339 etc: velocity field: were does the velocity come from? There is no time-dependent material law implemented. Is there a time-dependent load applied at the back edge of the model? Or do you mean displacement field?

Lines 284/285: Why is there a deviation of the critical basement angle between 2D and 3D models? Is it because of different values for $\delta$? See also comment to Lines 379-381.

Line 372: I find the term „rupture" misleading. Do you actually show a rupture distribution (in terms of a length unity)? What you are showing is a dimensionless quantity related to the distance of stress and/or strain to a failure criterion. Maybe I still didn't get it. Is there actually frictional sliding and/or plastic straining of the bulk material occurring in your model or do you have basically a model with elastic bulk material in which simulation terminates as soon as the yield criterion is reached at some point? If the former is the case you could show the amount of frictional sliding and plastic strain and if the latter is the case you could plainly state it.

Lines 379-381: Here you discuss some differences in the results between your 2D model and the 3D model, which you refer to the fact that the 3D model exhibits the inherited fault only at the left side. Differences between the 2D and 3D models may also arise from the way you defined your 2D model.

Did you use plain strain, plane stress or generalised plain strain in your 2D model? See also comment to Lines Lines 284/285.

What is the reason at all why you use a 2D model?

**Small things**

Line 3. I would omit the expression „in a kilometric-scale model“ at this position or shift it to the end of the sentence in order to avoid strange associations („a model developed over a basal detachment“).

In line 3 it is called a „fault“, otherwise it is called a „fold“. I'm not a geologist but In my view what you are dealing with is a fault, not a fold. A fold may develop obove a fault but what should be folded here, the material is hnomogenous, not layered? I doubt whether it is possible to explain the creation of a fold by critical wedge theory.

Lines 9, 36, 47, 121 etc.: I find the term „rupture“ misleading in this context. With rupturing I associate specifically the dynamic frictional sliding during an earthquake, which is not modelled. More general, terms like „plastic deformation“, „shearing“, „creation of shear planes“ include aseismic behaviour as well.

Lines 20/21 and throughout the text: in general there are no brackets around year numbers if they appear within another bracket: (Zoback, 1992; Seggal and Fitzgerald, 1998; …)

Lines 29-31: „the focus was centered on … the maximum horizontal stress“: yes and no. Regarding the orientation of the maximum horizontal stress, you are right. Regarding the magnitudes: in the paragraph above all those methods used to a measure stress magnitudes, particularly the more reliable ones such as hydraulic fraturing, FITs, LOTs, etc. , aim at the minimum principal stress which is never the maximum horizontal stress.

Line 65: I would call „specific gravity“ gravitional acceleration

Line 66: just to be precise: as I understand, the term „basement“ does not refer to the crystalline rock in contrast to overlying sediments but to the base of the model (which may nevertheless reperesent the interface between sediments above and basement rocks below) and which is concurrently implemented as a detachment horizon?

Lines 65,66: is a specific rock type inteded, the specified density may represent granite …?

Line 75: why is vertical movement not allowed? If vertical movement would be allowed the high stresses at the back may become lower as uplift is possible. … Ah ok, you have a contact surface between the back wall and the bulk material. Is the surface of the model (except at the back edge) uncontrained?

Line 83,84: There are different mechanisms that can result in a detachment horizon e.g. layers containing salt , evaporites which would cause some sort of creeping instead of frictional sliding. Layers with very high pore pressure may result in the same phenomenon. But I agree, in terms of stress evolution above the detachment horizon there might be not so much difference depending on whether it's frictional sliding or viscous creep.

Lines 107-109: it may be helpful to have a small sketch illustrating the angles

Line 139: Is there a reason why you chose right these numbers? Do you expect qualitative changes of your results if other numbers were chosen? What geomechanical significance do these quantities have?

Line 164: in Fig. 2b you state „single V", whereas in the text you mention „two clearly distinct V positions". Is this an error or if not how can I understand that? Is it that in each model run only one V emerges but this one V occurs at two separated locations for cluster C1?

Line 160-182: I would refer several times to Fig. 3

Fig. 3 I find it a little difficult to discriminate the green lines from the colours of the contour plot. How about replacing the thick green lines by thin dashed white lines?

The contour colour is T, but where is d mentioned in the caption? Is d shown in terms of T (eq. 7)?

Line 197: replace that by than

Line 201: stresses exceeding 200 MPa at relatively shallow depth is quite a lot. Considering that these values are not principal stress magnitudes but mean stress and deviatoric stress, respectively, some principal stress magnitudes must be even way higher (e.g. SV ~ 95 MPa at most in your model, when calulated as the weight of the overburden). Although this research is an academic exercise I wonder if such high stresses are in any way realistic in nature.

Line 216, also Lines 11/12: stress decrease with depth: I only see an increase of stress with depth. Please describe, where you see it. Or do you mean the lateral decrease of stress which you refer to as stress drops in the following sentence? I would not call these stress drops as this term is often associated with the stress release occurring during an earthquake (see also Fig. 4 and caption etc.).

Fig. 10: I think most readers will know what mean stress and deviatoric stress is but it may be helpful to some if a definition is given (both in 2D and 3D).

In the caption of Fig. 4 I would replace the first word „Mean" by „Average" as indicated in the figure to avoid confusion with mean stress.

Line 236: I would be more specific: not necessarily high stress values in general indicate imminent failure but rather high deviatoric stresses. If the stress state is isotropic high stress levels should not lead to failure since the compressive strength of rocks is generally much higher than shear strength. Also the term „high" per se is not necessarily indicative of failure but high in relation to the yield level which increases with mean stress.

Caption of Figure 6 and/or Line 249: I would mention that in Fig. 6a one example of the cluster X1 is shown.

Line 257: insert friction between basement and angle

Lines 291/292: why not following the sign convention used in geoscience? This would make some of your descriptions easier to understand, at least it should be consistent. E.g. in Fig. 8 you write „stress decrease" but with the engineering convention it would be an increase. In Line 302 you write „values higher than 200 MPa" but with the engineering convention it would be smaller than -200 MPa, Line 348: decrease etc.

Fig. 8: Just suggestions:

In the scale for stress blue colour mean high stress whereas in the scale for standard deviation red colour mean high stress. I would find it easier if the same convention was used

It may be helpful to draw lines in the four figures that indicate the course oft he fault, which is different in a/b and c/d

Caption of Fig. 8: it should be a and c instead of a-c and it should be b and d instead of c-d

Lines 308/309: Again, the deviatoric stresses are very very high, see comment above. What do you mean by „prominent“ in Line 309? At the basement it seems that q is smaller than p (compare Figs. 8 and 9) or do you mean variability?

Caption of Fig. 9: it should be a and c instead of a-c and it should be b and d instead of c-d

Line 315: I wonder if „validate“ is the correct word here. If you validate a result you would need some independent information that is not part of the model.

Line 324: „rupture location prediction“ sounds like forecasting the hypocentre of a future earthquake. Is it that what you intend?

Line 328: principal instead of principle

Fig. 10: There is hardly any difference between X1 and X3 and the information content of this contour plot is limited. I would suggest to use not the Andersionian classification of tectonic regimes but a scale of the tectonic regime as suggested by Simpson (1997):

Simpson R.W., 1997. Quantifying Anderson's fault types, J. Geophys. Res., 102, 17909–17919. https://doi.org/10.1029/97JB01274

Line 346: I would find SH/SV more helpful than S1/S3 because interpreting S1/S3 involves dealing with the orientation of the principal stress axes.

Line 351/352: „constant value with depth (equal to 2)“: In the figures S1/S3 seems to be higher than 2 and not constant over the depth range of the model

Line 364: The tensional stresses of S3 extend to depths of ~1.2 km. Are you aware of observations of tensional stresses at such depths?

Line 366/367: Are you referring to Fig. 11a2 and b2? I do not see what you mean. S1/S3 = 2 and turning to negative values …

Fig. 11: At the surface S1 approaches or even exceeds 50 MPa, which is quite a lot. Do you know of any observations of such high stresses at the surface? Of course, your model is generic but nevertheless it should be made clear in what aspects the model explains realistic conditions and discuss potential simplifications that cause the model results to deviate from observations (e.g. weathered fractured zone in proximity to the surface with lower stiffness and/or strength, etc.)

Line 377: I think validation requires a 1:1 reproduction of existing models or analytic solutions. In a less strict sense validation could mean a comparison of model results with observational data. Otherwiese I would not use the word validation but rather terms like „… qualitatively agrees with …“, or discuss specific aspects of your results which are supported by previous findings.

Lines 427-429: You conclude that focusing on S1 and S3 may lead to biased interpretations. Thinking about a practical application I wonder if one is at all in a position to make interpretations based on S1 and S3. In a tectonic regime of compression what you have is basically S3 as the vertical stress from the load of the overburden. If you do stress measurements using the most common and reliable methods you get S3 which unfortunately corresponds again to SV in a compressional tectonic regime. SH=S1 is generally derived from S3 measurements with generally large uncertainties.

Line 432-434: You describe a temporal evolution („onset“, „not instantly“, „starts“, „spreads“). If there is no time-dependent material law involved or a time-dependent load at the back I don’t see where a temporal evolution should come from.

Line 515: The initial K. of the name appears twice.

A final comment:

Noticing that ENSI is among the co-authors curiosity is awakened as to what purpose this study may have in the context of the geological situation in northern Switzerland where the designated location of the Swiss repository for nuclear waste is investigated. I understand that this study, due to its generic character, is not intended and not suitable to make a contribution to the task of planning of the repository but it would be interesting to know in what aspects this study is addressing the geology/tectonics of this area (eastern prolongation of the Jura fold and thrust belt?), if at all, and in what way the model could be adjusted to consider more details of the conditions encountered there (e.g. presence of the Baden-Irchel-Herdern Lineament as a potential reactivated feature to the east (right side) of the inherited fault (Jura fold and thrust belt), changing frictional conditions of the basement from the west to the east side (left to right side) due to the thinning out of evaporitic layers which may explain the location of the present eastern end of the Jura fold and thrust belt, etc.). But I understand if you would not go into this in the manuscript and I do not expect this.