Review Report

Melosh et al. – Structural Control of Fluid Circulation and Permeability in Active Transpression Along and Adjacent to the Bartlett Springs Fault, Northern California Coast Ranges, TEKTONIKA, 2026.

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1st Round of Revisions

Response to reviewers

Dear Editors and Reviewers,

Thank you for your time and attention to this manuscript I think the comments have improved the paper and I appreciate your commitment to detail and helping me pay more attention to uncertainties in the analysis. Here I have included detailed responses to all the comments for both reviewers and the editors and described what I have changed, or in a few cases, why I have decided not to change anything. I hope you find this satisfactory for publication, please let me know of the next steps in the publication process and if the manuscript requires more attention. Original comments are included in blue text.

Thanks,

Ben Melosh

Editor comments:

Fig 6 is rather crowded with colors, and the dark grey background tends to "absorb" the

information indicated by the lines (at least to my eyes, I have slight red-green

colourblindness). It would be much better to "tune down" the background to allow the lines

to stand out.

Figure 6: I have made the hillshade background a lighter color which I think brings out the colors of the dilation tendency results much more clearly.

Reviewer 1

Major comment 1: Method naming and scope (Td vs. "stress modeling")

1. Method naming and scope (Td vs. "stress modeling").

Throughout, you describe a "static stress model" and in places this reads like you did

your own stress modeling. What you actually do is compute dilation tendency using an

assumed principal stress tensor largely taken from prior inversions. Please revise the

terminology accordingly (e.g., “map-based dilation tendency analysis under an assumed

stress field”) and separate what is assumed from what is modeled/computed. Your own

note acknowledges this distinction.

I have changed all instances where I mention “stress modeling” and replaced them with “dilation tendency analysis” as fitting with the text.

Major comment 2: 2-D treatment and vertical-dip assumptions

2. 2-D treatment and vertical-dip assumption are a major limitation, not a minor one.

You assume all faults are vertical “because the majority are strike-slip” and because the

analysis is 2D; yet you also cite mapping and geophysical work indicating significant dip

variability (50–80° oblique-normal faults near the hinge; folding of serpentinite at 2.5–5

km), which directly undermines that assumption. This should be elevated to a primary

limitation, with either a 3-D sensitivity or at minimum a systematic dip-variation sensitivity

analysis (e.g., ±20–30° for NE and NW sets). Also reference the stress-inversion figures you

cite that show S2 is seldom perfectly vertical.

The 2D assumption is a big one, but it is the reality when working in this area as the majority of faults are not exposed well enough to measure orientations from. I would love more 3D data but the fact that we have dips on several faults is a win. This study is aimed at a goal of presenting a conceptual structural model for fluid flow in a transpressional setting, rather than a more detailed resource

development focus. If we were to add a 3D component here only a few faults would be included, some sort of assumptions would have to be made on the majority of the faults. I decided not to go this way because I felt like it would give a false sense of certainty that we don't have. It's better to acknowledge the limitations.

However, the reviewer's comment is well taken, and I do see the need to be more quantitative with the uncertainty. This is why I have included an uncertainty analysis addressing variations in fault dip and variations in stress orientations (see the new Figure 8) and subsections 4.2.1 and 5.2.5 "Dilation tendency uncertainties". The stress inversion study (Provost and Houston, 2003) is cited various times in the paper and I have added the angular uncertainty that they calculated from stress inversion locations closest to the study area in the uncertainty analysis. That uncertainty was plotted in three stereonets depicting the possible range of fault dips (ranging from 90 to 50 degrees) for NE and NW sets in Figure 8.

Major comment 3: Clarify stress regime and principal stress usage

3. Clarify stress regime and principal stress usage.

Please state explicitly that SHmax is treated as s1 in a strike-slip regime and justify the

chosen principal stress magnitudes (50/20/5 MPa). Explain why Hayward Fault stress drop

was used as a proxy and discuss implications for uncertainty transfer to BSF. Consider

adding a stress-magnitude sensitivity (+- 25%) to show robustness of Td patterns.

I added explicit mention that SHmax is treated as sigma 1 in a strike-slip setting in the Methods section (section 3.2). I do mention why the Hayward fault stress drop was used (section 3.2), but I have added more discussion of the assumptions this requires in the Discussion section (section 5.2.5). I added stress magnitude sensitivity in Figure 8 showing changes in differential stress with depth in Figure 8B, which includes variations in fault dip as well for strike-slip, normal, and thrust fault settings. I added more text about this in the Discussion section (section 5.2.5).

Major comment 4: Avoid over-interpretation of Td as guaranteed permeability

4. Avoid over-interpretation of Td as guaranteed permeability.

High Td implies favorable tendency for opening under the assumed tensor; it does not

guarantee open, conductive fractures where s3 remains compressive at depth, nor does it

capture rock stibness and fault roughness ebects. Please rephrase “statically propped

open” language and consistently frame your results in terms of Dilation Tendency.

This is a good point and something I tried not to do but perhaps didn’t address well enough. I have gone back through and changed the wording around instances of “statically propped open”. In the Methods and Discussion sections, I added reference to the fact that dilation tendency does not guarantee open fractures as things like roughness or rock stiffness may play a role (section 3.2 and section 5.2). In the discussion I added more detail and I made sure not to overstate conclusions and be clear that Td is just a single metric in a complex problem.

Reviewer 2

General comment A:

A: This may look like a picky point on nomenclature, but (see next general comment) I think it’s important for clarity, and for the mechanics and broader applicability of the suggested fluid flow paths. I have always learned to separate stress and strain terms, specifically using ‘dilation/extension’ and ‘compaction/shortening’ to describe strain whereas ‘tension’ and ‘compression’ are stress terms. Therefore, I struggle understanding exactly what the authors mean by terms such as ‘extensional stress’. With that term as an example, with ‘extensional stress’, do the authors mean a stress field typical of crustal

extension and normal faulting - i.e. vertical sigma_1? Or do the authors mean 'tensile stress' (i.e. a stress acting to pull apart)? These are very different stress states and the wording is ambiguous, and I therefore urge the authors to go through the manuscript and clarify use of these terms, and define what they mean by, in particular, extension and compression - see specific comments for examples.

I had to go back to my textbook on this, but the term 'extensional stress' refers to a situation where the sigma 3 is greater than zero, whereas tension implies sigma 3 is zero or less. "*It is unfortunate that the term extension has a different meaning when applied to strain, and the distinction should always be made clear*" Twiss and Moores pg. 155. However, I think that this textbook is perhaps incorrect on this issue and may have misled me, see "*Marrett, R., Peacock, D.C.P., 1999, Strain and stress, Journal of Structural Geology 21, 1057-1063*". So, I went through the paper and removed every mention of "extensional stress" and replaced with either "tensile stress" or "extensional deformation" as appropriate.

General comment B:

B: The authors define a stress field by magnitudes and the orientation of SHmax in line 251. The authors discuss errors in the stress field related to the orientation of SHmax, which are pretty minor. However, what about variations in the 3D stress field and particularly stress magnitudes? As all stresses are > 0, the stress field is completely compressional, which is where my confusion about 'extensional' stresses comes from. Depending on what the authors actually envisage when discussing extensional stresses (see above), there may be implications for which principal stress is vertical, which will affect the calculation of dilation tendency as it by necessity will change the magnitudes of the principal stresses (assuming vertical stress is not changing dramatically, the horizontal stress magnitudes will have to change). This also questions the assumption of the principal stress magnitudes being constant across the area, which appears to be an assumption of the dilation tendency calculation (paragraph starting line 251).

Variation in the stress field is something I address in the new manuscript draft with the inclusion of the new Figure 8, which shows an uncertainty analysis that includes variations in stress field magnitudes, orientations, and fault dips. Variations in the 3D stress field are shown in the stereonets in Figure 8 (3D angular error of 20 degrees) and included in the Discussion (section 5.2.5) and Results (presented on in the final four paragraphs of section 4.2).

General comment C:

C: The discussion could be made clearer if the authors explicitly discussed that fluid flow

depends on (at least) three factors - a permeable pathway (focus of the paper), but also the presence of a fluid source and of sufficient hydraulic head/gradient to drive flow (could simplify this more, and say you need a fluid pressure gradient from a source to a sink). Several discussion points regarding mismatches between hot springs and dilation tendency could then be simplified to evaluate whether a fluid source or hydraulic gradient is available in these areas. See the specific comments for examples.

I added a new paragraph to the discussion that mentions other factors that control the location and pathways for fluids, including hydraulic head (section 5.2).

General comment D:

D: Probably not really in the scope of the paper, or constrained by the data, but what about temporal variations in stress and permeability? There are discussions early in the paper about transient variations in permeability - it might be worth revisiting this at least briefly, at least with the caveat that fluid flow and dilation tendency may both vary in time, and therefore the current snapshot may not be fully representative (e.g. what is now a relatively cold spring may become warmer at times where, transiently, the permeability changes).

I added a paragraph to the discussion about the time dependence of permeability and dilation tendency which is not captured by our analysis. This is included in the second from the last paragraph of section 5.2.5.

Specific Line Comments:

Line 9: What is meant by extension here - a strain in a particular orientation (text book meaning), or a stress state (that sustains open fractures - i.e. one that leads to low enough effective normal stress to not close the fractures)?

Line 9: Extensional strain, I changed use of stress and strain terms and tried to be explicit where need.

Lines 10-11: Here I understand ‘extension’ and ‘compression’ to describe Andersonian stress regimes. If that is not correct, then please clarify.

Line 10-11: I switched to only using strain terms here so as not to confuse stress and strain.

Line 26: What is extensional stress?

Line 26: See General comment A for this case and all others, I changed to "tensile stress".

Line 28: Here I read 'extension' as a local strain (in some direction) - correct? If not, please clarify.

Line 28: changed wording to "extensional deformation".

Line 30: 'up welling' = 'upwelling'

Line 30: Fixed

Lines 36-38: It is true that these structures can create transient permeability - however, they can also do the opposite, i.e. lead to local compaction and reduction in flow. Would be worth specifying this.

Line 36-38: I added two sentences in here to point this out and highlight the importance of this work.

Line 40ff: Correct, but these effects compete with healing, so as stated before, they are transient (while the analysis in this paper assumes static stress and fluid flow). See for example Sibson (1996, already cited).

Line 40: I added two sentences in here to point this out and highlight the importance of this work.

Line 60: Please help out those of us that don't know the region - ok, I can look at the map, but something like 'extends about 30 km further NE to the Bartlett Springs fault' would be a lot more effective than 'extends all the way to the Bartlett fault' .

Line 60: Changed.

Line 61: What is the age of the Geysers complex? (give us a sense of timescale for the geothermal system)

Line 61: I added age of the Geysers plutonic complex for reference.

Line 67: Extensional stresses in the hinge? What does that mean? Local tension (seen by generation of tensile fractures) or a local rotation to an extensional Andersonian regime (i.e. local preference for normal faulting)? Or do you mean extensional strain in fold hinges?

Line 67: see general comment A above.

Line 71: Define ‘compressional setting’ in this context - Andersonian regimes with vertical sigma_3?

Line 71: Changed wording a bit to reflect strain terms to be a bit more specific.

Section 2.2 and Figures 2 and 3: A cross-section would be really useful in both of these figures, as a lot of the description is of 3D geometry that is a bit tricky to visualise. In the text, there are various descriptions of geophysical data that are not shown in the maps (and as subsurface interpretations, they can’t really) - but, the relevant interpretations could be indicated in crosssections in Fig 2 and 3.

Section 2.2: I referred readers to a cross section in previous work that includes the Wilbur Springs area (cross section 11 Langenheim and others (2024)), for this paper I will use the 3D block diagram.

Line 152: Shortening in what direction? I don’t see how the rate supports the stress inversion data, but the orientation of shortening (which is not stated) could clearly be compared to the stress orientations.

Line 152: I rephrased the sentence, so I am not comparing GPS to stress inversion orientations.

Line 213: ‘wide spread’ = ‘widespread’

Line 213: Fixed.

Line 228: Totally agree with all of this - however, there is also the limitation that the temperatures are representative on some restricted temporal scale (I don’t know what this scale is, but presumably related to flow rate from the sampled source to the current fluid sampling location).

Line 228: I added reference of the uncertainty of the temporal scale.

Line 251: Are these effective stress magnitudes? Which stress is assumed to be vertical?

Line 251: This is total stress and sigma 1 is assumed to be horizontal. I added mention of this.

Line 393: Ok, the dilational tendency at Crabtree Hot Springs is not particularly high, but in Fig 6c it still looks higher than in the adjacent segments. In addition to the structural complexity discussed here, the 2D study assumes a constant

stress field, and relatively local variation (for example local change in differential stress) might make small differences in fault orientation more important.

Line 393: I added a sentence to the end of this paragraph detailing this, good point.

Line 399ff: So this paragraph is a nice discussion on how serpentinite might act as a barrier to flow - however, a question it rises is where the fluid is coming from. What is the envisaged source, and where is it relative to the serpentine? Or in other words, how serpentine affecting the flow path - is it meteoric water going down, hitting a low-permeability serpentine, then eventually returning to surface (so circulation of a meteoric fluid) or is there a fluid source below the serpentine, and serpentinite bodies constrain the potential paths for vertical ascent of this fluid?

Line 399: Another good point, I added a sentence to the end of this paragraph.

Line 413: What about serpentine polytype? This discussion lumps all serpentines together, but are there differences between the polytype of the different occurrences (i.e. Coast Range Ophiolite, detrital serpentines, Franciscan, etc.)?

Line 413: I distinguish detrital serpentinites from sheared serpentinite mélange of the Coast Range ophiolite and mention that detrital serpentinites may have higher porosities. I don't think any further distinctions are necessary for the purposes of this paper. Many of the serpentinites mixed into the Franciscan are Coast Range ophiolite serpentinites and differentiating them is difficult.

Section 5.1.1: Ok, this section discusses the uncertainties in the orientations of the horizontal stresses, but little about magnitudes, including whether there are stress field variations including changes in which principal stress is vertical. If the magnitudes change (e.g. where there are 'extensional stresses') then that would affect the dilational tendency. For example, if differential stress is small, then smaller variations in sigma_n will have greater effect on dilational tendency.

Section 5.1.1: I added discussion of the magnitude uncertainty including a new plot of dilation tendency with varying differential stresses.

Section 5.2: Throughout the paper, I have wondered about the exact meaning of 'fluid circulation', which to me implies exactly that - fluids are circulating. However, in this section, and Fig. 9, the picture seems to be that a fluid at depth is flowing roughly vertically upwards. In that case, it's not really circulation, which would also need a downward path to complete a circle (e.g. in a case where meteoric water flows down and then comes back up). So, can you be

more precise about the flow path, and avoid the term ‘circulation’ unless you can describe an actual circulation of fluids?

Section 5.2: Circulation of fluids is meant to include circulation at any scale including the downward flow of metoric fluids. However, because we did not really look at downward flow, I left it off the conceptual block diagram. All we might say is that downward flow is likely not along the same fractures that the hot springs are sourced from which would cool off the upwelling fluids. To address this, I added a short description toward the top of the section.

Line 475 and 481: What is meant by an extensional stress imparted by an active fold? Is it a change in stress field - which violates the assumption of constant stress orientations and magnitudes - or do you mean an extensional strain is locally imparted by the constant stress field?

Line 475 and 481: Extensional strain will be focused near the hinge of the fold because of tensile stress. I changed this in the text.

Lines 489-490; The observation of steep dips is important and it would be great if it was illustrated somewhere - adding cross-sections to the maps in Fig 1 and 2 would really help this, I think. Potentially one could also add stereonets showing key orientations.

Line 475 and 481: Steep dips are illustrated in the schematic block diagram as well as the uncertainty analysis.

Line 491: I am not sure how you have ‘circulation from depth’ - again, what is the source and path of fluids?

Line 491: I added a paragraph to the discussion to address this issue, but here I removed this because it seemed wordy and perhaps redundant.

Lines 500-505: Well yes, a dilatant path is no help unless you have a fluid that wants to go somewhere, so you could more simply discuss this point about lack of correlation with that open faults need to be connected to a fluid source AND there needs to be a hydraulic head driving flow of that fluid along the available fluid pathway.

Line 500-505: Good point, I added this to the discussion in this paragraph.

Figure 9: This is a nice figure, but a couple of points: (1) if the small black arrows are stress directions, then it seems to imply tensile stresses as the arrows point away from each other (i.e. negative stress magnitudes) in some hinge areas - if this is meant then describe these stresses as such (and discuss the clear change

in effective stress magnitude from the assumed regional values). If the stresses are meant to be compressional (as assumed) then the arrows should be consistently pointing towards each other, but you could vary length to show differences in degree of compression. (2) Except for a small area of local fluid circulation at depth below the synform, fluid flow seems to be vertical, not circulating. It also seems to be originating from the base of the Great Valley Group across the system, is this intentional? (3) One point that arises from this figure is depth-variable strain in the folds - below the antiform, this would involve compaction at depth and dilation/extensional strain at shallower depth - this would indeed drive upward fluid flow. On the other hand, under the synform, the situation is opposite, and would reverse the hydraulic head and likely drive downward flow. This is sort of between the lines already, but describing in terms of drivers of flow (and clearly separate stress and strain) may make it clearer.

Figure 9: 1) The black arrows are meant to represent stress and I have changed the text to match adding "tensile stress" where appropriate. 2) Arrows are shown only for warm water I am assuming cool meteoric water follows a different path, I added a paragraph describing this so I think it should be clear now. 3) I added this more explicitly in the discussion and then followed up with descriptions of the antiform-synform pair.

Line 541-543: Could specify here that these are 'potential' fluid pathways, that allow fluid flow if they are connected to a fluid source and a hydraulic head is present to drive flow from this fluid source and along the open pathway.

Line 541-543: I added potential fluid pathways.

2nd Round of Revisions

Decision Letter

Dear Authors,

Thank you for resubmitting your work to Tektonika, and for your patience regarding the review processed. We have now received two detailed reviews of your new submission. Both reviewers agree that the topic is suitable for publication and the manuscript is much improved, but one reviewer has raised some remaining issues to be addressed. As such, we would like to invite you to resubmit a revised version of your manuscript, taking into account the various points brought forward (see details below). We would like to ask you to provide a detailed answer to all review remarks along with your revised manuscript (and a version tracing the changes to the text). We are looking forward to receiving your new submission!

With best regards,

Frank Zwaan (associate editor Tektonika)

Craig Magee (executive editor Tektonika)

Response to reviewers:

Responses to Reviewer 1's comments are included below in blue text. In my responses I refer to line, section, and figure numbers from the new draft of the manuscript diff (track changes) file: "diff_main_manuscript_v2.pdf".

Reviewer 1 - Comments

I have read the new version of the manuscript. It reads much clearer, and the authors have largely responded well to questions and suggestions I had in the first version. There are a few things that I think could still be clarified further, I list these below referring to line numbers in the _TrackChanges_LatexDiff.pdf file.

Figures 2 and 3: I see the point about 3D geology being represented in the block diagram at the end, but that doesn't help the reader visualise the area from the start. So, I still highly recommend a cross-section to accompany either Figure 2 or Figure 3 - I don't think we should need to visit the referenced literature for this basic visual.

Figures 2 and 3: Regarding the cross section, perhaps it is better to include one, and not to force the reader to go searching for the cross section in some other paper. I have included a cross section in Figure 4; it is cited in the Background and Discussion sections.

Section 3.2: In this 2D version of dilation tendency, you seem to simply consider dilation in a horizontal plane, assuming faults are vertical and sigma_2 is vertical. I'd say that up front, and modify Eq 1 to be in terms of SHmax and Shmin. Similarly, on line 273, for ease of following, it would be good to specify, explicitly and in this location (instead of having to hunt around for it) which of these stress magnitudes are SHmax, Shmin, and the vertical stress. Actually, does the vertical stress even go into this 2D analysis anywhere?

Section 3.2: I have explicitly mentioned the 2d nature of the study in the methods section (Lines 237 to 240) and added to lines 242 and 244 what SHmax and Shmin are. I have also added the rational for doing a 2d study on Lines 233 to 234, and mentioned we do a sensitivity analysis (Section 3.2.1). In section 3.2.1, in the paragraph starting on line 278 I also added mention of the new fault observations I present.

Line 285 - at least in my version, something has gone wring with the latexdiff here.

Line 285: I fixed this error, I had used the wrong latex symbol.

Lines 288-291: So is this a 3D model now? Looks like it as sigma_n will now depend on the 3D stress field. In that case, it would be good to explicitly point this out, and also, how is sigma_2 as a variation of depth dealt with? Assumed to be a constant value of 20 MPa as stated in line 274? Or is some vertical gradient in vertical stress assumed? If so, what happens to the principal stresses, as they can't keep the constant values inferred in the 2D model.

Lines 288-291: It is a 2d model but a 3d uncertainty analysis which I now explicitly state on Lines 239 to 240. The uncertainty analysis is inherently 3d because it takes plunging sigma 1 orientation into account as well as changes in fault dip (Section 3.2.1). In this way, our initial dilation tendency analysis is a single snapshot in 2d given the unknown dip of most faults. I explicitly state this on Lines 233-234.

In dealing with Sigma 2: I have removed mention that sigma 2 is 20MPa in the "*Structure and Dilation Tendency*" section and moved it to a new "*Three-dimensional Sensitivity Analysis*" (section 3.2.1). I was clearer about why sigma 2 is not used in the dilation tendency analysis on Lines 238-240. I also clarified the usage of the full 3D stress tensor in the sensitivity analysis on Lines 262-264 and the paragraph starting on Line 265. In the first part of the uncertainty analysis sigma 2 is taken as 20 MPa (see Line 271) and in the second part of the uncertainty analysis it is varied with depth (see Lines 273-284) based on faulting regime.

Line 419: I can see how a depth-variable differential stress will matter, but in the methods (see comment above) you only outline assumptions of constant principal stresses - so how is this depth variation calculated (sorry if I missed it)?

Line 419: I have clarified this in the methods section 3.2.1 in the paragraph starting on Line 272 by explicitly saying that I use a depth dependent variation of the 3D stress tensor based on a faulting regime.

Line 510: It's not quite ideal, and there are a few assumptions made here. The statement in line 511 is better. I'd remove the 'ideal orientation' statement and keep the point about a dilation tendency of 0.9. More precise and less repetitive.

Line 510: You're right, I've changed this to "*...northeast-striking faults are comparatively well oriented for dilation under this stress field...*" and combined it with the following sentence. You can see changes on Lines 520 -522.

Discussion and conclusions: This is now much clearer - one point is that the effect of folds is concluded to be important, but is actually not explicitly modelled anywhere here. There is an inference of local tensile stress, but what is the orientation of sigma_3 (if it's related to extension in the hinge, it could be fold hinge perpendicular, quite different from the Shmin inferred regionally), and how would it affect dilatancy? Presumably in this case, you might even have a condition for dilatancy along NW-SE striking faults. There are some comments in Lines 697-702, but otherwise the discussion, after 5.1.1., focuses a lot on faults and the uncertainty of the analysis using regional stress, to then suddenly conclude that folds have a key role. I don't know if more analysis is really needed, but it may be worth considering adding some more detail in the text about the stress field in the fold hinges, what that stress field does to dilatancy in 3D (elaborating on Fig. 8), and just generally considering these effects throughout the discussion.

Discussion and Conclusions:

I think this is a good point, so I have added a new section 5.1.2 "*Stress Rotation at the Fold Hinge*" to discuss these issues. We support this discussion by including a new figure (Figure 6) which includes field data and photos of a mapped fault near the Wilbur Springs antiform. Section 5.1.2 includes a discussion about what this new geologic evidence means for the history of fluid flow along faults in the Wilbur Springs area and how it might deviate from the 2d Td calculations. I do not explicitly model the rotation of the stress field in the fold hinge in the Td calculation, I mention this on Lines 493-494, and the following lines.

Section 5.1.2 discusses many of the variables that are not included in our Td calculations including local rotations of the stress field, rotating faults in the fold limbs, formation of new fractures, and deformation textures and fluid precipitation on a fault in the area (Fig. 6). This section also considers the time component of a rotating stress field and changing Td while new fractures/faults are forming.

Section 5.1.3 "*Fault Orientations and Intersections"* is also edited to reflect some of the comments about stress rotation in the fold hinge that the reviewer

brings up. Including the issue of NE alignment of hot springs despite possible stress field rotations, see Lines 534 to 540.

3rd Round of Revisions

Decision Letter

Dear Authors,

Thank you for resubmitting your work to Tektonika, and for your patience regarding the review processed. We have received some final feedback on the last modifications and had a look at the manuscript as well. Overall, the manuscript is in very good shape and can soon be published, with only some final very minor modifications. Please have a look at these (see reviewer comments below, as well as some minor comments on figures and figure captions in the annotated PDF) and send us the final version of your manuscript at your convenience. For proper documentation, we would like to ask you to provide a detailed answer to all review remarks along with your revised manuscript (and a version tracing the changes to the text). This will all be included in the public review report upon publication. We are looking forward to receiving the final version of your manuscript!

With best regards, Frank Zwaan (associate editor Tektonika)

Craig Magee (executive editor Tektonika)

Reviewer Comments

I have read the new version, and I am happy with my previous points being addressed. I have a couple of minor points below, for a little more clarification of the text on the three dimensional analysis (sorry!), and a suggestion to merge Figures 3 and 4.

Thanks for adding a cross-section, I think this really helps picturing the geology of the area. I think it would be possible to merge figures 3 and 4, making the cross-section a panel rather than its own separate figure.

I've combined Figures 3 and 4, which makes sense for visual comparison.

This would avoid some flicking back and forth to compare map and section, and will also reduce the number of figures. The map legend would then also apply to both the map and section (with some minor edits) and you don't have to to explain the legend again in the caption (as done in the cross-section at the moment).

L 236: Delete 'that'

Deleted

L 277: What density was assumed for the lithostatic loading?

added

L 278: I can see how vertical stress is calculated and how of the principal stresses is defined as equal to the vertical stress. However, how are the horizontal stresses defined? Is there a fixed differential stress at some depth that is used as a starting point?

horizontal stress was calculated at depth using vertical stress and pore-fluid pressure using a friction coefficient of 0.6 and assumptions of each stress regime. I added this to the text. The uncertainty analysis assumes differential stress approaches zero near the surface.

L 281 ‘keep it near the middle of the principal stress range’: is this a complicated way of saying ‘keeping it approximately equal to the mean stress’ or does it mean something else?

No it is a complicated way of saying "approximately equal to the mean stress" I changed this. Also, i added a paragraph explaining that the assumed stress-depth profiles represent only one plausible set of conditions and that variations would cause changes to Td values.

Acceptance letter

Excellent! I had a look and it all seems ok. You should have received a notification that your submission is now accepted for publication.

Everything will now be forwarded to the copy-editors, who I believe will be in contact with you soon.

Thanks again for submitting to Tektonika and congratulations on a very nice paper, I am looking forward to seeing the final version online!

Cheers,

Frank